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The American Political Science Review

Articles and Essays by Edward G. Carmines, Wayne A. Cornelius, Thomas C. Nowak and Kay A. Snyder, Kenneth Jowitt, David W. Moore, Robert A. Bernstein and William W. Anthony, Bruce Bueno de Mesquita, John Wanat, Paul Allen Beck, Gordon S. Black, George H. Quester, Samuel H. Beer

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POLITICAL SCIENCE ★★★★★★★★★★★★★★★

Kenneth Prewitt and Sidney Verba

AN INTRODUCTION TO AMERICAN GOVERNMENT

This introduction to the foundations, spirit, and operation of the American political system includes innovative discussions of how the U.S. economic system has shaped our democracy, how political capitalism effects the class structure, the degree to which the principle of democracy has its roots in the hearts and minds of the American people, and how the American citizen participates in political life. Policy is discussed in depth with applications made to taxes, medical care, desegregation, and Vietnam. There is a postscript on Watergate. 630 pages; $9.95. 1974. *Instructor's Manual.*

Sidney Verba and Norman H. Nie

PARTICIPATION IN AMERICA: Political Democracy and Social Equality

☆ ***NOW IN PAPER*** ☆ *Received the 1973 Gladys M. Kammerer Award of the American Political Science Association for the best publication of 1972 in the field of U.S. National Policy as well as honorable mention in the 1973 Woodrow Wilson Foundation award competition.* The most extensive study of the subject available, this book, based on a large-scale empirical study, provides not only new information and a new perspective on the ways in which citizens take part in political life, but also a new theoretical understanding of the role of participation in a democracy. 428 pages; $5.95; paper. 1972.

Monte Palmer, Larry Stern, and Charles Gaile

THE INTERDISCIPLINARY STUDY OF POLITICS

This text deals with the problems of clarity, precision, operationalization, and measurement inherent in the use by political scientists of concepts, theories, and conceptual frameworks developed by members of other social science disciplines. The intent is to provide students with a basic conceptual understanding which will facilitate their acceptance of concepts and conceptual frameworks. Tentative: 177 pages; $4.95; paper. August 1974.

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The American Political Science Review

Vol. LXVIII September 1974 No. 3

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CONTENTS

951 Political Issues and Trust in Government: 1964–1970 Arthur H. Miller

973 Comment Jack Citrin

989 Rejoinder Arthur H. Miller

1002 Theory and Voting Research: Some Reflections on Converse's "Change in the American Electorate" Walter Dean Burnham

1024 Comment Philip E. Converse

1028 Comment Jerrold G. Rusk

1050 Rejoinder Walter Dean Burnham

1058 Social Change and Political Mobilization in the United States: 1870–1960 Jesse F. Marquette

1075 Institutionalization, the Ombudsman, and Bureaucracy Larry B. Hill

1086 Hegel's Critique of Liberal Theories of Rights Peter G. Stillman

1093 Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis Lawrence C. Dodd

1118 The Mediating Influence of State Legislatures on the Linkage Between Interparty Competition and Welfare Policies Edward G. Carmines

1125 Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities Wayne A. Cornelius

1147 Clientelist Politics in the Philippines: Integration or Instability? Thomas C. Nowak and Kay A. Snyder

1171 An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems Kenneth Jowitt

1192 Foreign Policy and Empirical Democratic Theory David W. Moore

1198 The ABM Issue in the Senate, 1968–1970: The Importance of Ideology Robert A. Bernstein and William W. Anthony

1207 Need for Achievement and Competitiveness as Determinants of Political Party Success in Elections and Coalitions Bruce Bueno de Mesquita

1221 Bases of Budgetary Incrementalism John Wanat

1229 Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior Paul Allen Beck

1245 Conflict in the Community: A Theory of the Effects of Community Size Gordon S. Black

1262 Communications

1281 Editorial Comment

1285 Book Reviews and Essays

1285 Chemical and Biological Warfare George H. Quester

1292 Tradition and Nationality: A Classic Revisited Samuel Beer

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ARTICLES

951 **Political Issues and Trust in Government: 1964–1970.** National survey data demonstrate that support of the federal government decreased substantially between 1964 and 1970. Policy preference, a lack of perceived difference between the parties, and policy dissatisfaction were hypothesized as correlates of trust and alternative explanations of this decrease. Analysis revealed that the increased distrust in government, or cynicism, was associated with reactions to the issues of racial integration and U.S. involvement in the Vietnam war. A curvilinear relationship was found between policy preference on these and other contemporary social issues and political cynicism. The minority favoring centrist policies was more likely to trust the government than the large proportion who preferred noncentrist policy alternatives. This complex relationship between trust and policy preference is explained by dissatisfaction with the policies of *both* political parties. The dissatisfied noncentrists formed highly polarized and distinct types: "cynics of the left," who preferred policies providing social change, and "cynics of the right," who favored policies of social control.

By ARTHUR H. MILLER, Study Director, The University of Michigan, Center for Political Studies.

Comment. By JACK CITRIN, Assistant Professor of Political Science, University of California, Berkeley.

Rejoinder. By ARTHUR H. MILLER.

1002 **Theory and Voting Research: Some Reflections on Converse's "Change in the American Electorate."** Philip E. Converse has challenged the findings of a 1965 article, "The Changing Shape of the American Political Universe," and other work by Burnham. Converse asserts that most of the very high voter participation which occurred before 1900 can be explained by a combination of electoral corruption, the absence of personal-registration requirements and other "undramatic" factors, and thus that the anomalies which Burnham reported are largely spurious. Issues of major importance for social-science explanation are joined. The present article attempts to demonstrate that intervening structural variables cannot come close to explaining all the post-1900 decline in voting participation and that the genuine existence of universal nineteenth-century rural corruption has yet to be demonstrated. These efforts to explain away anomaly are held to be unpersuasive. The weight of evidence supports the objective reality of the phenomena originally reported. This in turn means that more adequate conceptualizations are needed to integrate empirical findings than those which have hitherto dominated the voting-behavior research community.

By WALTER DEAN BURNHAM, Professor of Political Science, Massachusetts Institute of Technology.

Comment. By PHILIP E. CONVERSE, Professor of Political Science, University of Michigan.

Comment. By JERROLD G. RUSK, Associate Professor of Government, University of Arizona.

Rejoinder. By WALTER DEAN BURNHAM.

1058 **Social Change and Political Mobilization in the United States: 1870–1960.** The central problem of the general search for explanations of political change has been the lack of adequate explanations of the relationship between social change and political change. This research proposes and tests a six-variable causal model of the process of social change and political mobilization in the United States during the period 1870 to 1960. The variables used are based on previous theoretical efforts which have indicated that the process of social and political change is a syndrome. From these previous efforts a new model is synthesized. The model is found to operate as proposed during the period 1870 to 1910, and a simplified version in four variables is identified for the period 1920 to 1960.

One of the central questions explored by this research is the degree to which the pattern of social change alters as the process of change proceeds through time. The transition in the United States is explained by reference to the threshold effect of two social infrastructures—urbanization and government activity in education.

In light of the identification of the model, an attempt is then made at revising certain aspects of modernization theory.

By JESSE F. MARQUETTE, Assistant Professor of Political Science, The University of Akron.

1075 **Institutionalization, the Ombudsman, and Bureaucracy.** This article proposes an authority based conception of the phenomenon of institutionalization. Whereas most social science writing on the subject focuses either upon the organization's internal structural integration (such criteria as well-boundedness, universalism, complexity, and coherence often are employed) or upon its ability to cope with environmental challenges (the concepts of adaptability and autonomy have been suggested), I propose that an organization's *offensive capabilities* vis à vis environmental actors be viewed as a measurement of its institutionalization.

Principally based upon fieldwork in New Zealand bolstered by additional research in Scandinavia, Britain, and Hawaii, this study focuses upon the institutionalization of the ombudsman—an increasingly

popular bureaucratic control mechanism. A sociometric analysis of ombudsman-bureaucratic interaction is undertaken, and four questions are investigated: How extensive is the interaction? How consequential is the threat posed by complaints? What demands does the ombudsman make? How cooperative is the agency in responding? The investigation provides answers which are indicative of the ombudsman's successful institutionalization. That is, the office performs its mission and has established itself with the environmental actors as an authority figure.

By LARRY B. HILL, Associate Professor of Political Science, University of Oklahoma.

1086 **Hegel's Critique of Liberal Theories of Rights.** In "Abstract Right," the first part of the *Philosophy of Right*, Hegel criticizes the usual content and formulations of liberal theories of rights. In terms of content, Hegel argues that the subject of rights is only a narrow abstraction from the full human being; that he has limited self-determination and limited political freedom; and that, when he acts on his rights, he produces terror and destruction. In terms of formulations, Hegel argues that the pervasiveness of contract relations is inaccurate and undesirable; that the state cannot be derived from the natural man's alienating his right to punish; and that it is inaccurate to conceptualize civil society as only limiting natural man's freedoms. By transforming natural to abstract rights, Hegel retains much of the substance of rights, while concurrently preparing for the later sections of his text which try to overcome the inadequacies of a political theory based only on rights.

By PETER G. STILLMAN, Assistant Professor in Political Science, Vassar College.

1093 **Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis.** This study focuses on A. Lawrence Lowell's classic thesis that a parliamentary democracy must possess a majority party system if durable cabinets are to exist. The argument of this study is that majority party government is not essential to cabinet durability. Rather, in line with the British analyst W. L. Middleton as well as more contemporary game-theoreticians, the critical factor is held to be the coalitional status of the cabinet: (1) cabinets of minimum winning status should be durable; as cabinets depart from minimum winning status, cabinet durability decreases; (2) the coalitional status of the cabinet that forms is partially a product of party system fractionalization, instability, and polarization. Hypotheses derived from the theory are tested with data drawn from 17 Western parliamentary democracies, from 1918 to 1940 and from 1945 to 1970. The findings generally support the theory. A key to durable government is the minimum winning status of the cabinet. Minimum winning cabinets are possible in multiparty and majority party systems.

By LAWRENCE C. DODD, Assistant Professor of Government, University of Texas, Austin.

1118 **The Mediating Influence of State Legislatures on the Linkage Between Interparty Competition and Welfare Policies.** The purpose of this paper is to reformulate the linkage between interparty competition and welfare policies in the American states. Specifically, it is hypothesized that this linkage should be substantially greater among states with strong, effective legislatures than among states with weak, ineffective legislative systems. When legislative effectiveness was defined in terms of professionalism and welfare effort was assessed by seven specific measures, it was found that the link between party competition and welfare expenditures was indeed stronger among states with effective, as opposed to ineffective, legislative systems. Further, when controls were introduced for several socioeconomic factors, the linkage continued to be stronger among states with professional legislatures. It is concluded, consequently, that the influence of party competition on state welfare policies is mediated by the differential effectiveness of state legislatures.

By EDWARD G. CARMINES, Ph.D. Candidate in Political Science, State University of New York at Buffalo.

1125 **Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities.** This paper investigates the proposition that rapid urbanization produces significant changes in the kinds, volume, and intensity of demand making aimed at local and national governments, leading to political system "overload" and pressure for major shifts in resource allocation. Drawing upon data gathered among low-income migrants to Mexico City and other Latin American cities, the paper analyzes the process through which objective needs are converted into demands upon government. The findings indicate that there are often major lags in the process of demand creation among cityward migrants, and that many kinds of felt needs are viewed by migrants as needs to be satisfied primarily through individual rather than governmental action.

Data are presented on the incidence of demand making among the migrant population and the substantive nature of the demands they make upon government. Strategies used in attempting to influence government decisions are described, and the attitudes and perceptions underlying the migrant's preference among alternative strategies are analyzed. The long-term propensity of migrants and their offspring to engage in demand making with regard to broad social and economic issues rather than individual or community-related needs is assessed.

By WAYNE A. CORNELIUS, Assistant Professor of Political Science, Massachusetts Institute of Technology.

1147 **Clientelist Politics in the Philippines: Integration or Instability?** Philippine data are presented which indicate that a contradiction exists between changes induced partly through capital accumulation by the indigenous elite and foreign investment, and both increased political factionalism and declining voting participation. While national elites become more powerful through capital accumulation, local political machines confront structural changes weakening their power. More specialized patron-client structures diminish local elites' ability both to deliver votes to national patrons and to stimulate electoral participation. Growth of the middle class in a stagnant economy increases competition for lucrative local political office Factions proliferate and with the increased concentration of private income, become more dependent on national patronage resources. Unable to meet rising patronage demands, the government resorts to extensive deficit spending which stimulates inflation and further undermines economic growth. The national elite's economic activities thus undermines its authority base as the state becomes increasingly less able to provide security to individuals dislocated by changes generating profit for the elite.

By THOMAS NOWAK, Assistant Professor of Political Sociology, Justin Morrill College, Michigan State University, and KAY SNYDER, Assistant Professor of Sociology, Michigan State University.

1171 **An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems.** This paper is addressed to three tasks and the analysis operates at three levels. First, there is an attempt to specify an analytic approach to Marxist-Leninist sociopolitical systems that integrates regime and sociocultural units. This approach rests on a structural conception of political culture, a conception that stresses the informal adaptive quality of political culture, and that includes behavioral as well as attitudinal patterns. The second task consists of analyzing the paradoxical character of development in Soviet-type systems; development that simultaneously reinforces and undermines traditional-peasant political cultures at the community, regime, and elite levels. Finally, this pattern of development is examined in the context of a single Soviet-type regime and society, the Romanian.

By KENNETH JOWITT, Assistant Professor of Political Science, University of California, Berkeley.

1192 **Foreign Policy and Empirical Democratic Theory.** This paper compares the research on the determinants of domestic policy outcomes, primarily in American state governments, with similar research in the area of foreign policy. Using seven foreign policy measures, it then tests a hypothesis based on Cutright's cross-national analysis of social insurance programs, that political representativeness is more important in accounting for policy outcomes among developed than among undeveloped nations. Finally, it suggests what implications the reported findings may have on empirical democratic theory.

By DAVID W. MOORE, Assistant Professor of Political Science, University of New Hampshire.

1198 **The ABM Issue in the Senate, 1968–1970: The Importance of Ideology.** Three hypotheses suggesting why senators might adopt or change positions on such an issue as the ABM are compared. The empirical analysis clearly substantiates the contention that position reflects ideology, not party commitment or potential state economic benefits. Furthermore, the influence of ideology is seen to have grown more apparent each year the issue was contested in the Senate. Virtually all the senators who changed position between 1968 and 1970 had initial positions that did not accord with their ideology, and they moved so as to bring them in accord. Virtually all those senators whose initial position was in accord with their ideology maintained that position.

by ROBERT A. BERNSTEIN, Assistant Professor of Political Science and WILLIAM W. ANTHONY, Lecturer in Political Science, Texas A&M University.

1207 **Need for Achievement and Competitiveness as Determinants of Political Party Success in Elections and Coalitions.** Need for achievement and strategic predispositions among political party elites are hypothesized to have an important impact on the success parties enjoy in elections and in coalitions. More specifically, this study develops and tests a model which suggests that parties whose leaders have high need for achievement and are predisposed to pursue a mixed competitive/cooperative strategy are more likely to do well in elections and in coalitions than are parties whose leaders are low in need for achievement and oriented to either cooperative or competitive strategies.

When the Indian political party system between 1967 and 1971 is used as the data base, the success or failure of political parties is correctly predicted by need for achievement for thirteen out of fourteen variables. By means of multiple regression analysis, as much as seventy-two per cent of the variance in the electoral success of Indian parties is explained by the model.

By BRUCE BUENO DE MESQUITA, Assistant Professor of Political Science, University of Rochester.

1221 **Bases of Budgetary Incrementalism.** This essay analyses the explanatory power of Davis, Dempster, and Wildavsky's theory of budgetary incrementalism. By means of sensitivity testing, it demonstrates that inferences to "gaming" or strategic explanations of budgetary incrementalism are not warranted on the basis of correlational analysis.

To explain budgetary incrementalism more satisfactorily, recourse is made to concepts and variables explicit in the vocabulary of the budget process participants. When mandatory requests are distinguished from programmatic requests, the differential treatment of the two by Congress is observed to allow good explanation of budgetary relations. In particular, the inexorable but small mandatory request, which is almost automatically granted, is adequate by itself to explain why requests always increase and why one year's appropriation surpasses the previous one.

By JOHN WANAT, Assistant Professor of Political Science, University of Kentucky.

1229 **Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior.** While many scholars have recognized that decentralization encourages American party organizations to tailor activities to the local environment, few have studied systematically the relationships between that environment and party behavior. This study examines the impact of certain political and demographic county characteristics on the activities of a national sample of county party organizations in 1964. Three dimensions of party behavior—organization, mobilization, and persuasion—are utilized as dependent variables. The relationships between the environment and these dimensions of party behavior in the North support a revised "machine theory" of environment and party: organizational effort does not vary with environmental conditions, while mobilization and persuasion activities are opposites in their relationships with the concentration of parochially-oriented voters. Additionally, the division of partisan strength influences party activity: parties perform their "natural" activities well where they have strong support and the other party's "natural" activities well under competitive conditions. Few significant relationships are found in the South, but their similarity in direction to those in the North suggests that the normal relationships may have been attenuated by circumstances unique to that region, particularly one-partyism and decades of "whites only" politics.

By PAUL ALLEN BECK, Assistant Professor of Political Science, University of Pittsburgh.

1245 **Conflict in the Community: A Theory of the Effects of Community Size.** Two theories of community conflict are examined in this paper with data from elections in 89 cities in the San Francisco Bay area. One theory is developed from the work on group conflict by Georg Simmel and Lewis Coser while the other is a rational choice theory based on assumptions about the costs and risks of conflict in different size cities. Both theories suggest that conflict, while more frequent in larger communities, is likely to become most severe in smaller communities. Both theories are confirmed by the pattern of findings in the analysis, but the rational choice theory proves to have the greater generality, i.e., that it can explain more of the findings in the paper.

By GORDON S. BLACK, Associate Professor of Political Science, University of Rochester.

1262 COMMUNICATIONS

From Arthur L. Stinchcombe, Thomas R. Dye, Douglas D. Rose, Abraham H. Miller and Stephen E. Bennett, Edward C. Banfield and James Q. Wilson, Christopher H. Achen, Richard S. Katz, Ken Allen, Peter K. Eisinger, Roy E. Licklider, Anne H. Cahn, Richard S. Beth, Peter J. Taylor, G. William Domhoff, Earl Latham

1281 EDITORIAL COMMENT

1285 BOOK REVIEWS AND ESSAYS

1285 **Chemical and Biological Warfare.** Frederic J. Brown, *Chemical Warefare: A Study in Restraints;* Carnegie Endowment for International Peace, *The Control of Chemical and Biological Weapons;* John Cookson and Judith Nottingham, *A Survey of Chemical and Biological Warfare;* J. B. Neilands, G. H. Orians, E. W. Pfeiffer, Alje Vennema, and Arthur H. Westing, *Harvest of Death;* Steven Rose, ed., *CBW;* Stockholm International Peace Research Institute, *The Problem of Chemical and Biological Warfare,* 6 volumes.

By GEORGE H. QUESTER, Professor of Government, Cornell University.

1292 **Tradition and Nationality: A Classic Revisited.** Walter Bagehot, *The English Constitution.*

By SAMUEL BEER, Professor of Government, Harvard University.

1296 Political Theory, History of Political Thought and Methodology

Jerald G. Bachman, Robert L. Kahn, Martha T. Mednick, Terrence N. Davidson, and Lloyd D. Johnst *Youth in Transition, Vol. I: Blueprint for a Longitudinal Study of Adolescent Boys;* Jerald G. Bachm *Youth in Transition, Vol. II: The Impact of Family Background and Intelligence on Tenth-Grade Bo* Jerald G. Bachman and Elizabeth Van Duinen, *Youth Look at National Problems: A Special Rep from the Youth in Transition Project;* and Jerome Johnston and Jerald G. Bachman, *Youth in Transiti Vol. V: Young Men and Military Service.* Jarol B. Manheim, p. 1296

Marshall Berman, *The Politics of Authenticity: Radical Individualism and the Emergence of Mod Society.* Allan Bloom, p. 1297

Hugh Brogan, *Tocqueville.* J. P. Mayer. p. 1299

Stanley Chodorow, *Christian Political Theory and Church Politics in the Mid-Twelfth Century: The E clesiology of Gratian's Decretum.* Ernest L. Fortin. p. 1300

Robert A. Dahl and Edward R. Tufte, *Size and Democracy.* Heinz Eulau. p. 1301

Gottfried Dietze, *Two Concepts of the Rule of Law.* Stephen L. Wasby. p. 1303

Andrew Dunsire, *Administration: The Word and the Science.* Robert T. Golembiewski. p. 1303

Giuseppe Fiori, *Antonio Gramsci: Life of a Revolutionary.* Dante Germino. p. 1305

Richard E. Flathman, *Political Obligation.* Leslie J. Macfarlane, p. 1305

Julian H. Franklin, *Jean Bodin and the Rise of Absolutist Theory.* Randal H. Ihara, p. 1306

Max Horkheimer and Theodor W. Adorno, *Dialectic of Enlightenment.* Translated by John Cumming Hiram Caton, p. 1307

Dick Howard, *The Development of the Marxian Dialectic.* Z. A. Jordan, p. 1308

Raphael G. Kasper, ed., *Technology Assessment: Understanding the Social Consequences of Technological Applications.* Victor Ferkiss, p. 1310

Nikki R. Keddie, *Sayyid Jamal ad-Din "al-Afghani": A Political Biography.* Marvin Zonis, p. 1310

Daniel C. Kramer, *Participatory Democracy: Developing Ideals of the Political Left.* Carole Pateman, p. 1311

Robert E. Meagher, ed., *Toothing Stones: Rethinking the Political.* D. M. White, p. 1313

Saad Z. Nagi and Ronald G. Corwin, eds., *The Social Contexts of Research.* Arnold Thackray, p. 1314

Vincent Ostrom, *The Political Theory of a Compound Republic: A Reconstruction of the Logical Foundations of American Democracy as Presented in The Federalist.* Robert S. Ross, p. 1315

Ralph Ross, *Obligation: A Social Theory.* Melvin M. Tumin, p. 1316

Louis Schneider and Charles Bonjean, eds., *The Idea of Culture in the Social Sciences.* Harry C. Triandis, p. 1318

Alfred Schutz and Thomas Luckmann, *The Structures of the Life-World.* Translated by Richard M. Zaner and H. Tristram Engelhardt, Jr. Hwa Yol Jung, p. 1319

Ellen Meiksins Wood, *Mind and Politics.* Vincent E. Starzinger, p. 1321

1322 American Government and Politics

Robert T. Bower, *Television and the Public.* Paul Conway, p. 1322

David W. Brady, *Congressional Voting in a Partisan Era: A Study of the McKinley Houses and a Comparison to the Modern House of Representatives.* Charles M. Tidmarch. p. 1323

Jonathan D. Casper, *American Criminal Justice: The Defendant's Perspective;* Jack D. Douglas, ed., *Crime and Justice in American Society;* and John T. Elliff, *Crime, Dissent, and the Attorney General: The Justice Department in the 1960's.* Ruth G. Weintraub, p. 1324

Robert D. Cuff, *The War Industries Board: Business-Government Relations During World War I.* Robert A. Kagan, p. 1325

Timothy L. Fitzharris, *The Desirability of a Correctional Ombudsman.* Karl A. Friedmann, p. 1326

William E. Fruhan, *The Fight for Competitive Advantage: A Study of the United States Domestic Air Trunk Carriers.* Robert L. Thornton, p. 1327

Perry Goldman and James S. Young, eds., *U.S. Congressional Directories, 1798–1840.* Nelson W. Polsby, p. 1328

Bennett Harrison, *Education, Training, and the Urban Ghetto.* JeDon A. Emenhiser, p. 1328

Bruce D. Johnson, *Marihuana Users and Drug Subcultures;* and National Commission on Marihuana and Drug Abuse, *Drug Use in America: Problem in Perspective.* Joseph L. Zentner, p. 1329

Michael Kammen, *People of Paradox: An Inquiry Concerning the Origins of American Civilization.* Jack P. Greene, p. 1331

Stephen G. Kurtz and James H. Hutson, eds., *Essays on the American Revolution.* Richard Crosby, p. 1332

Norman Markowitz, *The Rise and Fall of the People's Century: Henry Agard Wallace and American Liberalism, 1941–1948.* Edward L. Shapsmeier, p. 1333

George E. Mowry, *Another Look at the Twentieth-Century South.* Charles S. Bullock, III, p. 1334

Rolla Edward Park, ed., *The Role of Analysis in Regulatory Decisionmaking: The Case of Cable Television.* Nathan Shoehalter, p. 1335

Jeffrey L. Pressman and Aaron Wildavsky, *Implementation: How Great Expectations in Washington Are Dashed in Oakland; Or, Why It's Amazing that Federal Programs Work at All, This Being a Saga of the Economic Development Administration as Told by Two Sympathetic Observers Who Seek to Build Morals on a Foundation of Ruined Hopes.* Martha Derthick, p. 1336

Walter A. Rosenbaum, *The Politics of Environmental Concern*. Harris S. Cohen, p. 1337
Kirkpatrick Sale, *SDS*. Edward J. Bacciocco, Jr., p. 1338
Paul A. Theis and Edmund L. Henshaw, Jr., eds., *Who's Who in American Politics: 1973–1974*. Fourth edition. Charles O. Jones, p. 1339
Melvin I. Urofsky and David W. Levy, eds., *Letters of Louis D. Brandeis: Volume I, 1870–1907, Urban Reformer; Volume II, 1907–1912, People's Attorney; Volume III, 1913–1915, Progressive and Zionist*. Robert G. Seddig, p. 1340
Hanes Walton, Jr., *Black Political Parties: An Historical and Political Analysis*. William R. Keech, p. 1341
Arnold R. Weber, *In Pursuit of Price Stability: The Wage-Price Freeze of 1971*. William A. Niskanen, p. 1342
Edward Wenk, Jr., *The Politics of the Ocean*. Donald E. Milsten, p. 1343
William M. Wiecek, *The Guarantee Clause of the U.S. Constitution*. Robert J. Morgan, p. 1344

1345 Comparative Government and Politics

Richard Antoun and Iliya Harik, eds. *Rural Politics and Social Change in the Middle East*. Douglas E. Ashford, p. 1345
David E. Apter and James Joll, eds., *Anarchism Today*. Stanley Rothman, p. 1346
John Badgley, *Asian Development: Problems and Prognosis*. Khalid B. Sayeed, p. 1347
John M. Baines, *Revolution in Peru: Mariátegui and the Myth*. Harry Kantor, p. 1348
Correlli Barnett, *The Collapse of British Power*. F. S. Northedge, p. 1349
Harvey Cox and David Morgan, *City Politics and the Press: Journalists and the Governing of Merseyside*. Morris Davis, p. 1350
John Dunn, *Modern Revolutions: An Introduction to the Analysis of a Political Phenomenon*. Rex Mortimer, p. 1351
Milton Esman, *Administration and Development in Malaysia: Institution Building and Reform in a Plural Society*. Lucian W. Pye, p. 1352
John Friedmann, *Urbanization, Planning and National Development*. Jorge I. Domínguez, p. 1354
Robert E. Gamer, *The Politics of Urban Development in Singapore*. Lynn T. White, III, p. 1355
Nigel Harris, *Competition and the Corporate Society: British Conservatives, The State and Industry, 1945–1964*, S. C. Ghosh, p. 1357
Nicholas S. Hopkins, *Popular Government in an African Town: Kita, Mali*. William J. Foltz, p. 1358
Derek Hopwood, ed., *The Arabian Peninsula: Society and Politics;* and Elizabeth Monroe, *The Changing Balance of Power in the Persian Gulf: An International Seminar Report*. John G. Merriam, p. 1359
Shashishekhar Jha, *Political Elite in Bihar*. Douglas C. Smyth, p. 1361
Robert Kern, ed., *The Caciques: Oligarchical Politics and the System of Caciquismo in the Luso-Hispanic World*. Douglas A. Chalmers, p. 1362
Rajni Kothari, *Politics in India*. Myron Weiner, p. 1363
David Lane and George Kolankiewicz, eds., *Social Groups in Polish Society*. Szymon Chodak, p. 1364
Peter Mansfield, ed., *The Middle East: A Political and Economic Survey*. 4th Edition. Robert Owen Freedman, p. 1365
D. Bruce Marshall, *The French Colonial Myth and Constitution-Making in the Fourth Republic*. Peter Alexis Gourevitch, p. 1366
S R. Mehrotra, *The Emergence of the Indian National Congress*. Paul Wallace, p. 1367
John Meisel, *Working Papers on Canadian Politics*. Peter Regenstreif, p. 1369
Michael Parrish, *The Soviet Armed Forces, Books in English, 1950–1967*. Leon Goure, p. 1370
Howard Penniman, *Elections in South Vietnam*. Allan E. Goodman, p. 1370
Rutherford M. Poats, *Technology for Developing Nations: New Directions for U.S. Technical Assistance*. Brewster C. Denny, p. 1371
Walter R. Roberts, *Tito, Mihailović and the Allies, 1941–1945*. Alex N. Dragnich, p. 1372
Martin Schnitzer, *East and West Germany: A Comparative Economic Analysis*. Gerald G. Watson, p. 1373
Robert E. Scott, ed., *Latin American Modernization Problems*. Eldon Kenworthy, p. 1373
Peter J. Seybolt, *Revolutionary Education in China: Documents and Commentary*. Anthony W. Ferguson, p. 1375
Ralph Smith, *Vietnam and the West*. Robert L. Youngblood, p. 1375
Frederic Spotts, *The Churches and Politics in Germany*. Guenter Lewy, p. 1376
Arnold Strickon and Sidney M. Greenfield, eds., *Structure and Process in Latin America: Patronage, Clientage, and Power Systems*. Steffen Schmidt, p. 1377
Book Publishing in the USSR: Reports of the Delegations of U.S. Book Publishers Visiting the USSR, October 21–November 4, 1970, August 20–September 17, 1962. Second Edition. Patricia K. Grimsted, p. 1379
Karl-Eugen Wädekin, *The Private Sector in Soviet Agriculture*. 2nd revised edition of *Privatproduzenten in der sowjetischen Landwirtschaft*. Edited by G. F. Karcz. Translated by K. Bush. James R. Millar, p. 1379
Ben Whitaker, ed., *The Fourth World: Victims of Group Oppression*. Richard Sandbrook, p. 1381

W. F. Biddle, *Weapons Technology and Arms Control*. Bernard Brodie, p. 1382
Tarun Chandra Bose, *The Superpowers and the Middle East*. O. M. Smolansky, p. 1382
Henry Brandon, *The Retreat of American Power*. Frank Burd, p. 1383
Gerald E. Bunker, *The Peace Conspiracy: Wang Ching-wei and the China War, 1937–1941*. James E. Sheridan, p. 1384
David P. Calleo and Benjamin M. Rowland, *America and the World Political Economy*. Ole R. Holsti, p. 1385
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Helen D. Cohn, *Soviet Policy Toward Black Africa: The Focus on National Integration*. Vernon McKay, p. 1388
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William R. Duggan, *A Socioeconomic Profile of South Africa*. Thomas Karis, p. 1391
David V. Edwards, *Creating a New World Politics: From Conflict to Co-Operation*. J. L. Richardson, p. 1392
Theodore Geiger, *The Fortunes of the West: The Future of the Atlantic Nations*. Annette Baker Fox, p. 1393
Wesley L. Gould and Michael Barkun, *Social Science Literature: A Bibliography for International Law*. Anthony D'Amato, p. 1394
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Norman Rich, *Hitler's War Aims: Ideology, the Nazi State, and the Course of Expansion*. A. J. P. Taylor, p. 1406
Robert L. Rothstein, *Planning, Prediction, and Policymaking in Foreign Affairs*. Bernard C. Cohen, p. 1407
R. J. Rummel, *The Dimensions of Nations*. Gordon Hilton, p. 1408
Daniel B. Schirmer, *Republic Or Empire: American Resistance to the Philippine War*. Milton Plesur, p. 1409
Robert M. Slusser, *The Berlin Crisis of 1961: Soviet-American Relations and the Struggle for Power in the Kremlin, June-November 1961*. Werner J. Feld, p. 1410
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Political Issues and Trust in Government: 1964–1970*

ARTHUR H. MILLER
The University of Michigan

Recent Trends Toward Political Alienation

A democratic political system cannot survive for long without the support of a majority of its citizens. When such support wanes, underlying discontent is the necessary result, and the potential for revolutionary alteration of the political and social system is enhanced.[1] The existence of a substantial degree of political discontent within a society at any one point in time does not necessarily, however, signify a decaying of the social and political order. On the contrary, in a democracy such discontent may lead to political and social change or may result in the electoral practice of "throwing the rascals out." In either case, the satisfaction of the unfulfilled needs which presumably led to the discontent gives testimony to the flexibility of the political system and the ability of government to manage conflict. It has, in fact, been argued that having a minority of politically apathetic individuals (who are presumably satisfied with the status quo) adds to that flexibility.[2] When dissatisfaction with the existing situation leads, however, to pervasive and enduring distrust of government, this flexibility is greatly curtailed,[3] thereby increasing the potential for radical change. Extensive periods of high political discontent imply that needs among certain segments of the population are not being met; their discontent is thus an indicator of dissatisfaction with the quality of life they are experiencing.

A period of sustained discontent may result from deep-seated social conflict which, for some segment of the population, has been translated into a negative orientation toward the political system because their sense of insufficient political influence implies a futility in bringing about desired social change or control through political efforts; hence, they feel government is generally not to be trusted because it does not function for them. Such feelings of powerlessness and normlessness are very likely to be accompanied by hostility toward political and social leaders, the institutions of government, and the regime as a whole. In such case, "throwing the rascals out" will have little, if any, effect on restoring confidence in government or the political system. While discontent that exists only for a short time and acts as a catalyst for needed change may reflect a functional political system, extended periods of widespread political malaise suggest that the normal means by which conflict is managed in the political system are not fully operative.

A situation of widespread, basic discontent and political alienation exists in the U.S. today. Support for this contention can readily be found in the daily reports of the mass media, in an examination of recent political events—witness the substantial support for George Wallace in both the 1968 election and the 1972 primaries—and in the 1972 political campaign rhetoric. But, more convincingly, it is also found in national survey data.

Survey questions dealing with political efficacy and political trust, two dimensions of alienation,[4] provide evidence substantiating this contention. Converse[5] has recently reported that between 1960 and 1968 there was a steady decline in political efficacy—the feeling that an individual can have an impact upon the political process—despite large overall increases in education. In 1960, for example, nearly 42 per cent of the population disagreed (the efficacious response) with the following statement: "Sometimes politics and government seem so complicated that a person like me can't really understand what's going on." In 1968, the comparable figure was 30 per cent and in 1970, it was only 26 per cent.

A similar and equally dramatic change was found in five survey questions designed to measure

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[1] William A. Gamson, *Power and Discontent* (Homewood, Ill.: Dorsey Press, 1968), chapter 9.

[2] Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954), chapter 14.

[3] Gamson, pp. 42–48.

[4] For a discussion of the dimensions of political alienation, see Ada W. Finifter. "Dimensions of Political Alienation," *American Political Science Review*, 64 (June, 1970), 389–410.

[5] Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change*, ed. Angus Campbell and Philip E. Converse, (New York: Russell Sage Foundation, 1972).

political trust.[6] The percentage distribution of responses to these items is presented in Table 1. These data reveal a strong trend of increasing political cynicism for the general population between 1964 and 1970. The change in the responses to all but one of the items is somewhat more than 20 percentage points in the direction of increased distrust of the government. Even with all five items, the average increase in cynical responses was slightly more than 17 percentage points. The magnitude of this change can be better appreciated in light of the finding that responses to the same five items had showed an average change of only two percentage points in a cynical direction during the previous six-year period from 1958 to 1964. That some segment of American society believes that officials violate legal procedures in dealing with the public or in arriving at policy decisions is not surprising. What is startling, and somewhat alarming, is the rapid degree of change in this basic attitude over a period of only six years.

The Study of Political Cynicism. Political trust can be thought of as a basic evaluative or affective orientation toward the government,[7] and, more specifically for this study, the government in Washington.[8] The dimension of trust runs from high trust to high distrust or political cynicism. Cynicism thus refers to the degree of negative affect toward the government and is a statement of the belief that the government is not functioning and producing outputs in accord with individual expectations.

As noted, cynicism and efficacy have been viewed as components of the more general concept of political alienation,[9] and have been subject to increased empirical study recently. These studies tend to emphasize either background and personality characteristics or, to a lesser extent, reactions to political decisions as the main explanations of political trust and cynicism.[10] The focus of this paper is on the impact that reactions to political issues and public policy have on the formation of political cynicism.

The Data. The data used in this report are based on The University of Michigan Survey Research Center election studies (now conducted by the Center for Political Studies—CPS) of a national cross section of eligible voters for the years 1964, 1966, 1968, and 1970.[11] Each of these surveys included a battery of questions dealing with public policy on race relations, foreign affairs, and a variety of domestic problems. These issues provide a wealth of data on which to test the hypothesized relationships between political issues and cynicism. Since all of the policy questions were

[6] John Robinson, Jerrold Rusk, and Kendra B. Head, *Measures of Political Attitudes* (Ann Arbor, Michigan: Institute for Social Research, 1969), pp. 626–647.

[7] Donald E. Stokes, "Popular Evaluations of Government: An Empirical Assessment" in *Ethics and Bigness: Scientific, Academic, Religious, Political and Military*, ed. Harlan Cleveland and Harold D. Lasswell (New York: Harper & Brothers, 1962), p. 64.

[8] See Joel D. Aberbach, "Alienation and Political Behavior," *American Political Science Review*, 63 March, 1969), 36–99 for a discussion of the importance of a specific referent when measuring political alienation. See also Kenneth Keniston, *The Uncommitted: Alienated Youth in American Society* (New York: Harcourt, Brace & World, 1965), pp. 453–455.

[9] Cynicism and efficacy correspond very closely to Seeman's conceptualizations of normlessness and powerlessness and have been treated as theoretically separate components of political alienation by a number of analysts. Melvin Seeman in "On the Meaning of Alienation," *American Sociological Review*, 24 (December 1959), 783–791 defines normlessness as ". . . a high expectancy that socially unapproved behaviors are required to achieve given goals." Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown & Co., 1965) have emphasized the difference between these two concepts by drawing the distinction between "input affect," that is, political efficacy, and "output affect," or political cynicism (discussed in terms of trust). Gamson, in *Power and Discontent*, p. 42, has likewise stressed their conceptual differences. He notes that the efficacy dimension of political alienation refers to "people's perception of their ability to influence." There currently exists an extensive body of literature dealing with the conceptual problem of studying political trust, political efficacy and, more generally, political alienation. See, in particular, Aberbach, "Alienation and Political Behavior," and Finifter, "Dimensions of Political Alienation."

[10] The empirical literature investigating trust is characteristically lacking in two respects: longitudinal considerations are rarely entertained, and little use is made of political issues as explanatory variables. Furthermore, an overwhelming majority of those few studies dealing with any form of relationship between issues and cynicism were either based upon restricted populations, such as college students or particular cities, or done under special conditions, such as at times of riots. Some studies that have dealt with issues but used limited samples are: William A. Gamson, "The Fluoridation Dialogue: Is it an Ideological Conflict?," *Public Opinion Quarterly*, 25 (Winter 1961), 526–537; Edgar Litt, "Political Cynicism and Political Futility," *Journal of Politics*, 25 (May, 1963), 312–323; Joel D. Aberbach and Jack L. Walker, "Political Trust and Racial Ideology," *The American Political Science Review*, 64 (December, 1970), 1199–1219.

[11] The sample of respondents for each study is representative of a cross section of eligible voters living in private dwelling units within the continental United States. In 1970 the sample also included 18 to 20 year olds although they were not at the time eligible to vote. In the analyses making comparisons across the four different studies, only eligible voters were used. The 18 to 20 year olds, however, were included in any analysis which dealt with the 1970 data alone. The data were made available by the Inter-university Consortium for Political Research. Neither the Center for Political Studies nor the Consortium bear any responsibility for the analyses or interpretations presented here.

Table 1. Responses to Cynicism Items, 1964–1970

How much of the time do you think you can *trust* the government in Washington to do what is right—*just about always, most of the time*, or *only some of the time?*

	1964	1966	1968	1970
Always	14.0%	17.0%	7.5%	6.4%
Most of the Time	62.0	48.0	53.4	47.1
Only Some of the Time[a]	22.0	31.0	37.0	44.2
Don't know	2.0	4.0[b]	2.1	2.3
Total	100.0%	100.0%	100.0%	100.0%
(*N*)[c]	(4658)	(1291)	(1557)	(1514)

Would you say the government is pretty much run by *a few big interests* looking out for themselves or that it is run for the *benefit of all* the people?

	1964	1966	1968	1970
For Benefit of All	64.0%	53.0%	51.8%	40.6%
Few Big Interests[a]	29.0	34.0	39.2	49.6
Other; depends; both checked	4.0	6.0	4.6	5.0
Don't know	3.0	7.0	4.3	4.8
Total	100.0%	100.0%	100.0%	100.0%

Do you think that people in the government waste *a lot* of the money we pay in taxes, waste *some* of it or *don't waste very much of it?*[d]

	1964	1966	1968	1970
Not Much	6.5%	—	4.2%	3.7%
Some	44.5	—	33.1	26.1
A Lot[a]	46.3	—	57.4	68.7
Don't know; not ascertained	2.7	—	5.3	1.5
Total	100.0%		100.0%	100.0%

Do you feel that almost all of the people running the government are smart people who usually *know what they are doing*, or do you think that quite a few of them *don't seem to know what they are doing?*[d]

	1964	1966	1968	1970
Know What They're Doing	68.2%	—	56.2%	51.2%
Don't Know What They're Doing[a]	27.4	—	36.1	44.1
Other; depends	1.9	—	1.8	2.3
Don't know; not ascertained	2.5	—	5.9	2.4
Total	100.0%		100.0%	100.0%

Do you think that *quite a few* of the people running the government are a little crooked, *not very many* are, or do you think *hardly any* of them are crooked at all?[d]

	1964	1966	1968	1970
Hardly Any	18.2%	—	18.4%	15.9%
Not Many	48.4	—	49.3	48.8
Quite a Lot[a]	28.0	—	24.8	31.0
Don't know; not ascertained	5.4	—	7.5	4.3
Total	100.0%		100.0%	100.0%

[a] Indicates response interpreted as "cynical."
[b] Includes 1% coded "It depends."
[c] The sample size for each of the years applies to all five items. The 1964 *N* is weighted.
[d] These items were not included in the 1966 election study interview schedule.

not similarly measured in each of the studies, however, analysis was limited to those issues for which comparable measures existed at different time points.

The five survey questions regularly used in the CPS election studies to measure political cynicism (see Table 1) form a single dimension.[12] They were measured by means of a Guttman scale ranging from least cynical (score of zero) to most cynical (score of five). Whenever reliability of statistical analysis required fairly substantial numbers of respondents in each category, the six categories of the cynicism scale were collapsed into three by combining 0+1, 2+3, and 4+5.

Throughout this paper percentage difference indices will be used in the presentation of data. A percentage difference index (PDI) value indicates the preponderance of one type of response over another at a given time point for a single variable. For example, a trust in government PDI based on the first item of Table 1 may be computed by subtracting the percentage of "most cynical" responses ("only some of the time") from the percentage of "least cynical" responses ("always"). Resulting negative index values indicate a preponderance of "most cynical" responses over "least cynical" responses, while positive index values indicate the reverse. The possible range of PDI values is from +100 to −100. In the specific example for Table 1, the magnitude of the negative value indicates the *degree* of political cynicism; conversely, positive values indicate trust. If all respondents said they could trust the government "only some of the time," the resulting PDI value would be −100, whereas a +100 would result if all respondents said they can "just about always" trust the government to do what is right. The actual values that would result for the item from Table 1 are:

	1964	1966	1968	1970
PDI	−8.0	−14.0	−29.5	−37.8

These PDI values very clearly and parsimoniously display the trend toward increased cynicism previously discussed with respect to Table 1.

Cynicism PDI values can similarly be computed for categories of an independent variable rather than for a single item at different time points. This can be accomplished from the column percentages of a cross-tabulation in which the collapsed three-category political cynicism scale is the row variable and the independent variable (such as one of the racial issues from Table 3) is the column variable. The PDI values for Table 3 were computed by subtracting the percentage of "most cynical" (a score of 4 or 5 on the cynicism scale) from the percentage of "least cynical" (0 and 1 on the cynicism scale) for each category of the issue question. Resulting negative index values indicate degree of cynicism or a preponderance of "most cynical" responses over "least cynical" responses; conversely, positive values indicate degree of trust.

Racial Comparisons. Previous research has suggested the possibility of substantial differences in the attitude of political trust for blacks and whites.[13] Such differences, if they exist, could have confounding effects on other findings; thus racial comparisons are presented at this point. Figure 1, which presents the trust in government PDI based on the first item of Table 1 by race, indeed justifies this comparison as it provides a vivid portrayal of racial differences.[14] Blacks demonstrated more trust in the government than whites prior to 1968, with a sharp divergence and reversal occurring between the races after 1968. During the two-year period from 1964–1966, while blacks were registering more faith in the government, whites exhibited a rapid decline in trust. Since 1966, however, political cynicism has apparently been increasing among blacks and whites, although the rate of change for whites has been at a somewhat slower pace. That this trend does not occur because of some idiosyncratic aspect of the single survey item used in Figure 1 is fully substantiated by similar findings using the full cynicism scale. For example, the percentage of blacks giving cynical responses to all five scale questions in 1970 (40 per cent) was nearly four times the percentage doing so in 1964 (11 per cent). For whites, on the

[12] Factor analysis (with about 20 items for each study) indicated that for each of the four election studies under consideration, the five trust items formed a single dimension. The factor loadings for the five items were all well above .70 and the reproducibility coefficient was greater than .90 in all cases. On the basis of these criteria the five items were judged to satisfy the requirements of a one-dimensional Guttman scale. The scale scores were obtained with the Guttman scale scoring program in the OSIRIS package of social science software. The items were dichotomized as indicated in Table 1; only two items of missing data per individual were allowed; only two errors per individual were allowed; the "median" method of error correction was utilized for scoring error cases when appropriate.

[13] Several studies have dealt explicitly with racial differences and political trust but with conflicting results. Finifter, for example, found that in 1960 blacks manifested a higher degree of normlessness than whites, a finding that runs counter to the results obtained from 1964 SRC data.

[14] The item asking "How often can you trust the government in Washington to do what is right?" was selected for this portion of the analysis because it was one of the two items asked in all four years. It is also very much an "average" item for the set and has the greatest face validity as a measure of political trust. All responses including "Don't Know" were incorporated in the percentage base used to compute the percentage difference index.

other hand, the percentage of respondents giving cynical answers to all five items between 1964 (11 per cent) and 1970 (20 per cent) had nearly doubled. Such differences in level of political cynicism and rate of change suggest the possibility of several alternative explanations for the highly revealing racial subgroup differences found in the subsequent analysis.

Policy Position as an Explanation of Cynicism

The first possible explanation of political cynicism is that individuals are cynical because they prefer an unpopular policy alternative on some political issue or issues. Key[15] has described the distribution of public attitudes on political issues in terms of two general models of consensus and conflict. According to Key, the development of public opinion is from conflict to consensus, with consensus resulting either out of compromise and the movement of individuals to a middle position on the issue or coalescense in favor of one policy alternative. Presumably, the majority opinion would be enacted into policy and, according to the hypothesized relationship, the dissenting minority would fall into the most cynical category because their alternative was not adopted.

Racial Issues. Public policy dealing with racial integration provides a prime subject area for analysis here because the government's policy position, and even more so its rhetoric, can be equated with active enforcement of integration since passage of the 1964 Civil Rights Act. Hypothetically, therefore, individuals in agreement with an integration position should be least cynical while those opposed should be most cynical, leading one to expect a correlation between cynicism and issue position. The correlations (gamma) between political cynicism and racial issues dealing with the role of the government in racial integration, as well as the style and progress of the Civil Rights movement, are displayed in the final column of Table 2.

The difference in the pattern of correlations for 1964 and 1970 is striking. The 1964 correlations are all moderate in size and fit the prediction of the hypothesis. This is true for those items which reflect questions of government action, such as integration of schools and public accommodations, as well as for those concerning the style—whether peaceful or violent—and progress of the Civil Rights movement. For the 1970 data, however, the relationships are very weak.

What are we, therefore, to conclude from the 1970 racial integration items? Had the Civil Rights movement become so integral a part of

[15] V. O. Key, Jr., *Public Opinion and American Democracy* (New York: Alfred A. Knopf, 1965), 27–76.

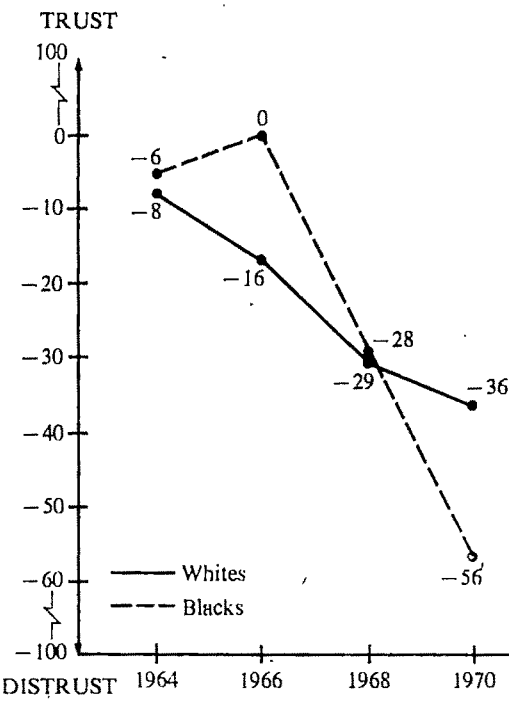


Figure 1: Index of trust in government by race, 1964–1970

the American consciousness that a depoliticization of the issue had occurred through the simple acceptance of integration as a way of life, and, more significantly, that even the dissenters were no longer cynical? Probably not, since such an interpretation would completely negate what is readily apparent from even a cursory examination of the racial situation in America today. On the contrary, a much different phenomenon appears to have occurred, altering the basic form of the relationships between political cynicism and racial issues.

The percentage difference indices (PDI) presented in Table 2 more completely describe the relationships between racial issues and cynicism and clearly display the changes which have occurred in this relationship between 1964 and 1970. In 1964, as hypothesized, there was a linear relationship between cynicism and issue position: those opposed to government intervention in race relations were considerably more cynical than those favoring such intervention. For example, in 1964, the 38 per cent of the population believing that integration of schools was not the federal government's business had a PDI value of 25.7; for the 41 per cent in favor of government intervention, it was 57.9; and for the seven per cent favoring a compromise position, 29.3. In 1970, the most cynical respondents were still those who

Table 2. Cynicism Percentage Difference Indices for 1964 and 1970

Do you think the government in Washington should see to it that White and Negro children are allowed to go to the same schools or should the government stay out of this area as it is not its business?[a]

	Same Schools	Other; depends	Stay Out	Gamma
1964 PDI[b]	57.9	29.3	25.7	.32
1970 PDI	− 0.6	− 1.0	−15.6	.07
Change	(58.5)	(30.3)	(41.3)	

Should the government support the right of Negroes to go to any hotel or restaurant they can afford or should it stay out of this matter?[a]

	Go to Any	Other; depends	Stay Out	Gamma
1964 PDI	55.9	35.6	23.3	.33
1970 PDI	− 2.9	3.7	−21.2	.13
Change	(58.8)	(31.9)	(44.5)	

Which of these statements would you agree with: White people have a right to keep Negroes out of their neighborhoods if they want to *or* Negroes have a right to live wherever they can afford to, just like anybody else?[a]

	Whites have right	Don't know; depends	Negroes have right	Gamma
1964 PDI	23.4	50.4	50.3	−.29
1970 PDI	−18.9	− 6.7	− 0.2	−.15
Change	(42.3)	(57.1)	(50.5)	

Do you think the civil rights leaders are trying to push too fast, are going too slowly, or are they moving about the right speed?[a]

	Too fast	About right	Too slowly	Gamma
1964 PDI	36.1	57.9	48.7	−.22
1970 PDI	−10.8	13.3	−27.3	−.08
Change	(46.9)	(44.6)	(76.0)	

During the past year or so, would you say that most of the actions Negroes have taken to get the things they want have been violent, or have most been peaceful?[a]

	Most peaceful	Some violent; some not	Most violent	Gamma
1964 PDI	32.3	46.1	60.1	−.29
1970 PDI	−12.6	24.5	4.3	−.16
Change	(44.9)	(21.6)	(55.8)	

[a] Full wording of the question can be found in the 1964 or 1970 CPS election study questionnaires.
[b] Negative values of the cynicism PDI indicate degree of cynicism; positive values indicate degree of trust.

opposed government integration of schools (−15.6), but the difference between these individuals and those favoring intervention (−0.6) had narrowed appreciably. Indeed, the difference in degree of cynicism between those favoring a compromise position and those favoring government intervention had disappeared. For the question on integration of public accommodations, the relationship had reversed, with those choosing compromise manifesting the least cynicism.

The shift in cynicism for those opposing and favoring intervention can be roughly estimated by subtracting the 1970 index values from those for 1964. The results of this operation are presented as the "change" values in Table 2. Understandably, "change" in the PDI from 1964 to 1970 is based upon aggregate shifts; statements about individual-level change from such a statistic are necessarily replete with untold hazards. The data obtained from such an index are presented and

used here, however, because, in the absence of panel data, they are not only suggestive but also theoretically interesting.[16]

The change in the attitude of trust indicated in Table 2 suggests that the increase in cynicism from 1964 to 1970 for supporters of government intervention was considerably greater than that for opponents of intervention; "change" values are 58.5 and 41.3, respectively. Again, similar patterns of change in cynicism can be observed for the issues dealing with integration of public accommodations and housing integration. In each case, there has been a greater degree of change in the direction of increased cynicism for supporters of government intervention than for opponents of intervention. Likewise, in each case the cynicism seems to have increased at a slower rate among the relatively small subset of the population favoring a compromise position.

Turning to the questions dealing with the style and progress of the Civil Rights movement, a similar pattern of change can be noted. In 1964, the five per cent of the population who thought the movement had been going "too slowly" and the 24 per cent who thought it was going at "about the right speed" were considerably less cynical than the 63 per cent believing that civil rights leaders were pushing "too fast." By 1970, a curvilinear pattern had developed with those (by then 9 per cent of the population) finding the progress of the movement too slow being much more cynical (−27.3) than the 52 per cent thinking it had been going too fast (−10.8); those who felt that the movement was going at about the right speed were still the least cynical (13.3). By far the largest increase in cynicism (indicated in Table 2 by a "change" value of 76.0) occurred among respondents impatient with the progress being made by the Civil Rights movement.

Furthermore, individuals in both 1964 and 1970 who associated actions taken by blacks "to get the things they want" with violence gave evidence of greater cynicism than did those perceiving such behavior as peaceful. In 1964, the difference in degree of cynicism between these two groups was astonishing. Those perceiving the movement as peaceful were highly trusting in their attitude toward the government (60.1), while those who saw black actions as mostly violent were much less so (32.3). By 1970, this difference had greatly narrowed, indicating a much greater increase in cynicism among people perceiving civil rights actions as peaceful. Notably, about two-thirds of the white respondents *in both years* thought the actions of the Civil Rights movement had been violent, whereas about an equal percentage (68%) of blacks felt that they had been peaceful.[17]

In summary, it can be said that consensus had not occurred on racial issues. In 1970, despite a gradual shift in a pro-integrationist direction,[18] attitudes on school integration were still distributed in a bimodal fashion, particularly for whites. There had not been a coalescing of support for enacted policy, as has so often been the case with controversial programs such as, for example, the sweeping social welfare programs of the New Deal era. Nor has the discontent among the dissenters subsided. On the contrary, political cynicism has consistently been most prevalent among those favoring segregation and believing that the federal government should not play a role in the integration of schools and public accommodations. The surprising and unexplained phenomenon disclosed by the data analysis, however, is that individuals in favor of forced integration had also become discontented, and apparently at a faster rate.

A Partial Explanation. A clue to this observed increase in discontent and cynicism among blacks and white integrationists is suggested by the pattern of change presented earlier in Figure 1. Trust in the government may have increased among blacks between 1964 and 1966 out of a reaction to the Civil Rights Bill of 1964; the decrease for whites may also be similarly explained. The Bill may have increased the hope among blacks and white integrationists that a real change in racial integration, along with a reduction in discrimination, would be forthcoming. At the same time, the security of white segregationists might have been threatened by the fear that their social and economic positions would be challenged by blacks with the support of the government. Or, they may have resentfully thought, "The government is helping the blacks, why doesn't it help me?" Such reactions would partially explain the change in cynicism between 1964 and 1966 while setting the stage for the changes which ensued after 1966.

It is suggested that actual modification of the

[16] It should be noted that a model based upon the replacement of older, less cynical individuals in the population (who have presumably died) with more cynical, newly eligible voters does not explain the general increase in cynicism for the population. This replacement hypothesis was rejected because the data analysis shows that in 1970 for the total population those under 30 years of age were less cynical than those over 60.

[17] For an insightful study of what various people perceive to be violence, see Monica D. Blumenthal, Robert L. Kahn, Frank M. Andrews, and Kendra B. Head, *Justifying Violence: Attitudes of American Men* (Ann Arbor, Michigan: Institute for Social Research, 1972).

[18] Angus Campbell, *White Attitudes Toward Black People* (Ann Arbor, Michigan: Institute for Social Research, 1971).

Table 3. Support for Vietnam Policy, 1964–1970

Which of the following do you think we should do *now* in Vietnam?	1964	1966	1968	1970
Pull out of Vietnam entirely	8.3%	9.1%	19.8%	32.3%
Keep our soldiers in Vietnam but try to end the fighting	25.0	35.6	36.0	32.0
Take a stronger stand even if it means invading North Vietnam	31.6	35.9	33.5	24.2
Other; depends	0.1	2.2	3.3	5.1
Don't know; not ascertained	35.0	17.2	7.4	6.4
Total	100.0%	100.0%	100.0%	100.0%
(*N*)	(4658)[a]	(1291)	(1557)	(1514)
Percentage Difference Index[b]	23.3	26.8	13.7	−8.1

[a] Weighted *N*.
[b] Positive values indicate greater support for escalation; negative values indicate more support for withdrawal.

racial situation and the conditions of racial discrimination have not paralleled the rising expectations of the black community or those sympathetic to their plight, nor at the same time have they been as drastic as feared by white segregationists. Trust in the government may have thus declined sharply among blacks after 1966 because of frustration arising out of the unfulfilled expectation of more active government involvement in the area of integration.[19] Coincidentally, the Civil Rights movement was making blacks, as a whole, increasingly more aware of the real constraints and discrimination existent in the system, thereby increasing their discontent and reaffirming for them even more staunchly the notion that only a change in the system would improve the "black condition."[20] On the other hand, those opposed to integration were also dissatisfied with governmental performance in this area, presumably in part because between 1968 and 1970 the Nixon administration did not attempt to, nor could it, completely reverse the process of integration.

The Vietnam Issue. While significant differences in the degree of cynicism have been found to exist among people favoring alternative policy positions on racial issues, these issues do not completely explain all the increase or the variation in cynicism occurring since 1964. The Vietnam war, an issue with steadily increasing political potency since the early 1960s, appears also to be related to the attitude of trust in government. Table 3 presents the distribution of support for Vietnam policy alternatives from 1964 to 1970. A percentage difference index for the Vietnam issue, computed by subtracting the percentage of respondents favoring withdrawal from the percentage favoring escalation whereby a negative value indicates greater support for withdrawal, is also presented.

The Vietnam index in Table 3 summarizes the trend in attitude among the general population on this issue. This trend toward support for withdrawal certainly comes as no surprise, for it has previously been documented by various polling agencies and publicized extensively in the mass media. However, the *source* of increased support for withdrawal requires more careful attention. (These data, of course, are only suggestive, since the interpretation cannot be termed conclusive without recourse to panel data.) From 1964 to 1970, the percentage of individuals favoring withdrawal from Vietnam increased by 24 per cent while the percentage preferring escalation remained relatively stable, except for an approximate 9 per cent decline from 1968 to 1970. This suggests that most of the increased preference for withdrawal from Vietnam may have come less from converting "hawks" to "doves" than either from attitude changes among those preferring a compromise position or from the uninformed

[19] This explanation is given additional support by the degree of change in political trust found for blacks who felt the Civil Rights movement was going too slowly. In 1964, 27 per cent of the blacks fell into this category; six years later, the figure was 39 per cent. The growing impatience on the part of the blacks appears to be reflected in their increased cynicism, as indicated by the respective cynicism index values of 57.8 and −45.2. The magnitude of attitude change suggested by the difference between these two values is extraordinary, particularly for such a basic and presumably stable attitude as political trust. The aggregate increase in distrust of the government was substantially less for the 63 per cent of black respondents who felt that Civil Rights was moving at about the right speed. In 1964, the cynicism index value for these blacks was 53.6—not very different from impatient blacks—but by 1970 the blacks who were satisfied with the speed of the movement (now down to 54 per cent) were very much less cynical (−19.6) as a group than blacks who thought progress was too slow.

[20] Patricia Gurin, Gerald Gurin, Rosina Lao, and Muriel Beattie, "Internal-External Control in the Motivational Dynamics of Negro Youth," *Journal of Social Issues*, 25 (July. 1969), 29–53, investigates whether an individual places the blame for social or economic failure among blacks on the individual or the system.

public taking a dovish position as the Vietnam issue became more salient.[21]

A model that relates policy dissent with discontent would predict that dissenting "doves" are more cynical than "hawks." Such a relationship would help explain the increase in cynicism occurring between 1964 and 1970, the same period during which the ranks of the "doves" increased. The trust in government (from Figure 1) and Vietnam percentage difference indices were plotted with respect to each other for whites and blacks and are presented in Figure 2.

Figure 2 reveals that the pattern of change in support for withdrawal from Vietnam is very similar to change in trust, especially for blacks who have been consistently more "dovish" than whites. The slight increase in support for escalation for both groups in 1966 probably reflects a leadership effort that rallied early support for Johnson's policy before attitudes on Vietnam became more informed. This would seem to be particularly true for blacks as about 29 per cent of them did not have an opinion on Vietnam in 1966, compared to 16 per cent of the whites. After 1966, however, the black community quickly moved in the direction of support for withdrawal, with only about nine per cent of the total black population in favor of escalation and 50 per cent supporting withdrawal.

The whites, on the other hand, have been more reticent in their support for withdrawal. Furthermore, a closer look at the Vietnam and trust curves for whites reveals certain discrepancies at odds with the original hypothesis. Not only are the Vietnam and trust curves less parallel for whites than for blacks, but there is also a definite

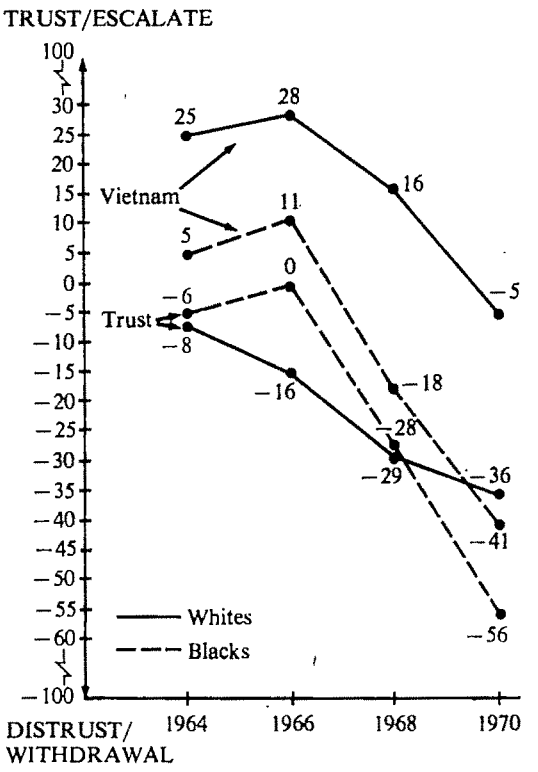


Figure 2: Indices of trust in government and support for withdrawal from Vietnam, 1964–1970

divergence of the two curves for whites in 1966. While support for escalation was increasing relative to support for withdrawal among white respondents, trust was declining. Inspection of the relationships between Vietnam policy preference and political cynicism for 1964 and 1970 provides a partial explanation for these discrepancies. Table 4 presents the percentage responses and cynicism index for the Vietnam question for whites only for these years.

The most immediate observation made from Table 4 is that the original prediction that the most cynical would be those favoring withdrawal is partly false. In 1964, respondents favoring escalation were less trusting of the government (cynicism index value of 27.5) than those preferring withdrawal (37.1); in 1970, the reverse was true (−7.2 and −12.6, respectively). The most striking difference found in Table 4 is not, however, between those supporting withdrawal and those preferring escalation, but rather between the latter two groups and those wanting to keep the soldiers in Vietnam while trying to end the fighting. Respondents choosing this category were definitely less cynical than either the "hawks" or the "doves," both in 1964 and in 1970. Hence, a curvilinear rather than a linear relationship is

[21] Two pieces of information lend credence to this interpretation. First, the very substantial decrease in the percentage of respondents saying "don't know" corresponds more closely to the percentage increases in the middle and withdrawal categories than it does to the percentage fluctuation found in the escalation category. Second, as Milton Rosenberg, Sidney Verba and Philip E. Converse point out in *Vietnam and the Silent Majority: The Dove's Guide* (New York: Harper & Row, Publishers, 1970), p. 55, it is the "hawks" who have been the more informed on the Vietnam issue; hence, their attitudes on Vietnam would be more crystalized and less susceptible to change. What these data suggest, therefore, is that while there had been a trend toward support of withdrawal from Vietnam, there had not been a concurrent reduction in the polarization of attitudes on Vietnam policy. Continued conflict over Vietnam policy is understandable, given continued dissent among opinion leadership—identified by Richard A. Brody and Sidney A. Verba in "Hawk and Dove: The Search for an Explanation of Vietnam Policy Preferences," *Acta Politica*, 7 (July, 1972), 285–322, as among the most important independent variables in explaining opinions on Vietnam —as well as changes in official policy, a factor which tends to reduce the impact "current policy" would have on a converging policy preference.

Table 4. Current Vietnam Policy Preference in Relation to Trust in Government for Whites, 1964 and 1970

Trust in Government	Vietnam Policy Preference: 1964		
	Withdraw	Maintain	Escalate
High	58.3%	74.6%	53.8%
Medium	20.4	14.9	19.9
Low	21.3	10.5	26.3
Total	100.0%	100.0%	100.0%
(*N*)	(108)	(315)	(433)
Percentage Difference Index[a]	37.1	64.1	27.5

Trust in Government	Vietnam Policy Preference: 1970		
	Withdraw	Maintain	Escalate
High	29.5%	46.8%	32.5%
Medium	28.4	28.4	27.8
Low	42.1	24.8	39.7
Total	100.0%	100.0%	100.0%
(*N*)	(437)	(455)	(363)
Percentage Difference Index[a]	−12.6	22.0	−7.2

[a] Negative values of the Cynicism Percentage Difference Index indicate degree of political cynicism; conversely, positive values indicate trust.

found between Vietnam policy preference and political trust, thereby increasing the complexity of the interpretation.

As Rosenberg and others[22] have demonstrated, individuals preferring escalation have often been dissatisfied with Vietnam policy even while a stronger stand was being taken because in their opinion the escalation was not sufficient. Increased dissatisfaction with the intensity of the escalation may explain the increase in cynicism among "hawks" in 1966 and thus the divergence noted in Figure 2. Cynicism among "doves" can similarly be explained on the basis of dissent in 1964 and on the basis of growing impatience in 1970.

The Social Issue. Does the opinion polarization and corresponding curvilinear relationship with cynicism also exist for other contemporary issues? In the 1968 election study, a new type of structured question was used to measure attitudes on contemporary issues.[23] Respondents were presented a series of questions utilizing a seven-point scale running from one policy alternative specified at point "1" to an opposing policy alternative at point "7." (See Appendix.) The 1968 respondents were asked to place themselves and several political leaders (the presidential contenders) on the scale. In 1970, they were asked not only to place themselves and three political leaders (Nixon, Wallace and Muskie) on the scale, but also the Democratic and Republican parties.

The data obtained from these new measures are excitingly more vast than those provided by traditional structured issue questions which elicited the respondent's preference for one of a limited number of alternative policy choices. The new items furnish information not only on the respondent's policy position, but also on measures of agreement between what the respondent would like to see accomplished and what he perceives each of the parties and various political leaders as doing with respect to particular issues. For this reason, greater emphasis is given to those issues comparable over time, as well as to the contemporary problems underlying the issue measures of the 1970 election study.

It was felt that the relationship between these contemporary social problems and the attitude of trust should be investigated to determine if present discontent is related to particular issues only or if it arises out of a general pattern of policy preference.[24] Percentage distributions for the respondents' self-placement on the various issues, along with the cynicism PDI for each issue, are presented in Table 5.

The distributions of policy preference for the eight issues in Table 5 have two basic patterns. Skewed distributions are found for campus demonstrations, government action against inflation, and pollution; the remaining five issues have trimodal distributions. The trimodal distributions disclose a great deal of conflict over policy alternatives. On the average, approximately twice as many respondents preferred the policy alternatives suggested by the extremes of the scales as

[22] Milton Rosenberg, Philip E. Converse, Sidney Verba, *Vietnam and the Silent Majority;* as well as Philip E. Converse, Warren E. Miller, Jerrold Rusk and Arthur Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review,* 63 (December, 1969), 1083–1105.

[23] The new measures were developed by Richard A. Brody and Sidney Verba.

[24] The saliency of the contemporary issues is important, and an attempt was made to assess its impact on the relationships investigated here. The association between political cynicism and the seven-point issue measures was obtained for respondents who mentioned the particular issues as a problem in an open-ended question referring to the most important problems facing the government today. A comparison of the correlations for this presumably "more aware" subset and the remainder of the respondents consistently showed only a *very slight* increase in the correlations for the "more aware." This suggests that although people may not volunteer particular contemporary issues in response to open-ended questions, they are still aware of them.

Table 5. Policy Preference and Cynicism Percentage Difference Indices for 1970

	1	2	3	4	5	6	7	Total
	Solve problems of poverty & unemployment					*Use all available force to quell urban unrest*		
Row %	20.1%	9.5	12.4	26.4	9.5	5.6	16.5	100.0%
PDI[a]	− 5.1	0.0	20.4	3.4	6.1	−10.5	−22.6	(*N*=1535)
	Immediate withdrawal					*Complete military victory*		
Row %	21.7%	8.4	11.0	24.2	8.9	6.4	19.2	100.0%
PDI	−27.7	7.1	7.8	10.3	27.9	10.2	−14.1	(*N*=1522)
	Sympathetic with students & faculty					*Use force to stop campus disturbances*		
Row %	6.2%	5.0	6.0	17.6	10.8	14.5	40.0	100.0%
PDI	−16.5	− 1.4	−15.0	9.3	15.9	15.6	−11.8	(*N*=1459)
	Government help minority groups				*Minority groups help themselves*			
Row %	14.9%	8.3	7.9	25.1	12.1	10.9	20.8	100.0%
PDI	−11.4	7.6	15.2	8.7	16.9	− 4.5	−21.7	(*N*=1419)
	Total government action against inflation					*No government action against inflation*		
Row %	46.4%	20.1	13.7	10.5	3.1	2.2	4.0	100.0%
PDI	− 8.8	5.6	10.7	10.0	− 2.2	19.4	−17.2	(*N*=1433)
	Protect rights of accused					*Stop crime regardless of rights of accused*		
Row %	19.7%	7.9	6.8	17.2	12.4	14.3	21.6	100.0%
PDI	− 2.7	18.5	13.9	8.2	8.9	1.1	−16.4	(*N*=1359)
	Government force private industry to stop polluting					*Industries should handle pollution in their own way*		
Row %	52.2%	13.6	9.0	12.2	3.3	3.3	6.3	100.0%
PDI	− 3.4	14.4	− 6.4	2.4	19.6	− 8.7	−11.5	(*N*=1377)
	Government health insurance plan				*Private health insurance plans*			
Row %	28.4%	8.9	8.2	14.5	6.4	9.7	23.9	100.0%
PDI	−10.0	− 1.7	3.7	− 7.2	21.0	20.0	− 1.9	(*N*=1337)

[a] Negative values of the Cynicism Percentage Difference Index (PDI) indicate degree of political cynicism; conversely, positive values indicate trust.

did those favoring a centrist solution. From 36 to 52 per cent supported the extreme alternatives (#1 and #7) while only 14 to 26 per cent chose a compromise solution (#4) to the five issues. In fact, the combined total of respondents preferring the three center alternatives (#3, #4, #5) never exceeded 50 per cent, a finding which contradicts the notion that a centrist policy alternative to these issues is preferred by a majority of Americans.[25] On the contrary, the distributions suggest a tendency toward bipolarization rather than unanimity.

While the distribution of policy preferences varied over the issues from skewed to multimodal, there was a clear pattern to the relationships between issue position and trust in government. Those individuals preferring the opposing policy alternatives identified with the extreme ends of the scales were, on the whole, more cynical than those placing themselves in the center for each item in Table 5. Thus, the curvilinearity found in the relationships between policy preference on the Vietnam and civil rights issues and trust in government is also found for the social and economic issues of Table 5, regardless of the actual distribution of policy preferences.

The preceding analysis clearly demonstrates that no monolithic description of cynicism arises out of policy preferences for the issues investigated. Rather, what evolves is that those in the center were the least cynical, and that the most cynical formed two ideologically distinct types:

[25] Richard M. Scammon and Ben J. Wattenberg, *The Real Majority* (New York: Coward, McCann and Geohegan, Inc., 1971). The implications of their major premise—that a presidential candidate must move to the center in order to win—is that a centrist policy is preferred by a majority of the voters.

"cynics of the left" and "cynics of the right." Operationally defining "cynics of the left" and "cynics of the right" as those both "most" cynical (#4 or #5 on the cynicism scale) and preferring the respective policy alternatives #1 through #3 and #5 through #7 on the five most highly clustered (interitem Pearson r ranging from .20 to .50) of the eight issues, allows for a refined comparison of the two groups. The five issues included in the cluster were: urban unrest, Vietnam, campus demonstrations, protecting the rights of the accused, and government aid to minorities. Such an operational definition provides fairly pure types of cynics, with 11 per cent of the population ($N=176$) classified as "most" cynical on the left and 19 per cent ($N=290$) on the right.

"Cynics of the left" may be described as predisposed to social and political change. Witness their support of policy alternatives indicating that solutions to contemporary social problems lie in the alteration of the existing system: the prevention of riots in cities by solving the problems of unemployment and poverty, government aid to improve the social and economic conditions of minority groups, a pro-integrationist stand on civil rights, and protection of the rights of the accused. This predisposition is supported by their apparent belief that constraints in the system, rather than individual shortcomings, are to blame for the failure of people, particularly minority groups, to "get ahead" in society. This system-blame attitude is evident in their strong preference for government action to improve the social and economic conditions of minority groups as well as that of all those who suffer from poverty and unemployment. As is further suggested by their responses to the student unrest issue, they appear to be somewhat favorably inclined toward the use of collective action for obtaining that change; hence, they are sympathetic with demonstrating students.

"Cynics of the right," may, on the other hand, be described as more predisposed toward social control[26] than social change. That is, they are bent upon maintaining the system as it exists and support authorities and policies which act to control those who may be potentially disruptive of the orderly functioning of the system. They are inclined toward segregation policies and policies tending to disregard the rights of the accused in an attempt to prevent crime, and are strongly in favor of complete military victory in Vietnam. The Protestant Ethic seems engrained in "cynics of the right" as evidenced by their greater stress on the individual rather than the system for one's failure to attain social status; they were firmly against government assistance aimed at improving the social and economic position of minority groups. Their corresponding support for using police force against demonstrators suggests that since they apparently believed that unsuccessful attempts at social advancement were individual shortcomings, such disruptive actions were attempts by a few to gain special privileges for themselves.

The two types of cynics were as different demographically as they were attitudinally. One-third of the "cynics of the left" were under 30 whereas only 12 per cent of the most cynical on the right were young. Blacks comprised 38 per cent of the "cynics of the left"; 99.7 per cent of the "cynics of the right" were white. While "cynics of the left" tended to identify more with the Democratic (71 per cent) than the Republican party (12 per cent), those on the right were more evenly split: 48 per cent were Democrats and 35 per cent were Republicans. The "cynics of the right" had higher incomes, with only 21 per cent making less than $4,000 a year; 38 per cent of those on the left were below that figure. Those on the left, however, were much better educated: 28 per cent had some college training or held a college degree, as compared with only 18 per cent among those of the right. Again, with blacks removed, the percentage of those on the left who had had some college rose to 38 per cent.

Alternatively, the curvilinearity in the relationship between policy preference and trust may be explained in retrospect by the increased political awareness and the type of issues prevalent today rather than by the distribution of preferences. It may be suggested that these issues, unlike those of earlier years that were more removed from an individual's immediate experience, seem to affect the very basic values and fears that form the foundation of everyday existence for the average citizen. Attitudes on these issues would, therefore, tend to be more crystallized and more firmly held than on those more remote from an individual's daily existence. Thus, it would be understandable that respondents with the most completely formed attitudes—presumably those favoring the extreme alternatives[27]—should be the most cynical. It is they who would be most dissatisfied were their well-formulated policy expectations not only left untended but also threatened by enactment of opposing policies. This "new issues" hypothesis is, however, considerably more complex an explanation of the observed curvilinearity than the "distance from current policy" interpretation. Fortunately, a comparison of the "new issues" versus

[26] For a definition of social control, see Gamson, *Power and Discontent*, pp. 11–19.

[27] Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, Inc., 1960), p. 178.

the "all policy" hypotheses can be parsimoniously arranged: Consider two indices of policy distance, that is, stated preference for policy different than current—the one composed of responses to all policy items; the other of only the new issues (Vietnam, youth protest, and rights of the accused). If the newer, more meaningful, and thus more "crystallized" issues hypothesis is correct, only that index should display the requisite curvilinearity. On the other hand, if the "all issues" index shows the same curvature, the "poor-performance/dissatisfaction/cynicism" hypothesis would seem to be affirmed. The results of such a comparison do indeed demonstrate a marked curvilinearity for both indices, as well as a third index which incorporates all issues except the "new issues." These results would, therefore, seem to substantiate the hypothesis that the curvilinearity arises out of a reaction to current policy rather than a qualitative difference between contemporary and past issues.

By 1970, Americans to a considerable degree had withdrawn some of their trust from the government because they had become widely divided on a variety of issues, for in the normal attempt to satisfy the greatest numbers, the government had generally followed a more or less centrist policy which in reality appears to have displeased a substantial proportion of the population. Issue position was, therefore, found to be related, in a complex manner, to trust in government. There is, however, nothing intrinsic in support for a particular policy alternative, or even a pattern of policy preferences, which provides a theoretical interpretation for such a finding. It was hypothesized that dissenters, that is, those favoring the policy alternative endorsed by a minority of individuals, would be the most dissatisfied and thereby the most cynical. This hypothesis proved to be inadequate in explaining either the widespread distrust of government in 1970 or the increase in distrust occurring between 1964 and 1970, because often the majority favored opposing policy alternatives and was more cynical than the minority preferring the centrist policy. Whenever an explanation was offered for the findings, it was couched in terms of expectations and policy dissatisfaction. Such an interpretation cannot, however, be tested by the data analyzed thus far. To test such explanations of political cynicism directly, the concepts of policy expectation and dissatisfaction need to be clearly operationalized and incorporated in hypotheses. Two further hypotheses which may explain the relationship between cynicism and the issues are:

(1) Cynicism arises because individuals do not perceive any difference between the policy alternative offered by the Democratic and Republican parties or their leaders.

(2) Cynicism is a result of dissatisfaction with the expected or perceived policy performance of the party currently in control of the Presidency.

Lack of Party Differences

Anthony Downs in *An Economic Theory of Democracy* contends that a rational voter casts his ballot for the political party which will provide him with "a higher utility income than any other party during the coming election period"; should the voter not feel that he would benefit relatively more from either party, he abstains.[28] Rational abstention because of a lack of perceived difference between the candidates or parties suggests the possibility that alienation, and hence political cynicism, may, in part, be related to a perceived lack of choice between the parties.

The seven-point issue scales from the 1970 election survey provide excellent measures for determining the degree of perceived party difference on the issues. As noted, respondents were asked to place both parties on each issue continuum; perceived difference between the parties can thus be computed as $\text{Democratic}_j - \text{Republican}_j$, that is, as the absolute difference between scale placements for the Democratic and Republican parties on the scale for issue "j" ($j=1$ to 8). The values for the computed difference range from 0, implying no difference, to 6, maximum difference.

The percentage of the population perceiving no difference between the parties in 1970 varied from 33 to 53 per cent, depending on the issue. The number perceiving no difference was greater than 40 per cent on only two: pollution and protecting the rights of those accused of crime. On the remaining six issues, 61 to 67 per cent of the population perceived some difference between the parties. Republicans tended to perceive slightly more difference between the parties than did Democrats, whereas Independents (Independent as defined here and throughout this article does not include those leaning toward either party) perceived the least difference, with 50 to 69 per cent perceiving no difference at all between the parties.[29]

The data analysis reveals that a fairly substantial degree of difference is, in fact, perceived between the parties. This does not, however, dis-

[28] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, Publishers, 1957), p. 39.

[29] On the whole, the Democratic party was placed more toward the left end of the issue scales, while the Republican party was perceived as endorsing policy alternatives found at the right end. Only 9 to 25 per cent (with an average of approximately 15 per cent for the eight issues) of those who were able to place both parties on the issue scales perceived the Democratic party to the right of the Republican party.

count the possibility that political cynicism increases as perceived difference between the parties decreases. If such were the case, a negative correlation would be found between political cynicism and perceived difference. The predicted inverse relationship between cynicism and a measure of the average perceived party difference for the eight issues is found, however, only for Independents, and then to a very slight degree ($r = -.10$). There is a near-zero relationship between cynicism and perceived party difference for Republicans ($r = .03$), a weak positive correlation for white Democrats ($r = .14$), and a slightly stronger positive relationship for black Democrats ($r = .20$). Only among Independents, therefore, can cynicism possibly be attributed to a lack of perceived difference between the parties; the correlation is so small, though, that such a conclusion must be tenuous.

Nevertheless, it is interesting to note that Independents may feel less trusting of the government because they believe they are not given an adequate choice, whereas Democrats, and particularly black Democrats, who perceive a greater difference between the parties and hence presumably a clearer choice, are somewhat more cynical than Democrats who perceive less difference. What these correlations suggest is that while Independents are cynical because of a lack of choice arising out of the absence of party differences, blacks are cynical because they perceive the policy gap between the Democratic and Republican parties as so large that the Republican party is not a viable alternative, thus, ironically, also resulting in a lack of choice for them.

On the other hand, the positive relationship between cynicism and perceived party difference found for Democrats may be linked to dissatisfaction with the policies of the Republican administration which was in power in 1970. It may be presumed that Democrats would prefer Democratic to Republican party policies, and that Democrats who perceive the greatest difference between the two parties would be the most dissatisfied because their policy preferences were least likely to be enacted. Cynicism would thus be expected to be greatest among those perceiving the largest differences between the Republican and Democratic parties. A more complete investigation of alternative explanations for the positive relationship between cynicism and perceived party differences is presented in the following sections. It can be said here, however, that cynicism among Americans in 1970 was not a function of perceiving little difference between policy alternatives identified with either political party. On the contrary, most people perceived substantial differences between the parties on the issues, and, indeed, there was a slight tendency for those who perceived the largest differences to be more cynical. This trend suggests that cynicism may arise out of dissatisfaction with the policy alternatives offered by one's own party and the belief that the other party is not a viable alternative.

Policy Dissatisfaction

Gamson contends that distrust of the government arises out of "the nature of the decisions made and the satisfaction or dissatisfaction with them . . . ,"[30] particularly when dissatisfaction is generalized across a number of decisions which result in undesirable outcomes. It may be hypothesized, therefore, that discontent is related to dissatisfaction with the perceived policies of the party in power on issues that are relevant to the values of a substantial segment of the population.

Data used to test the hypotheses in this section come almost exclusively from the eight 1970 election study seven-point issue questions which provide measures for operationalizing the concept of

[30] Gamson, *Power and Discontent,* p. 178. Gamson provides a general theoretical notion of the process underlying the development of political distrust. The dissatisfaction begins to be generalized when an undesirable outcome is seen as a member of a class of decisions with similar results. Authorities represent the first target of such generalization:

> If such experiences extend over more than one set of authorities, potential partisans may conclude that the institutions themselves may be the source of bias, and "throwing the rascals out" will have little effect if indeed it is even possible. . . . Attacks on political institutions may in turn lead to distrust in the ideology or public philosophy used to justify them. Finally . . . the disaffection may be generalized to the political community itself and a desire for political separation may develop.

Gamson's developmental model does not, however, incorporate a concern with cause and effect or the intricate set of interactions which may occur over time between policy dissatisfaction, distrust and social or historical events. Declining levels of political trust may be the result of increased policy dissatisfaction, or it may be the other way around, or there might be a further independent variable responsible for the decline in both these variables. Cynicism may, for example, be considered in a very generalized form to be a personality attribute that would condition one's perceptions of the world, and one of its effects might be to cause a person to perceive a greater than normal distance between his preferences and what was being done by political authority to satisfy these preferences. Some societal factor may be causing an increase in political cynicism, which in turn acts as an intervening variable to bring about a rise in dissatisfaction with government policies. Alternatively, cynicism and dissatisfaction could both be connected with some changing social condition and therefore be only spuriously related to each other. These are alternative interpretations for trends that show declining trust in government to be related to policy dissatisfaction and they are worthy of further exploration. Unfortunately, they go beyond the scope of this report and would require the analysis of panel data before any conclusive separation of cause and effect could be accomplished.

policy dissatisfaction over a set of issues in a more complete manner than the issue questions of earlier studies. The 1964 and 1968 election studies have been used, however, to validate the findings obtained from the 1970 data. Separate analyses of the earlier surveys demonstrated that the generalizations arising out of the 1970 data are, within limits of the available measures, indeed substantiated.

Policy dissatisfaction has been operationalized here in terms of the distance between the individual's own policy preference and the policy alternative that he identified with a particular party. Underlying this is the assumption that if an individual completely agrees with the policy alternative of a particular party as he perceives it (that is, when a respondent places both himself and the party at the same point on a scale), he is more satisfied with the party's policies than he would be if he identified the party with a policy some distance from his own position on the issue. It should be emphasized that this is either perceived or expected policy and not necessarily an alternative which the parties have already enacted. Similarly, that perception is, of course, unique to the individual respondent.

An indicator of policy dissatisfaction on issue "j" can be computed as the absolute difference between the individual's (i) issue scale value (i_j) and the scale value associated with the position where the respondent has placed the party (p_j). More formally, this distance measure can be represented as $d_{ip}=|i_j-p_j|$, with a range of scores from 0, minimum policy dissatisfaction, to 6, maximum policy dissatisfaction.

An average measure of policy dissatisfaction, as indicated by a mean distance score for the eight issues, was used in the analysis because this was more parsimonious than using the eight issues separately, and because it captures the effects of dissatisfaction that may be cumulative across the several issues. Using an average measure, however, posed two disadvantages: the average measure correlates at a somewhat lower value with the political cynicism scale than does the multiple correlation of the eight individual measures; and it does not answer the interesting question of the relative strength of the relationship between cynicism and any particular issue. Unfortunately, the scope of this paper does not allow for an explication of the relationship with the full set of issues nor for an unraveling of the cause and effect relationship between cynicism and policy dissatisfaction. Prior cynicism may possibly have an impact on whether or not a policy is perceived in an unfavorable fashion and this may in turn increase cynicism even more. The data used here, however, do not permit a conclusive determination of this causal sequence.

Dissatisfaction with the Republican Administration. Data which test the hypothesis that cynicism is related to dissatisfaction with the perceived policies of the party in power are presented in Table 6.[31] The table displays the mean cynicism score by distance from the Republican party on the eight issues for the total population, white and black Democrats, Republicans, and Independents. The percentage distributions for each group on the policy dissatisfaction measure (distance from Republican party) are also presented.

It is apparent from the means for the total population that a substantial increase in cynicism accompanies increased dissatisfaction with Republican party policies on the issues. The mean level of cynicism for the 13 per cent of the population least dissatisfied with Republican policy was 1.79, whereas the two per cent most dissatisfied had a mean of 4.00. Recalling that the cynicism scale values range from 0, for least cynical, to 5, for most cynical, helps place this difference in perspective.

One of the more interesting results found in Table 6 is that the relationship between policy dissatisfaction and cynicism holds for Republicans, as well as for Democrats. Indeed, the close similarity between the group mean cynicism value for Republicans (2.28) and white Democrats (2.50) demonstrates that distrust of the government is not related to party identification but to policy dissatisfaction. As expected, Republicans were, on the whole, less dissatisfied than Democrats; only about 26 per cent of the Republicans placed the party, on the average, two or more issue points away from their own policy preference, whereas 63 per cent of all Democrats did so. Still, as policy dissatisfaction increases among Republicans, trust in the government declines significantly.

It must be emphasized that these data are for 1970 only. Comparable distance measures did not exist prior to 1970; thus, the hypotheses tested here cannot be compared with data from earlier years. The single snapshot resulting from the 1970 data should not, of course, be unthinkingly generalized to all other years. Some cynicism may, indeed, arise from reaction to the "in" party, and under some circumstances party identification may have a significant, linear relationship with

[31] Despite the fact that respondents had also been asked to place President Nixon on the scale, the following analysis deals with policies perceived to be associated with the political parties only. That no particular Democratic leader was fully recognized as the counterpart to the President precluded doing analysis with political leaders. It should, however, be noted that there was for the total population an extremely close correspondence between where the Republican party and Nixon were placed on the issue scales and analysis using either one provided virtually identical results.

Table 6. Mean Cynicism by Distance from Republican Party, 1970

Distance from Republican Party	Total Cross-section		White Democrats		Black Democrats		Republicans		Independents	
	Mean[a]	%	Mean	%	Mean	%	Mean	%	Mean	%
0	1.79	(13.1)	1.42	(7.8)	1.33	(6.1)	1.89	(23.8)	2.64	(8.5)
1	2.27	(36.9)	2.14	(32.9)	2.09	(10.6)	2.25	(50.1)	2.43	(35.4)
2	2.66	(30.0)	2.60	(36.0)	2.63	(27.2)	2.57	(20.3)	3.03	(39.7)
3	3.10	(13.4)	3.06	(16.9)	3.44	(27.3)	2.65	(4.3)	3.67	(11.0)
4	3.72	(4.5)	3.25	(4.6)	4.32	(18.7)	4.57	(1.5)	3.88	(5.4)
5	4.00	(2.1)	4.00	(1.8)	4.00	(10.1)	[b]	—	[b]	—
6	[b]	—	[b]	—	[b]	—	[b]	—	[b]	—
Group Mean and Total Per cent	2.53	100.0	2.50	100.0	3.10	100.0	2.28	100.0	2.90	100.0
(*N*)	(1414)[c]		(646)		(210)[c]		(463)		(164)	
Eta	.28		.28		.44		.22		.27	

[a] Cell entries are mean cynicism values. The cynicism scale ranged from 0 = least cynical to 5 = most cynical.
[b] *N* was less than 5.
[c] To avoid the problems of weighted data, the black supplement was not included in the cross-section analysis. However, the cross-section and supplement blacks form a self-weighting sample of blacks, and supplement blacks were therefore included among black Democrats.

trust in government. That such a relationship is not derivable from the data for any of the election studies prior to 1970 does not eliminate the possibility that such a relationship could develop. In fact, the policy dissatisfaction theory argued in this paper would lead to expectations of a linear relationship between cynicism and party identification under certain conditions. This pattern can be expected, for example, when the policies that are most strongly related to political discontent over time are also strongly aligned with party identification. Thus, if Republicans control the administration and Democrats believe Republican policies to primarily benefit certain types of individuals who may also happen to be Republican, then a correspondence between policy dissatisfaction, party identification and cynicism can be expected. When the policies that are correlated with discontent over time are orthogonal to party identification and partisan cleavages, however, the relationship becomes curvilinear and weaker. As the dynamics of a spiral of discontent come into play, the resulting cynicism may also affect perceived policy performance on more traditional issues, and this in turn may affect party identification itself. Unfortunately, the testing of these dynamic hypotheses cannot be accomplished with the data presented here.

The strongest relationship between dissatisfaction with Republican policy and political cynicism was found for black Democrats. They were more dissatisfied with Republican policy than any of the groups found in Table 6. This dissatisfaction is understandable, given the nature of contemporary issues. Nearly all deal with problems that impinge more directly on the lives of black people who face the constraints in the present system daily and who surely want social change more than do whites.

Furthermore, a comparison of data from 1968 and 1970 for the two seven-point issue scales used in both those studies—urban unrest and Vietnam—reveals that blacks perceived Nixon as somewhat more "hawkish" in 1970 than they did in 1968 and similarly more in favor of using "police force" in the prevention of urban unrest. Thus, even if blacks had no expectations for improvements in their social condition under the Republican administration elected in 1968, these data suggest that cynicism among blacks would still increase because they apparently perceived their situation as deteriorating under the Republicans.

A noteworthy relationship between cynicism and policy dissatisfaction was also found among Independents. As a group, they were somewhat more distrusting of the government than either Democrats or Republicans were; however, they were less cynical than black Democrats. Independents, on the whole, were less dissatisfied with Republican policy alternatives than were Democrats: 56 per cent were two or more issue points from where they placed the Republican party, but only a small number were beyond four units from the party.

Dissatisfaction with Democratic Policies. Are we to conclude from the data of Table 6 that discontent in America would be reduced by replacing the Republican administration and Republican policy alternatives with the Democratic alternative? Not necessarily. Data describing the relationship between dissatisfaction with Democratic party policies and political cynicism are presented in Table 7. The data in Table 7 clearly demonstrate that in 1970 a noteworthy proportion of the population was also dissatisfied with the policy alternatives they perceived the Democratic party as pursuing to solve the problems reflected by

contemporary issues. Nearly 60 per cent of the population fell into the two "least dissatisfied" categories (0 and 1) of Table 7, compared to 50 per cent who were equally satisfied with Republican policies. Thus, while there was somewhat less dissatisfaction, in general, with Democratic policies, a sizable fraction of the population (40 per cent) was still somewhat in disagreement with perceived Democratic policy alternatives. All of the dissatisfaction was not found among Republicans either; on the contrary, as a group they were slightly more in agreement with Democratic policies than Democrats were with Republican policies. The distributions of Independents on the measure of dissatisfaction with Democrats and Republicans are almost identical, indicating that Independents, as a group, were equally dissatisfied with both parties. This finding for Independents supports the earlier result which suggested that Independents may be cynical, to a certain extent, because they perceive no difference between the policies of the two parties. Black Democrats, in contrast, exhibited much more policy agreement with the Democratic than the Republican party. Almost 47 per cent of black Democrats thought the Democratic party would pursue policies which they themselves preferred.

Despite this relatively low degree of dissatisfaction with Democratic policies, those blacks who were most dissatisfied were definitely more cynical than those who were in complete agreement with Democratic policies. Indeed, the relationship between cynicism and dissatisfaction with Democratic policy was stronger for black Democrats than for any other group studied, even Republicans. For all the groups, except Republicans, it was somewhat weaker than found in Table 7 for the Republican party. The relationship for Republicans was almost identical in magnitude for both cases: an eta of .22 was obtained in Table 6 and .24 in Table 7.

The data in Tables 6 and 7 demonstrate emphatically that distrust of the government in 1970 was related not only to dissatisfaction with the policies of the party in control of the federal government's administration, but also to dissatisfaction with the policy alternatives identified with the Democratic party. Dissatisfaction with the Republican party was, to a certain extent, correlated with dissatisfaction with the Democratic party, thus suggesting that distrust of the government was related to *dissatisfaction with both parties.*

Data which directly test the hypothesized relationship between dissatisfaction with both parties and cynicism is presented in Table 8. The table displays a 25-celled cross-classification of the total cross-section sample determined by distance from both the Republican and Democratic parties' perceived policies. Categories 4 through 6 of the distance-from-party measures used in Tables 6 and 7 were collapsed for the construction of Table 8 so that the number of cases in the cell entries would be sufficiently large for computing the mean cynicism value for each of the groups in the data array.[32] The data entries are the mean cynicism values for the cases of the given cell. The percentage of the total population falling into the particular cell is indicated in parentheses, and these percentages total to the corner entry.

The results of Table 8 clearly demonstrate the

[32] The basic relationship between distance from both parties and cynicism is left unchanged by collapsing categories 4 through 6 of the original measures. The eta for the correlation with the full set of values was .33 as compared with .32 for the collapsed measures. Further analysis of the type presented in Table 8 but with various subgroups of the population was not possible because of the limited number of cases.

Table 7. Mean Cynicism by Distance from Democratic Party, 1970

Distance from Democratic Party	Total Cross-section		White Democrats		Black Democrats		Republicans		Independents	
	Mean[a]	%	Mean	%	Mean	%	Mean	%	Mean	%
0	2.16	(21.2)	2.17	(26.4)	2.47	(46.7)	1.82	(9.8)	2.47	(11.7)
1	2.42	(38.6)	2.43	(44.5)	3.50	(39.2)	1.89	(33.3)	2.71	(31.3)
2	2.71	(26.3)	2.71	(22.2)	4.04	(14.1)	2.42	(31.9)	3.02	(37.4)
3	2.88	(9.7)	3.43	(5.5)	[b]	—	2.50	(16.6)	3.00	(12.9)
4	3.15	(3.3)	3.70	(1.4)	[b]	—	3.03	(6.6)	3.20	(6.7)
5	3.85	(0.9)	[b]	—	[b]	—	4.00	(1.8)	[b]	—
6	[b]	—	[b]	—	[b]	—	[b]	—	[b]	—
Group Mean and Total Per cent	2.53	100.0	2.50	100.0	3.10	100.0	2.28	100.0	2.87	100.0
(*N*)	(1412)[c]		(651)		(212)[c]		(457)		(163)	
Eta	.16		.19		.31		.24		.12	

[a] Cell entries are mean cynicism values. The cynicism scale ranged from 0 = least cynical to 5 = most cynical.
[b] *N* was less than 5.
[c] To avoid the problems of weighted data, the black supplement was not included in the cross-section analysis. However, the cross-section and supplement blacks form a self-weighting sample of blacks, and supplement blacks were therefore included among black Democrats.

Table 8. Mean Cynicism by Distance From Both Parties, 1970

Distance from Republican Party	Distance from Democratic Party 0	1	2	3	4	Total
0	1.69[a] (5.9%)	1.60 (4.1)	2.50 (1.7)	1.50 (0.6)	2.10 (0.7)	1.79 (13.0%)
1	1.84 (6.1)	2.19 (18.2)	2.56 (8.4)	2.33 (3.0)	2.85 (0.9)	2.27 (36.6)
2	2.19 (4.6)	2.71 (9.8)	2.67 (11.4)	3.00 (3.1)	3.24 (1.2)	2.66 (30.1)
3	2.79 (2.4)	3.25 (4.2)	2.88 (3.5)	3.46 (2.5)	3.50 (0.6)	3.10 (13.2)
4	3.46 (2.0)	3.10 (2.1)	4.40 (1.1)	4.37 (0.9)	4.69 (1.0)	3.81 (7.1)
Total	2.16 (21.0%) Eta = .32	2.42 (38.4)	2.71 (26.1)	2.88 (10.1)	3.29 (4.4)	2.53 (100.0%) N = 1401

[a] Cell entries are mean cynicism values. The cynicism scale ranged from 0 = least to 5 = most.

degree to which distrust of the government increases as dissatisfaction with the policies of both parties increases. Those individuals who were completely satisfied with the perceived policies of the Democratic and Republican parties (cell 0,0) were the most trusting of the government (mean cynicism of 1.69), whereas those who were most dissatisfied with both parties (cell 4,4) were dramatically more cynical (mean of 4.69). It should be noted that while every column of Table 8 shows a monotonic increase in mean cynicism, every row breaks monotonicity. This substantiates what was previously observed from Tables 6 and 7, namely, that cynicism was, for the total population, somewhat more directly related to distance from the Republican party than distance from the Democratic party. The non-monotonicity of the row entries of Table 8 is greatly reduced, but is slightly apparent, when blacks and Independents are removed from the analysis. Yet for each subset analyzed, the diagonal entries of Table 8 were always strictly monotonic. Also, the difference in mean level of cynicism for those most satisfied with both parties and those most dissatisfied with both parties was always greater than the comparable difference between any of the sets of individuals most and least satisfied with the policies of any one particular party. These data thus provide strong evidence that those who feel that *neither* party offers viable solutions to contemporary social problems are among the most cynical, distrustful, and alienated citizens in the U.S. today.

Policy Dissatisfaction and Cynicism: An Explanation. How is the relationship between policy dissatisfaction with both parties and political cynicism to be explained? And how does this finding correspond to the increase in cynicism between 1964 and 1970? A possible explanation, inspired by the concepts of party identification and relative deprivation, is that Democrats would tend to be dissatisfied with Republican policies, and thereby more distrusting of the government, because they do not believe that Republican policies benefit them. This party-based interpretation would, however, lead to the prediction of no correlation between cynicism and Democratic policies, a prediction obviously not supported by the data. Another interpretation which would seem theoretically reasonable, but similarly unsupported by these data, is that a negligible relationship between cynicism and dissatisfaction with Republican policy would be found for Democrats because their expectations during a Republican administration would be low and policy dissatisfaction would thus not lead to increased cynicism. Perhaps the reason these explanations do not fit the data is that contemporary social issues are not closely related to party identification, and what appears confusing when approached from a party focus becomes more meaningful from another vantage point.

An investigation of policy dissatisfaction for "cynics of the left" and "cynics of the right" produces a partial reduction in the confusion and a clearer explanation. It appears that the Democratic "cynics of the left" (N = 122) were dissatisfied with the Democratic and Republican parties because, to a certain extent, they perceived both of them as being too far to the right: 38 per cent

placed the Democratic party further to the right than their own policy preference and nearly 96 per cent saw the Republican party as more conservative on the issues than they were themselves. Their average distance from the Democratic party was 1.4, from the Republican party, 3.2, thus illustrating a greater degree of discontent with Republican than Democratic policies. The Democratic "cynics of the right" ($N=138$), however, were dissatisfied with *both* the Republican and Democratic policies equally: 74 per cent perceived both parties as further left than their own policy preference. On the average, the Democratic "cynics of the right" placed the Democratic party 1.9 units away from their own policy position, while their mean distance from the Republican party was 2.4.

That dissatisfaction with Democratic and Republican policies coincides so closely for Democratic "cynics of the right," unlike that for those on the left, may partially be explained by the possibility that they were less well informed, as is suggested by the fact that they were not as well educated as "cynics of the left." The importance of this explanation is somewhat reduced, however, by the finding that Republican "cynics of the left," who were, on the whole, very well educated, were also equally dissatisfied with both parties. Their average distance from the Democratic party was 1.8, compared to 1.6 from the Republican party. The Republican "cynics of the left" perceived Republican party policies as somewhat more conservative than their own preferences but very centrist; on the average, 61 per cent placed the party at point 4 on the issues. Surprisingly, they perceived Democratic policies as somewhat more to the right than Republican policies. Unfortunately, there were only 25 respondents so classified, thus making inferences about Republican "cynics of the left" rather tenuous. In contrast, Republican "cynics of the right" ($N=102$) were more dissatisfied with Democratic party policies (average distance of 2.7) which they perceived as being further left than Republican policies. Nevertheless, 57 per cent placed the Republican party further to the left (average distance of 1.8) than their own policy preference, implying a distrust for the government because of too liberal administration policies.

In short, both Democratic and Republican "cynics of the right" perceived partisan solutions to contemporary social problems as too liberal. It is important to add that individuals preferring centrist policy alternatives, regardless of whether "centrist" is defined as points 3 to 5 or only 4 on the scale, were proportionately more likely to perceive both the parties in the center; hence they were less dissatisfied and correspondingly less alienated. Besides demonstrating which Democratic and Republican subgroups were dissatisfied more with one party than the other, or with both equally, these data support a reformulated explanation of how policy dissatisfaction corresponds to the recent increase in political distrust.

When approached from the left-right cynicism perspective, the data strongly imply that the increased distrust of government in 1970 was for some the result of a combination of unfulfilled expectations, and for others, the perception that their situation, which was already one of deprivation and discrimination, had actually worsened relative to that of others. For example, in 1968 Richard Nixon promised to end the war, but respondents in that year's study who perceived him as tending toward a policy of complete military victory outnumbered, by 2 to 1, those who saw him as favoring withdrawal. By 1970, U.S. involvement in Vietnam had not ended, and Vietnamization—a centrist approach to the war—had been declared the official policy of the Republican administration. Furthermore, a larger proportion (34 per cent) of the "cynics of the right," who on the whole were very "hawkish," perceived Nixon as favoring withdrawal (points 1–3 on the Vietnam issue scale) in 1970 than did in 1968 (23 per cent). In contrast, the most "dovish" of the "doves," the blacks, perceived Nixon, despite his promises to end the war, as more in favor of a complete military victory in 1970 (52 per cent placed him at points 5–7) than they did in 1968 (42 per cent). Thus, the expectations of both "hawks" and "doves," regardless of their party identification, had not been met, resulting in a decline in confidence for both groups.

The failure of the Republican administration to meet the expectations of the Democratic "cynics of the right" may have led them to the view that 'the Republicans are just as bad as the Democrats." Such an interpretation would coincide well with the close correspondence between dissatisfaction with Democratic policy and dissatisfaction with Republican policy that was found earlier for Democratic "cynics of the right." It would also explain why Republican "cynics of the right" were dissatisfied with Republican policies. However, their party identification, which was somewhat stronger than that of the Democratic "cynics of the right," would be expected to keep them from perceiving Republican and Democratic policies as identical; exactly what the analysis demonstrates.

Similarly, the combined effects of party identification, unmet expectations, and the belief that social and economic conditions had deteriorated under the Republican administration can also explain increased cynicism among those on the left. Further, the relationship found between distrust of the government and dissatisfaction with the policies of both parties suggests that discontent in

1970 was related to an earlier dissatisfaction with the policies of the previous (Democratic) administration. Indeed, analyses from 1964 to 1968 support this contention and reveal a relationship between policy dissatisfaction and cynicism for both Democrats and Republicans.

In summary, it has been demonstrated that support of the federal government decreased substantially between 1964 and 1970. Data presented suggested that this increased distrust of the government was partially related to changing attitudes on the issues of racial integration and U.S. involvement in the Vietnam war. A curvilinear relationship was found to exist between policy preference on these issues (as well as on a wide range of other contemporary social issues) and political cynicism, with those favoring centrist policies being less cynical than those preferring noncentrist opposing policy alternatives. This complex relationship between cynicism and policy preference was later explained by dissatisfaction with the policies of both political parties.

The dissatisfied noncentrists formed two highly polarized and very different types of cynics, "cynics of the left" and "cynics of the right." It was established that "cynics of the left" preferred policies bringing social change, while those on the right favored social control policies. The data suggested that increased cynicism could be explained for "cynics of the left" in terms of frustrations arising out of unmet political expectations for social change, an impatient desire for the alteration of existing policies in order to solve social problems more rapidly, and the belief that the policies of the Republican administration, elected in 1968, had brought a deterioration in the social and economic position of certain groups in society. Increased cynicism on the right has similarly been explained in terms of unmet expectations but for greater social control (a hoped-for Vietnam victory, a slowing of racial integration, a more forceful stand on law and order), and fear arising out of what were perceived as the "too liberal" policies of both parties which threatened to negate their values, as well as undermine their social and economic position.

Finally, the data analysis suggests that the individual evaluates the policies of the parties with respect to his own preference, and if he is dissatisfied with those policies it is more likely that he will feel alienated from the political system. There is, however, nothing *inherent* in his own policy preference that would result in his being less trusting of the government. If an individual takes a centrist position on an issue and perceives the policies of the parties to be in disagreement with his desires and expectations, he is as likely to be distrusting of the government as the individual who prefers a more extreme policy position. But, since public opinion on the issues is, in fact, polarized and because the parties are perceived by a sizable proportion of the population to offer centrist policy alternatives as solutions to contemporary problems,[23] it follows that those who prefer noncentrist policies are more dissatisfied and therefore the most alienated.

Conclusion

What conclusions can be drawn from these research results, and what implications for change do they suggest?

This study demonstrates that the widespread discontent prevalent in the U.S. today arises, in part, out of dissatisfaction with the policy alternatives that have been offered as solutions to contemporary problems. The findings strongly suggest, moreover, that policy alternatives more acceptable to the total population will be exceedingly difficult to discover in the future because of the existing degree of issue polarization. This polarization implies that, on the one hand, there is growing discontent among some individuals because of an unfulfilled desire for social change, while, on the other hand, there is an even larger group of alienated individuals who are fearful of change. Those desirous of change, and particularly blacks—since the contemporary problems so directly affect their lives—are those most in need of the use of political influence to obtain that change; however, they perceive the legitimate means of attaining such influence as ineffective. This suggests, for blacks at least, that other means, whether peaceful or violent, legal or illegal, will have to be found to bring about the desired modifications. A move on the part of the administration to make its policies more responsive to the needs of blacks and other minority groups would undoubtedly reduce cynicism among those on the left, but, as the data suggest, this would at the same time increase the size of the cynical group on the right and probably intensify its distrust. The current political dilemma arises out of such issue polarization and also out of the difficulties implicit in maintaining government responsiveness and providing for orderly change without the occurrence of intense political conflict under such conditions. Discontent can be functional for a political system if it acts as a catalyst for orderly change, but when the normal channels are perceived as ineffective, the probability that the conflict may burst forth in the form of extra-legal behavior increases.

One goal of the U.S. political system is to contain protest and rage within the electoral process,

[23] The proportion of the total population perceiving the Democratic party as taking a centrist position (#'s 3–5) on the eight issues ranged from 51.9 to 66.5 per cent. In comparison, 60.7 to 68.9 per cent placed the Republican party in positions # 3–5 on the eight issues.

and parties generally provide a vehicle for such protest. However, investigation of the data has demonstrated that people lack confidence in the ability of the existing parties to bring about responsive government. The great dissatisfaction with the policies of both parties implies that conditions are highly conducive to party realignment and reformation or a third party movement. What appears necessary to re-establish confidence in the parties is a wave of populism and party reforms which will allow those who normally lack access to legitimate power, but want change, to bring about desired modifications. Whether the current reforms of the primary system and the Democratic party will have a substantial impact on confidence in parties remains to be seen. The results of the study suggest that we are in a critical period of change in the attitude of political trust. There are numerous indications that the distrust of the government has increased beyond the 1970 level. In a system as stable as that in the U.S., however, it is difficult to conceive of the trend in trust continuing to decline at the same rate that it has from 1964 to 1970. A slowing in the rate of decline would certainly have been expected to accompany a settlement of the Vietnam conflict, improvement in the economic condition of the country, the easing of racial tensions, and a reduction in turmoil in schools and on university campuses. Indeed, between 1970 and 1972 the decline in political trust was arrested; some segments of the population were even displaying increased trust in the government by the fall of 1972.[34] The Watergate revelations, seemingly unending political scandals in the Nixon administration, continuing inflation, an energy crisis, and a near-collapse in the president's credibility have, however, rendered short-lived any hopes for renewed trust in the near future.

What happens if the policies of the administration elected in 1972 continue to bring no reduction in dissatisfaction? Likewise, what happens if present policy is maintained in the future? The trends in the data suggest that trust of the government would continue to decline, increasing the difficulty for leaders to make binding political decisions, as well as raising the probability of the occurrence of radical political change. This would again seem to be particularly true for blacks, for whom the very means of legitimate political control and change are now on trial. Further increases in alienation would presumably bring into question the very philosophy and goals of the political system, as well as the viability of the political community itself, and a "desire for political separation may develop."[35]

The importance of political trust is often couched in terms of a discussion of social control or the necessity for influence. It is argued that without enjoying sufficient political trust, political leaders cannot freely make decisions and commit resources to attain collective goals. Likewise, as cynicism increases, the desire for change increases; if confronted with an immobile leadership, this can lead to desire for *radical* change. Both of these concerns have important implications for the political system and merit attention. Such analysis, however, almost smacks of an insensitivity to the questions about the quality of life which must have led to such discontent in the first place. The use of political trust as a simple barometer of satisfaction with the political system is too often ignored or buried beneath a series of esoteric arguments and theoretical concerns. The disenchantment and dissatisfaction of individuals who feel politically inefficacious and cynical about the government is real, and it arises out of a reaction to real conditions of life. The use of political cynicism as an indicator of the quality of American life is, in and of itself, an important justification for its continued study. It is hoped that continued study will bring a better understanding of those conditions which lead to political cynicism and the basic information from which solutions to the problems may be found.

The distribution of policy preferences on contemporary issues, and the curvilinear relationship between preferences and discontent implies that these solutions will be difficult to come by. It is, however, important that these problems be solved, particularly since experts agree that the three major types of contemporary civil violence—urban riots, campus disturbances and crime—all flow from the wellsprings of unsolved social problems. Solutions to these problems necessitate change, but if alterations of the current social system are to be forthcoming, the resistance and fear which often deter change must be dealt with. The findings of this study suggest that a reduction of alienation among those favoring change can be brought about by creative and constructive action in domestic areas that will benefit the majority of people and diminish feelings of frustration and relative deprivation. But the caveat must be repeated that these policies need to be designed to solve existing social problems without further alienating a substantial proportion of the population. The vast opinion leadership potential of the president would have to be fully exploited if this strategy were to be realized in the current climate of opinion polarization. A president initially viewed as a moderate or centrist ought to have a

[34] Between 1970 and 1972 the cynicism PDI value for whites increased from 0 to 7, while trust continued to decline for blacks. The change in cynicism between 1970 and 1972 for blacks was, however, much slower than it had been between 1968 and 1970, going from a 1970 PDI value of −31 to −39 in 1972.

[35] Gamson, *Power and Discontent*, p. 52.

much better chance of mobilizing broad support for reform policies under these circumstances than a president who is viewed as a noncentrist. However, the leadership role cannot rest with the president alone; the decrease of discontent among those preferring social control policies would require that constructive programs of social change be administered by leaders not only sensitive to the fears evoked by change but also capable of dealing with the resulting resistance to such programs. These programs would require educating the public so that it will better understand the need for social change and the benefits which would evolve from such change. Whether the necessary modifications of the social and political system and the style of leadership necessary to bring about these changes with minimal conflict will be forthcoming is, again, yet to be realized. The results of this study, however, clearly imply that current discontent in America will not be reduced with a continuation of centrist policies, or centrist politics, especially if such politics include numerous promises which give rise to expectations that are never fulfilled.

APPENDIX

Issue Questions for 1970

Urban Unrest

[a]1. There is much discussion about the best way to deal with the problem of urban unrest and rioting. Some say it is more important to use all available force to maintain law and order—no matter what results. Others say it is more important to correct the problems of poverty and unemployment that give rise to the disturbances. And, of course, other people have opinions in between. Suppose the people who stress doing more about the problems of poverty and unemployment are at one end of this scale—at point number 1. And suppose the people who stress the use of force are at the other end—at point number 7.

Solve problems of poverty and unemployment — *Use all available force*

1 2 3 4 5 6 7

a. Where would you place yourself on this scale? _____
b. Where would you place the Democratic party? _____
c. Where would you place the Republican party? _____
d. (Where would you place) Richard Nixon? _____
e. (Where would you place) Edmund Muskie? _____
f. (Where would you place) George Wallace? _____

[a] Only these two items were used in the 1968 election study.

Vietnam

[a]2. There is much talk about "hawks" and "doves" in connection with Vietnam, and considerable disagreement as to what action the United States should take in Vietnam. Some people think we should do everything necessary to win a complete military victory, no matter what results. Some people think we should withdraw completely from Vietnam right now, no matter what results. And, of course, other people have opinions somewhere between these two extreme positions. Suppose the people who support an immediate withdrawal are at one end of this scale at point number 1. And suppose the people who support a complete military victory are at the other end of the scale at point number 7.

Campus Demonstrations

3. Some people are pretty upset about rioting and disturbances on college campuses and in high schools. Some feel sympathetic with the students and faculty who take part in these disturbances. Others think the schools should use police and the national guard to prevent or stop disturbances. And others fall somewhere between these extremes.

Government Aid to Minority Groups

4. Some people feel that the Government in Washington should make every possible effort to improve the social and economic position of Negroes and other minority groups. Others feel that the Government should not make any special effort to help minority peoples but they should be expected to help themselves.

Inflation

5. There is a great deal of talk these days about rising prices and the cost of living in general. Some feel that the problem of inflation is temporary and that no Government action is necessary. Others say the Government must do everything possible to combat the problem of inflation immediately or it will get worse.

Protecting Rights of Accused

6. Some people are primarily concerned with doing everything possible to protect the legal rights of those accused of committing crimes. Others feel that it is more important to stop criminal activity even at the risk of reducing the rights of the accused.

Ecological Pollution

7. There are many sources of air and water pollution; one of them is private industry. Some say the Government should force private industry to stop its polluting. Others believe industries should be left alone to handle these matters in their own way.

Government Health Insurance

8. There is much concern about the rapid rise in medical and hospital costs. Some feel there should be a government health insurance plan which would cover all medical and hospital expenses. Others feel that medical expenses should be paid by individuals and through private insurance like Blue Cross.

Comment: The Political Relevance of Trust in Government*

JACK CITRIN
University of California, Berkeley

"In God We Trust: Everyone Else Pays Cash." America's political leaders should not pretend to godliness; no one will be fooled. According to prestigious biennial national surveys, the government's credit rating has steadily declined as a result of a disastrous foreign investment and growing consumer resistance to its "line" of products. Neither the country's present management nor its most prominent rivals inspire public confidence. How, then, can the political system rebuild its depleted reserves of political trust, the basis of future growth and stability? Will "one good season," better advertising, new blood in the boardroom or product innovation be sufficient? Or is a drastic restructuring of the regime's organization and operating procedures the only alternative to liquidation?

A Political Theory of Political Trust

Arthur Miller's article, "Political Issues and Trust in Government: 1964–70" makes an important contribution to our understanding of the sharp increase in political cynicism among the American public. Miller evokes the language of the corporation balance-sheet and the imagery of *Executive Suite* by suggesting that the cumulative outcome of exchanges between political authorities on the one hand and citizens on the other determines the level of public trust in government. Political elites "produce" policies; in exchange, they receive trust from citizens satisfied with these policies and cynicism from those who are disappointed. Since Miller defines both policy satisfaction and political trust in attitudinal terms, the exchange transactions he records are purely psychological in nature. Operationally, dissatisfied respondents are those whose own policy preferences are discrepant with their perceptions of the positions advocated by the party controlling the presidency. Miller's findings confirm the hypothesis that the greater the perceived discrepancy the less likely one is to express a generalized sense of trust in government. This conclusion, of course, constitutes further evidence for one of social science's most familiar generalizations: We tend to trust and like those who agree with us.

While the reaffirmation of an eternal verity is comforting, the significance of Miller's analysis lies elsewhere. In my view, "Political Issues and Trust in Government: 1964–70" argues that the performance of political officeholders and institutions determines their legitimacy. To be sure, an individual's ideological orientation and policy preferences influence his evaluations of governmental behavior, but such mediating effects are quite consistent with a theoretical emphasis on political events and experiences as the main source of public support for the political system. Thus, Miller interprets the sharp decline in the aggregate level of trust in government as the result of increased discontent with putatively unsuccessful "centrist" policies.

An emphasis on political factors as determinants of attitudes toward the political system represents a departure from earlier theoretical perspectives that stressed the causal influence of social background or personality.[1] But analysis of the well-known election studies conducted by the Survey Research Center and Center for Political Studies of the University of Michigan indicates that social background variables are neither strongly nor consistently correlated with political trust, as measured by the Trust in Government scale.[2] For example, in 1964 blacks were more

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[1] Other studies that support the conclusion that political factors such as ideological orientations, evaluations of the performance of governmental institutions, and responses to personal contacts with political authorities are important causes of political disaffection are Joel Aberbach and Jack Walker, "Political Trust and Racial Ideology," *American Political Science Review*, 64, (December, 1970), 1199–1219, Edward N. Muller, "The Role of Political Distrust in a Theory of Support and Opposition to the Regime," unpublished paper delivered at the Madison, Wisconsin Conference on Public Support for the Political System, Aug. 13–17, 1973, Jack Citrin, "Political Disaffection in America: 1958-68" (Ph.D. dissertation, University of California, Berkeley, Jan., 1972); and Jack Citrin, Herbert McClosky, J. Merrill Shanks, and Paul M. Sniderman, "Personal and Political Sources of Political Alienation," *British Journal of Political Science*, Vol. 4 (September, 1974).

[2] On this point see Citrin "Political Disaffection in America," chap. 4, and Arthur Miller, Thad Brown,

trusting than whites; people earning less than $5000 a year were more trusting than those earning more than $15,000; and manual workers were more trusting than businessmen. In 1970, and again in 1972, these relationships were reversed. Moreover, the decline in political trust documented by Miller cannot be attributed to changes in the social composition of the American public; virtually every social group became more politically cynical between 1964 and 1972. Turning to the psychological explanations, although measures of personal competence and "trust in people" do correlate positively, albeit rather weakly, with feelings of political trust,[3] we cannot conclude that the recent erosion of public confidence in the political process reflects changes in these psychological dispositions. A comparison of the responses of the 1964 and 1970 election study samples to the questions used to measure ego strength and trust in other people reveals no systematic decrease in the number of "competent" or "trusting" answers. In sum, while social background and personality factors doubtless have some influence on how one evaluates the political order, Miller's identification of the strong and independent impact of political attitudes and experiences is convincing.

This comment accepts Miller's main conclusion that policy-related discontent is a source of political cynicism and proceeds to focus on three issues: (1) The meaning of political trust as measured by the Trust in Government scale; (2) The independent impact, if any, of *attitudes* of political cynicism on political *actions* at the individual level; and (3) A re-examination of the claim that a continuation of "centrist" policies will inhibit the restoration of public confidence in the political process. In pursuing these questions, my principal interest is in the implications of Miller's analysis for the study of political change in contemporary America.

The Meaning of Declining Trust in Government

Miller interprets the persistent decline in the levels of political trust and political efficacy to indicate that "a situation of widespread, basic discontent and political alienation" is the existing condition in the U.S. today (p. 951). He refers to "hostility toward political and social leaders, the institutions of government, and the regime as a whole" and of a "negative orientation toward the political *system*" (p. 951, italics mine). According to Miller, large segments of the American public have unfulfilled needs and blame the government for the unsatisfactory quality of life they are experiencing. Moreover, the high level of political discontent has endured so long (1966–70!) that simply voting out the incumbent leaders will not restore confidence in the political system. The persistence of widespread mistrust of government suggests that "the normal means by which conflict is managed in the political system are not fully operative" (p. 000) and, therefore, that strong potential for radical change exists. In sum, utilitarian as well as expressive motives underlie feelings of attachment to the political system, and the failure of government policy to "fulfill needs" or "meet expectations" has resulted in the erosion of political legitimacy in the United States.

These conclusions rest on the assumption that the Trust in Government scale measures alienation from the political regime rather than mere disapproval of incumbent political leaders. Unfavorable evaluations of the trustworthiness, competence, and responsiveness of "the government," "people running the government," and "public officials," the attitude-objects of the standard political trust and political efficacy items, increased substantially between 1964 and 1970. Allegiance to the political system, however, does not preclude criticism of specific policies, authorities, or institutions; many people readily combine intense patriotic sentiments with cynicism about politicians. Moreover, opinions about incumbents inevitably color evaluations of political roles or institutions, and the tendency of respondents in a survey to perceive "the government in general" in terms of the incumbent president and his administration probably is strongest in an electoral context, when partisan cues are at a maximum. Thus, the political implications of Miller's analysis depends on the discriminant validity of the Trust in Government scale as an indicator of attitudes toward the political regime.[4]

The level of political disaffection observed varies with the focus (or "object") of the questions used to measure it. For example, the 1972 National Election Study indicates that 47 per cent of the general public believed that "you can trust the government in Washington to do what is right only some or none of the time" and that 44.3 per cent felt that "quite a few of the people running the government don't seem to know what they're

and Alden Raine, "Social Conflict and Political Estrangement," unpublished paper delivered at the 1973 annual meetings of the Midwest Political Science Association.

[3] For example, in 1964, the association between personal trust and political trust was .21 (tau-b); in 1968 it was .20 (tau-b). Aberbach and Walker report a relationship of .16 (gamma) for their Detroit sample.

[4] Throughout this paper I use the terms political authorities, regime, and community in the sense of David Easton, *A Systems Analysis of Political Life* (New York, John Wiley and Sons, 1965). In addition, I use the term political system as a synonym of regime.

Table 1. Political Trust and Attitudes toward the American Form of Government,* 1972 (percentages down)

	Trust in Government			
	Low (n=419)	Middle (n=373)	High (n=224)	Total (n=1016)
Pride in Government				
I am proud of many things about our form of gov't.	74.3	91.8	97.9	86.0
I can't find much about our form of gov't to be proud of.	25.8	8.2	2.1	14.0
Change our Form of Government?**				
Keep our form of gov't as is	43.2	63.3	80.8	58.8
Some Change needed	31.7	27.6	14.3	26.4
Big Change needed	25.1	9.1	4.9	14.7

* Access to these materials was provided by Merrill Shanks.

** The exact wording of this question is: "Some people believe that change in our whole form of government is needed to solve the problems facing our country, while others feel no real change is necessary. Do you think . . . ?"

doing." But only 13.5 per cent said "I can't find much in our form of government to be proud of." And 54.9 per cent believed that the existing political system should remain "pretty much as it is" whereas only 14 per cent advocated "a big change in our whole form of government."[5] Thus a diffuse sense of pride in and support for the ongoing "form of government" can coexist with widespread public cynicism about "the government in Washington" and the people "running" it.

The questions about pride in existing institutional arrangements and the desirability of changing them appear to be on their face more valid indicators of a basic attachment to the political regime than most of the items comprising the Trust in Government scale. To be sure, the manifest content of attitude items cannot be the sole criterion of their validity. A process of construct validation that focuses on the relationships among responses to a proposed attitudinal measure and theoretically relevant attitudes and actions constitutes a more definitive test.[6] Nevertheless, the cynical responses to the CPS political trust items are hardly extreme. To believe that the government wastes "a lot" of money, can be trusted to "do what is right only some of the time," and includes "quite a few" people who are "crooked" or "don't know what they're doing" need not bespeak a deep-seated hostility toward the political system at the regime or community levels. As Table 1 shows, in the 1972 election study 74 per cent of those who score "low" on the Trust in Government scale express pride in "our form of government."[7] And 43 per cent of the political cynics would like existing institutional arrangements to remain unchanged compared to 25 per cent who advocate a "big change in our form of government." This strongly suggests that many political cynics focus their dissatisfaction on incumbent authorities rather than systemic values and processes.

Increasing discontent with current government policy undoubtedly has contributed to the growth of political cynicism, but the decline in "trusting" responses to the Trust in Government items may also reflect a higher level of political sophistication and realism among the general public. In addition, the current *zeitgeist*, which legitimizes, even encourages, the expression of anti-political rhetoric, makes it fashionable to denigrate politicians and to criticize established institutions. As a result, the burgeoning ranks of the politically cynical may include many who are verbalizing a casual and ritualistic negativism rather than an enduring sense of estrangement that influences their beliefs and actions. And if the meaning of identical responses to the Trust in Government items change as political and cultural contexts vary, it may be inappropriate to use the same items or cutting-points to measure political dis-

[5] These figures are computed from the marginal distributions provided in the ICPR 1972 Election Study Codebook. "No answer" responses are omitted from the total base. The last two questions are restricted variables made available by J. Merrill Shanks.

[6] See the extended discussion of this point in Citrin et al., "Personal and Political Sources of Political Alienation."

[7] Unless explicitly noted, all the tables reported and all figures in the text derive from my own analysis of the 1964, 1968, 1970, and 1972 election study data made available by the Inter-University Consortium for Political Research. My scoring of the Trust in Government scale employs the same item dichotomies as Miller does, although our handling of missing data varies slightly and I construct scale scores by simply summing responses rather than using a formal Guttman scoring procedure.

affection at different points in time, regardless of whether these continue to form a Guttman scale.[8]

Table 1 implies that many "political cynics" do not repudiate the political *regime;* the data reported in Table 2 point directly to a connection between lack of political trust and negative evaluations of the *incumbent* national administration. For example, in 1968 "trusting" scores on the Trust in Government scale were associated with approval of "how President Johnson is handling his job," and in 1972 political trust covaried with approval of President Nixon's performance. With a Democrat in the White House in 1964, strong Republicans formed the most cynical group on the party identification continuum, and strong Democrats were the most likely to express trust in government; during the Nixon presidency the positions of Republican and Democratic partisans were reversed. It is difficult to reconcile this pattern of attitude change among those most committed to the major political parties with the claim that the Trust in Government scale measures alienation from the political system. But if cynical responses record opposition to the "ins" as well as or instead of rejection of the political regime, the shifting relationship between party identification and political trust is to be expected. After all, in the American context partisans of the "out" party usually believe that there are alternatives to the incumbent president whose advent to power would assuage their present sense of disenchantment.

The *overall* relationship between party identification and trust in govenment (tau$-b=-.16$ in 1964, $-.09$ in 1968, and .12 in 1972, with party identification scored in the Republican direction) is relatively weak, and how someone votes for president (Goldwater in 1964, Wallace in 1968, and McGovern in 1972) is a better predictor of political cynicism than his party identification. In an era of strong ideological focus and declining party loyalty, apparently, the personal qualities and policy orientations of the presidential candidates, rather than their party labels, cue cynical (or trusting) responses.

Table 3 provides additional evidence that trust in government and support for the incumbent national administration are correlated, whereas political cynicism reflects a sense of identification with critics of the status quo. Positive evaluations of the incumbent president and vice-president—Johnson and Humphrey in 1968, Nixon and Agnew in 1970 and 1972—were more widespread among the politically trusting than the politically cynical. By contrast, the cynics were more likely to view positively politicians who themselves attacked the underlying assumptions of ongoing public policies, whether from the right or left, George Wallace in 1968 and George McGovern in 1972. These data confirm Miller's analysis of the curvilinear relationship between policy preferences and trust in government: respondents with either "leftist" or "rightist" candidate preferences are disproportionately likely to feel politically cynical.

Quite predictably, "leftist" and "rightist" cynics diverged sharply in their evaluations of presidential candidates and of groups such as civil rights leaders, black militants, student protesters, rock-festival goers, or the police. *But the responses of "leftists" and "rightists" who trusted the government differed in much the same way.* Thus, whether or not the feeling thermometer ratings of the politically cynical and trusting will diverge depends on the relative balance of ideological predispositions within these groups. (It also requires that the group or candidate being rated somehow engages these ideological predispositions.) For example, between 1970 and 1972 the predominance of conservatives among the politically trusting grew, whereas the political cynics, as a group, became more liberal. As a result, neither a negative relationship between political trust and a favorable image of George Wallace nor a positive association between trust in government and "warm" feelings about civil rights leaders appeared in the 1972 sample.

The close connection between disapproval of the incumbent president and cynical scores on the Trust in Government scale does not establish definitively that the "object" of this attitude measure is the current administration rather than the overall political system. After all, someone who is deeply estranged from the underlying values of the political system and rejects its constitutional order is unlikely to view the incumbent authorities favorably. At a minimum, however, the Trust in Government scale fails to discriminate between the politically alienated and those who mistrust particular leaders or politicians as a class without repudiating regime values or institutions. And to the extent that today a cynical score consitutes a surrogate measure of partisanship or anti-Nixon sentiment, a new face in the Oval office might well lead to higher levels of trust in government.

One difficulty with my own argument, of course, is that trust in government has continued to decline despite fluctuations in the level of presidential popularity. In 1972, 57 per cent of those expressing approval of "how President Nixon is handling his job" had cynical scores on the Trust in Government scale. One possible explanation for the coexistence of widespread support

[8] This conclusion has disturbing implications for analysts of time-series data. Again, the appropriate safeguards lie in conceiving of the construct validation process as a continuous one.

Table 2. Political Trust and Support for Incumbents, 1964–72
(percentages down)

A) Approval of Incumbent President's Performance
1968 Sample:

	Quality of Johnson's Performance					
	Very Good [n=137]	Good [n=372]	Fair [n=489]	Poor [n=172]	Very Poor [n=107]	Total [n=1314]
Trust in Government						
Low	24.1	20.7	32.7	47.7	56.1	32.6
Middle	40.9	47.3	46.8	38.4	35.5	44.1
High	35.0	32.0	20.4	14.0	8.4	23.2

1972 Sample:

	Evaluations of Nixon's Performance*		
	Approve [n=746]	Disapprove [n=291]	Total [n=1037]
Trust in Government			
Low	34.7	67.0	42.8
Middle	39.5	24.1	36.2
High	25.7	8.9	21.0

* Richard Brody provided me access to this material.

B) Political Trust and Party Identification, 1964–72

	Strong Democrat	Weak Democrat	Independent Democrat	Independent	Independent Republican	Weak Republican	Strong Republican	Total
Trust in Government								
1964: Low	16.5	19.8	17.3	29.1	31.2	26.6	37.8	22.7
Middle	33.1	39.4	37.0	39.8	38.7	40.1	37.2	37.2
High	50.4	40.8	45.7	31.1	30.0	33.3	25.0	40.0
	[n=393]	[n=358]	[n=127]	[n=103]	[n=80]	[n=142]	[n=156]	[n=1409]
1968: Low	24.8	33.2	31.2	49.2	32.2	30.9	33.8	32.6
Middle	42.9	42.6	43.7	36.4	50.4	46.6	53.4	44.1
High	32.3	24.2	25.0	14.4	17.4	22.5	12.8	23.2
	[n=266]	[n=331]	[n=128]	[n=132]	[n=115]	[n=191]	[n=133]	[n=1296]
1970: Low	44.4	43.4	44.1	46.8	38.9	37.9	34.3	42.3
Middle	35.9	37.2	32.3	34.1	47.3	40.0	39.9	37.4
High	19.7	19.4	23.6	19.0	13.7	22.1	25.9	20.4
	[n=304]	[n=366]	[n=161]	[n=205]	[n=131]	[n=235]	[n=143]	[n=1545]
1972: Low	53.7	44.0	51.5	45.2	33.5	35.1	32.4	42.8
Middle	31.2	36.8	32.8	35.1	41.2	36.7	40.2	36.2
High	15.1	19.2	15.8	19.7	25.3	28.2	27.5	21.0
	[n=324]	[n=582]	[n=241]	[n=279]	[n=245]	[n=319]	[n=244]	[n=2234]

C) Political Trust and Presidential Vote: 1964–1972

	Trust in Government			
	Low	Middle	High	Total
1964 Vote:	[n=240]	[n=428]	[n=434]	[n=1102]
Johnson	42.1	64.7	84.3	67.5
Goldwater	57.9	35.3	15.7	32.5
1968 Vote:	[n=292]	[n=444]	[n=226]	[n=962]
Nixon	46.6	49.8	43.8	48.0
Humphrey	32.2	41.2	54.4	40.6
Wallace	21.2	9.0	1.8	11.4
1972 Vote:	[n=646]	[n=600]	[n=347]	[n=1583]
Nixon	51.9	69.7	76.7	63.9
McGovern	47.2	29.5	23.3	35.4

Table 3. Political Trust and Affect Toward Candidates and Selected Groups: 1968–1972*

Trust in Government		Nixon	Agnew	McGovern	Johnson	Humphrey	Wallace	Policemen	The Military	Civil Rights Leaders
1968:	Low	63	49		50	54	39	71	65	
	Middle	67	51		59	62	30	71	65	
	High	68	51		68	70	25	73	67	
		[n=1278]	[n=1191]		[n=1283]	[n=1281]	[n=1270]	[n=888]	[n=925]	
1970:	Low	53	43	45		47	35	77	71	42
	Middle	61	47	46		52	28	80	73	48
	High	65	50	46		53	30	80	74	50
		[n=1531]	[n=1485]	[n=983]		[n=1506]	[n=1465]	[n=1541]	[n=1510]	[n=1492]
1972:	Low	56	48	53		63	50	73	67	42
	Middle	70	58	45		53	51	76	71	40
	High	76	63	44		54	53	79	72	42
		[n=2136]	[n=2063]	[n=2112]		[n=2174]	[n=2097]	[n=2115]	[n=2074]	[n=2040]

* Figures given are mean ratings on feeling thermometer; 0 = very cold feeling, 97 = very warm feeling. Blank spaces in the table mean that this "object" was not rated in the year in question.

for the President and a pervasive mistrust of "government in general" is that for many respondents approval of Nixon's performance represented a short-term rationalization of an anti-McGovern vote. One possible explanation for the coexistence of widespread support for the President and a pervasive mistrust of "government in general" is that for many respondents approval of Nixon's performance represented a short-term rationalization of an anti-McGovern vote. The finding that pro-Nixon Democrats were more likely than their Republican counterparts to be politically cynical supports this interpretation. An alternative, but not mutually exclusive, explanation is that many cynical responses are ritualistic rather than genuine. The tendency to demean politics is a well-established cultural tradition in America. Even Lincoln described politicians as "a set of men who have interests aside from the interests of the people and who, to say the most of them, are, taken as a class, at least one long step removed from honesty."[9] In the vocabulary of contemporary American politics, government and politics are "dirty" words that convey contemptuous and derisive feelings. Yet precisely because the political culture sanctions expressions of political cynicism, their consequences may be purely symbolic. According to this point of view, to agree verbally that many people "running the government" are corrupt, incompetent, or untrustworthy is like shouting "Kill the umpire!" at a baseball game. Bloodthirsty rhetoric threatens neither the life expectancy of umpires nor the future of the national pastime. Thus, a diffuse opposition to the "government in general" does not preclude support for its authority in specific instances. For example, the 1972 election study reveals that while 56.1 per cent of the public agreed that "the government in Washington is getting too powerful for the good of the country and the individual person," an overwhelming majority also favored "total government action against inflation" and felt that "government should force private industry to stop its polluting."

To summarize, low scorers on the Trust in Government scale appear to form a heterogeneous group. They include "ritualistic cynics" and partisans of the "outs" as well as respondents who see no viable alternative to the incumbent authorities and reject the ongoing constitutional order. If, as seems likely, ritualistic cynics, partisan cynics, and alienated cynics differ in their backgrounds, attitudes, and modes of political participation, the policy implications of declining political trust will vary according to the relative contribution of each "type" of political cynic to this aggregate shift. "Political Issues and Trust in Government: 1964–70" provides no basis for disaggregating the politically cynical respondents or for identifying the correlates of the relevant "types" of cynic. Thus, the meaning of recent increases in the level of political cynicism remains ambiguous, and to decisively conclude that there exists widespread support for radical political change or pervasive alienation from the political *system* is premature, if not misleading.

Political Cynicism and Political Action

An investigation of the behavioral consequences of political cynicism serves two purposes. First, inferences about the impact of declining aggregate levels of trust in government require knowledge of whether and how the politically cynical and trusting differ in their actions. And methodologically, the validity of the Trust in Government scale as an indicator of alienation from the political regime depends ultimately on the emergence of theoretically predicted differences in the be-

[9] Quoted in Richard N. Current, *The Lincoln Nobody Knows* (New York, McGraw Hill, 1958), p. 187.

havior of "high" and "low" scorers. Miller does not examine the empirical relationships between cynical attitudes and political actions, but his remarks point to a conclusion that political cynics, unlike those who trust government, favor large-scale political change. Both the precise objectives of the politically disaffected, however, and the tactics they are likely to adopt remain ambiguous. Are political cynics merely seeking new policies, or do they favor a complete reconstitution of the ongoing political order? Will political cynics seek change in the streets rather than at the ballot box? Does mistrust of government lead people to disobey the law, participate in civil violence, or join revolutionary organizations? And are the politically cynical more likely than those who trust the government to withdraw from involvement in electoral politics?

The standard hypothesis, of course, is that political disaffection (cynicism, alienation) is associated with a rejection of conventional or "conformist" modes of political participation such as voting, lobbying, writing letters to congressmen, and campaigning for political candidates. Thus, the politically cynical should be more likely than those who trust the government *either* to withdraw from political activity altogether *or* to engage in noncustomary, sometimes illegal, activities such as participating in sit-ins or riots, or organizing for revolution. Whether a political cynic adopts an active or an apathetic mode of behavior depends on the interactions among factors such as the level of social support for "alienated" behavior, the availability of an "alienated" response option in the concrete situation, and the individual's social status and personality.[10]

Previous research provides substantial support for these ideas. For example, Muller's Waterloo, Iowa study indicates that a low degree of trust in political authorities is strongly related to a "readiness to engage in acts of unconventional dissent against the state."[11] Schwartz reports that among both university students and urban blacks political alienation leads to the repudiation of "conformist" modes of political participation.[12] Sears and Maconahay show that participation in the 1965 Watts riot was associated with both a generalized sense of political disaffection and mistrust of specific local agencies and officials such as Mayor Yorty and the police.[13] Paige's study of the 1967 riots in Newark indicates that mistrust of local government, when combined with a strong sense of political efficacy, fostered participation in riot activity.[14] Finally, our Berkeley-based study of political alienation in the San Francisco-Oakland Bay Area found a strong relationship between feelings of disaffection from the national political regime and self-reported involvement in unconventional political protests.[15]

While these studies confirm the general proposition that unfavorable evaluations of the political system are associated with support for or participation in "oppositionist" political behavior, they leave several important issues unresolved. For example, there is conflicting evidence about whether political cynicism has a *direct* effect on riot participation.[16] More importantly, there is no consensus about the exact nature of the attitudes that underlie unconventional or illegal protest behavior. Some studies refer to feelings of pride in or belonging to "the country," others to trust in the national government or public officials "in general," and still others to evaluations of specific leaders or local government. But since these attitudes are not always strongly intercorrelated and their behavioral consequences may vary, the political implications of aggregate change on any one attitudinal dimension are unclear and may often be quite limited. Thus, the finding that a sense of estrangement from the political *community*, in Easton's terms, leads people to adopt an "oppositionist" stance and to withdraw from electoral politics does not imply that increased cynicism about the incumbent national administration will reduce turnout at presidential elections.

In this regard one recalls that many of the Watts rioters combined mistrust of local government with strong approval of the federal government, President Johnson, and the Democratic party. Quite plausibly, attitudes and actions are more likely to be linked when they are responses to

[10] See David Schwartz, *Political Alienation and Political Behavior* (Chicago: Aldine Publishing Co. 1973), ch. 8, for a good summary of the problems in "modeling" the attitude-behavior linkage. The concept of a "behavioral orientation" is similar to Rokeach's concept of "attitude toward the situation." See Milton Rokeach, *Beliefs, Attitudes and Values* (San Francisco: Jossey-Bass Inc., 1970).

[11] Edward N. Muller, "A Partial Test of a Theory of Potential for Political Violence", *American Political Science Review*, 66 (September, 1972), 928–959.

[12] Schwartz, chaps. 9, 10.

[13] David O. Sears and John Maconahay, *The Politics of Violence* (Boston: Houghton Mifflin, 1973).

[14] Jeffery. Paige, "Political Orientation and Riot Participation", *American Sociological Review*, Vol. 36, (1971), 810–820.

[15] Jack Citrin, Herbert McClosky, J. Merrill Shanks, and Paul M. Sniderman, "Sources and Consequences of Political Alienation: A Preliminary Report on Indicator Development," unpublished paper delivered at the Madison, Wisconsin, Conference on Public Support for the Political System, August 13–17, 1973.

[16] See, for example, the conflict between Paige's study of Newark rioters and the report on the Watts riot in H. Edward Ransford, "Isolation, Powerlessness and Violence: A Study of Attitudes and Participation in the Watts Riot", *American Journal of Sociology*, Vol. 73 (1968), 581–591.

the same stimulus object.[17] "Oppositionist" activity usually expresses a sense of grievance about some concrete situation and it takes as the targets of its verbal and physical attacks those perceived as responsible for that unsatisfactory situation. Those blamed need not include every, or even any, political institution and authority; in fact, we would expect protestors to express trust rather than cynicism toward government officials and agencies that share their objectives.

The closeness of the relationships among feelings of dissatisfaction about some specific issue, the belief that some circumscribed set of political actors or institutions are to blame, and the decision to act on that issue or against those authorities raises two difficulties for the argument that a diffuse sense of mistrust in government is causally related to "oppositionist" behavior. There is, first of all, the familiar possibility that people rationalize actions based on a narrowly defined grievance or on nonpolitical grounds by characterizing the entire political system as unresponsive and untrustworthy. In other words, behaviors may cause attitudes rather than the reverse. Given the connection between policy dissatisfaction and political cynicism, however, a more fundamental issue arises: Even if mistrust of government precedes acts of dissent against the authorities, does it have an independent effect on behavior. The customary hypothesis that feelings of political mistrust or alienation intervene between a sense of discontent and "oppositionist" actions requires not only that cynical and trusting repondents differ in their behavior, particularly at high levels of policy dissatisfaction, but also that with respondents' orientation toward the political system controlled, the relationship between feelings of dissatisfaction and the relevant behavioral dependent variable is significantly diminished. Few researchers have addressed this issue directly;[18] thus it remains unclear whether or not our capacity to predict the willingness of those who are dissatisfied with current policy to engage in "oppositionist" activity is enhanced by knowing that they mistrust government.

The election studies Miller and I have analyzed provide few opportunities for an analysis of the behavioral consequences of feelings of political cynicism. The 1968, 1970, and 1972 studies did, however, ask respondents whether they approved of a person's showing his disagreement with government policy by: (1) taking part in a legal protest march, (2) refusing to obey an "unjust" law, or (3) attempting, as a last resort, to disrupt the normal activities of government by joining a sit-in or mass demonstration.[19] Admittedly, these forms of protest, while unpopular and in some instances illegal, pose less severe challenges to the political regime than unorganized civil violence or organized attacks on political targets by revolutionary groups; moreover, the survey questions cited above refer to a generalized readiness to approve of certain forms of noncustomary dissent rather than to the respondent's willingness to engage in such behavior himself. Nevertheless, Muller's Waterloo, Iowa study indicates that approval of disruptive sit-ins and of noncompliance with "unjust" laws fall midway on a continuum of support for anti-regime activity that is defined by opposition to all forms of political dissent at one end and approval of armed revolution at the other.[20] Muller also reports that his measures of approval of political violence and the intention to engage in political violence were strongly interrelated (Somers's $d = .53$)[21]. Thus, it appears appropriate and unexceptional to consider the CPS questions about approval of protest marches, civil disobedience, and participation in disruptive sit-ins as indicators of a behavioral orientation that is both "nonconformist" and activist. Accordingly, if the Trust in Government scale measures feelings of alienation from the political regime, political cynics should be more likely than those who trust the government to endorse acts of political dissent.

The data, however, provide only partial support for this hypothesis. As Table 4 shows, the aggregate level of trust in government declined between 1968 and 1972. The extent of unequivocal opposition to protest marches civil disobedience, and disruptive sit-ins also diminished. But at the microlevel, there is neither a strong nor even a consistent association between political cynicism and approval of these forms of political protest. In 1970 and 1972, "low" scorers on the Trust in Government scale were slightly more likely than respondents with a high level of political trust to express at least qualified approval of protesting against government policy through disruptive demonstrations. In 1968, however, political cynicism and approval of sit-ins or mass demonstra-

[17] See the argument made by Martin Fishbein, "Attitude and the Prediction of Behavior," in *Readings in Attitude Theory and Measurement*, ed. Martin Fishbein (New York, John Wiley and Sons, 1967).

[18] The exception is Muller, who finds that relative deprivation has a weak influence on the potential for political violence and that this influence is entirely dependent on whether such feelings of deprivation are associated with political mistrust. Muller's measure of relative deprivation, however, does not include the element of politicization that is subsumed by the concept of policy dissatisfaction.

[19] The exact working of these questions can be found in the ICPR codebook of the CPS 1970 National Election Study, pp. 83–84.

[20] Muller, "A Partial Test of a Theory . . .", p. 934.

[21] *Ibid.* p. 936.

Table 4. Political Trust and Approval of Political Protest, 1968–1972 (percentages down)

	1968 Trust in Government				*1970* Trust in Government				*1972* Trust in Government			
	Low ($n=388$)	Middle ($n=523$)	High ($n=268$)	Total ($n=1179$)	Low ($n=348$)	Middle ($n=332$)	High ($n=171$)	Total ($n=851$)	Low ($n=951$)	Middle ($n=801$)	High ($n=466$)	Total ($n=2218$)
Legal Protest Marches:												
Approve	15.1	20.1	24.2	19.5	14.0	14.0	14.6	14.2	21.6	18.4	16.8	18.7
Depends	27.1	25.7	28.9	26.9	40.9	43.5	37.4	41.1	39.5	40.1	41.6	40.8
Disapprove	57.8	54.2	46.9	53.5	45.1	42.6	48.0	44.7	38.9	41.5	41.6	40.5
		($p<.05$)				n.s.				n.s.		
Refusal to Obey Unjust Laws:												
Approve	13.1	17.1	12.6	14.7	12.9	10.4	11.0	11.6	20.9	13.5	11.7	16.6
Depends	27.8	23.1	18.8	23.9	40.7	35.9	37.2	38.2	42.2	39.0	36.9	39.3
Disapprove	59.0	59.8	68.6	61.4	46.4	53.7	51.7	50.3	36.9	47.6	51.4	44.0
		($p<.05$)				n.s.				($p<.05$)		
Disruptive Sit-ins, Demonstrations:												
Approve	6.2	8.2	10.1	8.0	9.8	6.6	6.9	7.9	10.0	7.2	5.2	7.9
Depends	15.5	20.1	17.2	17.9	36.5	31.0	24.9	31.8	35.5	32.4	29.8	33.6
Disapprove	78.4	71.7	72.8	74.1	53.7	62.3	68.2	60.3	54.5	60.4	65.0	58.5
		n.s.				($p<.05$)				($p<.05$)		

tions were unrelated. Moreover, in 1968 the politically trusting were *more* likely than political cynics to approve of noncompliance with an "unjust" law or participation in legal protest marches.

The weak and unsystematic relationships between scores on the Trust in Government scale and support for an activist behavioral orientation belie the contention that a diffuse mistrust of political authorities intervenes between political cynicism and approval of disruptive sit-ins or mass demonstrations. The relationships that do appear may be a spurious reflection of the association between mistrust of government and policy dissatisfaction. In both 1970 and 1972, policy dissatisfaction, operationalized as Miller proposes by measuring the distance between an individual's policy preferences and those he identifies with the ruling Republican party, was positively correlated with approval of all three forms of political protest cited above.[22] Controlling for policy dissatisfaction causes the relationships between political cynicism and support for disruptive sit-ins to vanish, whereas the relationships between policy dissatisfaction and support for activist modes of protest remain statistically significant even when we control for the putative intervening variable, trust in government. Thus, it appears that feelings of cynicism about "the government in general" have no independent effect on someone's willingness to approve of or engage in noncustomary and illegal protests. Of course, failure to confirm a strong connection between mistrust of government and support for "oppositionist" activity may reflect inadequacies in our measures of these attitudes and behaviors rather than faulty theorizing.

The 1970 and 1972 election studies indicate that the intensity of a person's discontent with current policy has a direct effect on his orientation toward acts of political dissent. In the 1968 study, however, no such association was found between policy dissatisfaction and approval of legal protest marches, civil disobedience, or sit-ins. Part of the explanation for this discrepancy between the 1968 data and the results of the later studies is that the *direction* of a person's policy preferences as well as the *degree* of his dissatisfaction influence his willingness to support an activist behavioral orientation. In all three data sets analyzed, leftist positions on policy questions are correlated with approval of legal protest marches, noncompliance with unjust laws, or disruptive sit-ins. Given the historical and contemporary associations of these types of protest activity with left wing or liberal causes, this is hardly surprising. But while the relationships between issue orientations and approval of noncustomary protest tactics remained stable, the proportions of "leftists" and "rightists" among those dissatisfied with government policy, and among the political cynics, shifted between 1968 and 1972. And as the discontented and cynical groups came to include relatively more blacks and liberals, they were more likely than respondents expressing satisfaction with current policy and trust in government to endorse unconventional forms of political protest. Thus, whether declining levels of political trust imply a growing potential for political protest and violence depends at least in part on the nature of the relationships among political cynicism, policy dissatisfaction, and ideological orientations.

Granting that political cynicism plays a minor role in producing activists or rebels, does this attitude toward government generate apathy? Are political cynics more likely than people who trust government to withdraw from participation in "conventional" electoral politics? Table 5 conclusively refutes this proposition. Although feelings of political powerlessness and perceptions of governmental institutions as unresponsive to the public's demands are consistently and strongly associated with low levels of political interest and participation,[23] mistrust of government and political apathy do not go together. Even without controlling for education, differences between "low" and "high" scorers on the Trust in Government scale tend to be small whatever the type or measure of political awareness and involvement we examine. In 1964, political cynics, many of whom were well-educated Goldwater supporters, tended to be more active than trusting respondents, whereas during the 1968, 1970, and 1972 elections the levels of interest and participation among cynical and trusting respondents were virtually identical.

Perhaps the most appropriate context for testing the hypothesis that mistrust of government leads to withdrawal from conventional political activity is the 1970 election, a low-stimulus contest without an "anti-mainstream" presidential candidate to mobolize political cynics. In that year, however, cynical and trusting respondents did not differ in their level of prior voting, voting in the 1970 election itself, media exposure during

[22] The measure of policy dissatisfaction for both 1970 and 1972 refers to the mean distance between a respondent's issue position and the position he attributes to the Republican party on the following eight issues: Vietnam, health insurance, inflation, urban violence, crime control, campus protests, and pollution. See Miller, this issue of the *Review*. The figures concerning the relationships between policy dissatisfaction and approval of political dissent are not included, but in every case these are significant at at least the .01 level.

[23] See Citrin, "Political Disaffection in America," chap. 6, for a detailed report on the connections between diverse orientations toward the political system and political participation.

Table 5. Political Trust and Political Involvement, 1964–1972
(percentages down)

	Trust in Government			
1964:	Low ($n=324$)	Middle ($n=531$)	High ($n=569$)	Total ($n=1424$)
Voted	74.8	81.2	77.1	77.7
Did not vote	25.2	18.8	22.9	22.3
Paid a great deal of attention to campaign	43.5	40.1	35.5	38.2
Paid some attention	33.0	37.5	39.4	36.6
Paid little attention	23.5	22.4	25.1	25.1
Pay a great deal of attention to governmental affairs	37.5	32.2	25.3	30.3
Some attention	34.8	40.1	47.4	41.6
Little attention	16.3	16.6	17.5	16.9
Have written to a public official	21.3	18.4	13.3	16.9
Have never written	78.7	81.6	86.7	83.1
Campaign activity index:*				
Performed all five activities	2.2	1.3	0.7	1.2
Performed four activities	2.5	1.9	0.5	1.5
Performed three activities	4.0	2.3	3.1	3.0
Performed two activities	9.0	8.3	10.7	9.3
Performed one activity	26.5	30.1	23.6	26.4
No campaign activities	55.9	56.1	61.4	58.6

	Trust in Government			
1968:	Low ($n=427$)	Middle ($n=574$)	High ($n=303$)	Total ($n=1304$)
Voted	70.6	78.6	76.3	78.5
Did not vote	29.4	21.4	23.7	21.5
Paid a great deal of attention to campaign	39.3	40.3	38.8	38.9
Paid some attention	37.2	42.4	45.1	40.4
Paid little attention	23.4	17.4	16.1	20.8
Follow public affairs most of the time	37.1	31.9	28.7	33.0
Some of the time	23.8	34.0	34.0	30.7
Now and then	18.6	18.8	19.5	18.7
Hardly at all	20.5	15.3	17.8	17.6
Have written to a public official	21.5	18.0	19.4	19.8
Have never written	78.5	82.0	80.6	80.2
Campaign activity index:*				
Performed all five activities	0.0	1.6	1.0	0.9
Performed four activities	2.3	1.7	1.0	1.8
Performed three activities	3.5	5.4	2.6	4.1
Performed two activities	7.0	9.6	5.9	7.9
Performed one activity	25.1	27.5	26.2	26.6
No campaign activities	62.1	54.2	63.3	58.7

Table 5 continued on next page

Table 5.—*Continued*

	Trust in Government			
1970:	Low (n=647)	Middle (n=566)	High (n=309)	Total (n=1522)
Voted	56.2	60.1	57.6	57.6
Did not vote	43.7	39.9	42.4	42.4
Paid a great deal of attention to campaign	31.1	36.0	33.0	33.1
Paid some attention	41.8	43.0	47.2	43.2
Paid little attention	27.1	21.0	19.8	23.7
Campaign activity index:*				
Performed all five activities	0.9	1.5	1.3	1.2
Performed four activities	1.7	3.4	2.5	2.5
Performed three activities	2.3	4.5	3.1	3.2
Performed two activities	4.8	5.5	6.9	5.4
Performed one activity	22.3	22.8	18.9	21.6
No campaign activities	68.0	62.3	67.3	66.0

	Trust in Government			
1972:	Low (n=931)	Middle (n=786)	High (n=454)	Total (n=2171)
Voted	69.7	76.0	75.1	72.8
Did not vote	30.3	24.0	24.9	27.2
Paid a great deal of attention to campaign	31.9	34.4	32.8	31.5
Paid some attention	41.1	42.4	44.8	41.1
Paid little attention	27.0	23.2	22.4	27.4
Follow public affairs most of the time	35.7	40.1	33.5	36.6
Some of the time	34.0	36.9	40.1	36.2
Now and then	17.4	14.2	15.9	15.9
Hardly at all	12.9	8.8	10.6	11.4
Have written to a public official	23.4	30.1	30.2	27.1
Have never written	76.6	69.9	69.8	72.9
Campaign activity index:*				
Performed all five activities	0.4	0.9	0.7	0.6
Performed four activities	1.7	0.6	2.2	1.4
Performed three activities	3.1	3.8	3.3	3.4
Performed two activities	7.9	10.9	10.1	9.4
Performed one activity	26.0	24.7	24.7	25.0
No campaign activities	60.8	59.1	59.0	60.2

* This index was created by simply summing the number of these campaign activities performed. The activities were: trying to influence someone's vote, attending a political meeting or rally, belonging to a political club or organization, working for a candidate, and displaying a button or bumper-sticker.

the campaign, concern about the election's outcome, general political interest, political knowledge, or campaign activities such as contributing money, wearing a campaign button, attending political meetings, belonging to a political club or organization, and attempting to influence other people's voting decisions. In other words, the politically cynical were as likely as those expressing trust in government to be eligible for good citizenship awards. *Thus, the evidence that mistrust of government, as operationalized by Miller, produces neither political apathy nor political activism reinforces the argument that many cynical responses merely record opposition to incumbent officeholders or largely ritualistic expressions of fashionable clichés.*

Political Trust and Public Policy: Must "Centrism" Fail?

I do not dispute Miller's finding that disagreement with government policy on important contemporary issues engenders political cynicism.

The data indicate relatively robust and stable correlations between his proposed measure of policy dissatisfaction and the Trust in Government scale—Pearson's $r = .30$ in both 1970 and 1972. Nevertheless, while only 6 per cent of the 1970 respondents with a high level of policy dissatisfaction (mean issue distance scores of 4 or above) expressed a strong sense of trust in government, fully 24.3 per cent of those who agreed completely with the policy preferences of the ruling Republican party expressed political cynicism. Quite plausibly, disagreement with the policies of incumbent authorities is a sufficient but not a necessary condition for judging them untrustworthy and incompetent.

Miller goes beyond this general conclusion to make the more specific assertions that mistrust of government results from dissatisfaction with *both* Republican and Democratic policies and that the "centrist" character of these policies arouses discontent. These claims about the focus and content of policy dissatisfaction are problematic and deserve more detailed consideration.

The argument that political cynicism is related to dissatisfaction with both parties rests on the comparison of mean Trust in Government scores among groups of 1970 respondents cross-classified according to their locations on measures of dissatisfaction with the parties' policies (Miller, Table 8). Miller himself acknowledges that the relationship between cynicism and dissatisfaction with the Democrats, controlling for perceived distance from the incumbent Republicans, is weak and uneven. And the finding that mean levels of trust in government decline monotonically along the diagonal entries in his Table 8 appears to be due largely to the successive increases in perceived distance from the Republican party.

The 1972 election study data indicate unambiguously that dissatisfaction with the perceived policies of the "out" party and its leader are not related to feelings of political cynicism. The correlations between political cynicism and dissatisfaction with the policy stances of the Democrats and George McGovern were an insignificant .04 and −.03 respectively. In addition, Table 6 below a replication of Miller's Table 8 using the 1972 election study data, confirms that at any given level of dissatisfaction with the policies of the Republicans (or Nixon), increased perceived distance from the policies of the Democrats (or McGovern) was *not* associated with higher levels of political mistrust. (Readers are warned that according to my scoring procedures, unlike Miller's, 0 is the most cynical and 5 is the most trusting score on the Trust in Government scale.) Between 1970 and 1972, the tendency of political trust to signify support for the incumbent national administration grew concomitantly with the increased visibility and salience of ideological cleavages between the parties. One can no longer conclude, therefore, that "those who feel that *neither* party offers viable solutions to contemporary social problems are among the most cynical, distrustful, and alienated citizens in the U.S. today" (Miller, p. 968).

Table 6. Dissatisfaction with Party Policy and Political Cynicism: A Replication of Miller's Table 8 with the 1972 Election Study Data

(Entries are mean scores on trust scale. Trust in Government scores range from 0 = most cynical to 5 = most trusting.)

Distance from Republican Party	Distance from Democratic Party 0	1	2	3	4	
0	2.5	2.6	2.4	2.6	2.2	(n=329)
1	2.4	2.1	2.4	2.2	2.0	(n=513)
2	1.3	1.5	1.9	2.1	2.2	(n=244)
3	1.1	1.4	0.9	0.5	1.4	(n=110)
4	1.1	0.8	0.8	0.5	2.2	(n= 82)
	(n=300)	(n=511)	(n=268)	(n=92)	(n=30)	

Distance from Nixon	Distance from McGovern 0	1	2	3	4	
0	2.5	2.6	2.3	2.3	2.2	(n=455)
1	2.2	2.3	2.3	2.3	2.0	(n=514)
2	1.4	1.7	1.9	1.7	1.9	(n=275)
3	1.2	1.11	1.4	1.0	2.5	(n=141)
4	0.9	1.07	0.5	1.6	1.7	(n= 99)
	(n=213)	(n=443)	(n=313)	(n=179)	(n=75)	

Miller bases the proposition that "centrist" policies are the source of public discontent and cynicism toward government on the finding that people who placed themselves at the extremes (scale values 1 and 7) on a number of policy questions were less likely to express trust in government than those who located themselves at the center (scale value 4). Given the institutional and cultural pressures toward compromise, bargaining, and coalition-building in American politics, this conclusion should surprise no one. Indeed, the slight relationship between political cynicism and dissatisfaction with both parties in the 1970 sample reflects the tendency of those at the ideological extremes to reject moderate parties whose primary goal is electoral victory. It is noteworthy, therefore, that Miller's data reveal several deviations from the reported tendency of both the "leftist" and "rightist" camps to contain relatively more political cynics than the group at the center of the political spectrum. For example, Table 5 in his paper indicates that respondents at scale position 4 on the national health insurance issue were more likely to be politically cynical than those at the extreme right of this issue continuum. Similarly, on the question of what should be the rights of accused criminals, the most trusting group of respondents were those with a self-assigned scale value of 2, near the liberal end of the continuum. Only on the urban riot and Vietnam policy questions were there consistently monotonic decreases in the level of political trust as one moved from the center of the policy continuum toward its extremes.

Even if one makes the somewhat unrealistic assumption that people regularly monitor the extent of their disagreement with government policies and then re-evaluate how much they trust political authorities, the impact of a move from the center on the aggregate level of political cynicism depends on the size and cohesion of the "centrist" bloc. Miller, however, underestimates the number of "centrists" in the public by applying this term only to those who assign themselves a scale value of 4 on the seven-point issue continua. That more than half of the 1970 respondents whose Vietnam policy scale values were 3 and 5 respectively agreed that U.S. policy should be "to keep our soldiers in Vietnam but try to end the fighting" suggests that the "centrist" bloc includes a larger number than he allows for. But when the "center" is redefined to include scale values 3, 4, and 5 it becomes the modal, although not the majority, position of the 1970 sample on the issues of urban unrest, Vietnam, rights for the accused, and government aid for minority groups. And when the 1972 study gave respondents the explicit choice of saying they had not thought about an issue, the proportion of respondents with "leftist" (scale values 1 and 2) or "rightist" (scale values 6 and 7) preferences declined even further.

Determining the precise meanings of "leftist," "centrist," or "rightist" policy preferences presents another methodological problem. Miller tends to rely too much on the numerical symbols that denote opinions on a given issue and to overlook the ambiguity of their concrete referents. In some cases it is difficult to imagine what these specific policy positions might be. For example, what is a "centrist" policy on inflation? Does someone who assigns himself a scale position of 5 on this issue reject a policy of wage and price controls? Does someone who calls for "total" government action against inflation advocate a planned economy, a tight money policy, or both? What does it mean to give oneself a scale value of 6 on the issue of urban unrest? Does this indicate support for the use of only two-thirds of the available force to quell riots?

On some issues, of course, elites have defined policy alternatives in ways that make it easy for the public to identify meaningful left, right, and center positions. In Donald Stokes's terms these are "position" issues; that is, on such questions there are rival bodies of opinion about the goals of public policy and how to achieve them.[24] American policy toward the war in Vietnam became a "position" issue on which the policy of "fight and negotiate" emerged as a distinctively "centrist" option. But on most of the other policies Miller discusses, the public does not encounter a well-articulated set of choices that can be ranked along the left-right continuum. And if one cannot tell where along the seven-point continuum respondents would locate a particular policy, the effect of its adoption on the aggregate level of political trust is indeterminate. The self-anchoring scale is an easily administered and hence convenient methodological device, but it is hardly the most appropriate technique for identifying concrete policy preferences. Similarly, when the referents of perceived policy stands are vague or vacuous, we should be cautious about using issue distance measures as valid indicators of policy dissatisfaction. Clearly, the diffuse character of the perceptions that define a respondent's distance from a party or candidate enhances the possibility that "high" policy dissatisfaction scores result from a tendency to attribute discrepant opinions to people one already dislikes.

Thus, the relationships between political cynicism and "extremist" policy orientations do not imply that abandoning "centrist" policies would rebuild trust in government. Miller argues that

[24] For the distinction between "position" and "valence" issues, see Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review*, 60 (March, 1966), pp. 19–38.

when government moves to the left it often increases the number of "cynics of the right," and vice versa. By the logic of his own argument, such shifts in policy would also produce "cynics of the center." As both George McGovern and Barry Goldwater have discovered, Anthony Downs remains relevant reading material.

Miller is convinced that only policies of social change—"leftist" or liberal policies, that is—can solve existing social problems, improve the quality of life, and thereby stem the tide of public cynicism and dissatisfaction. He advocates "creative and constructive" action that will benefit the majority of people and diminish feelings of frustration. It is difficult to oppose such a recommendation. Of course, government will have to counter the inevitable resistance to creative radical change by "educating the public so that it will better understand the need for social change and the benefits which would evolve from such change" (Miller, p. 972). In other words, the restoration of public trust in government requires a "centrist" president to sell "leftist" policies to a sizeable group of "rightists." According to Miller, failure to achieve these changes in policies and leadership will cause even more people to repudiate the underlying goals of the ongoing political system and raise the probability of extra-legal protests leading to major institutional transformations.

It seems to me that this apocalyptic prognosis rests on a dubious interpretation of the meanings of political cynicism and policy dissatisfaction, as operationally defined. In addition, Miller's pessimism about the consequences of the government's continuing to pursue "centrist" policies, or, more properly, of its seeming to do so, assumes that the most salient political questions are "position" issues on which there is a sharp polarization of public opinion. Today, however, "valence" issues such as inflation, economic prosperity, the energy crisis, and honesty in government are uppermost in the public mind. On these issues, everyone agrees about the goals of public policy; everyone, including political cynics, is against inflation and corruption and for prosperity, full employment, and cheap gasoline. Because public opinion on "valence" issues does not constrain the government's choice of specific policies for achieving consensual goals, *results*, such as an improving economy, will do more to rebuild trust in government than the adoption of some particular program or ideological orientation.

The current decline in trust in government and the public's seeming loss of confidence in a wide range of social institutions and elites are expressions of a pervasive sense of malaise. Many Americans feel unhappy about the state of the nation and, quite naturally, blame incumbent authorities for this situation. But many political cynics are probably expressing the conviction that "the times are bad" rather than repudiating the political regime on ideological grounds. To use another baseball analogy, political systems, like baseball teams, have slumps and winning streaks. Having recently endured a succession of losing seasons, Americans boo the home team when it takes the field. But fans are often fickle; victories quickly elicit cheers. And to most fans what matters is whether the home team wins or loses, not how it plays the game. According to this analysis, a modest "winning streak" and, perhaps, some new names in the lineup may be sufficient to raise the level of trust in government.

Unfortunately, winning baseball games comes more easily than progress in solving the complex and intractable problems that face American society. Overcoming public resistance to social change becomes a pressing issue only after government discovers the "creative and constructive" programs that, if implemented, will benefit the majority of the people. In the meantime, politicians should follow Miller's advice to promise less and deliver more. For as long as "bad times" persist and current linguistic habits do not change, political authorities, *whatever their ideological predilections*, will remain the object of considerable public scorn and mistrust.

Conclusion

Since the previous sections have necessarily accentuated my disagreements with Miller's analysis, I would like to re-emphasize the significance of his demonstration that political events, attitudes, and expectations have been a primary source of declining trust in government. Future research should develop superior indicators of subjective orientations toward the political system and specify more precisely the cognitive processes that link policy dissatisfaction to political cynicism. In this regard, we need to distinguish, operationally, between the following attitudes: dissatisfaction with current government policy positions, dissatisfaction with the *outcomes* of ongoing events and policies, mistrust of incumbent officeholders, and rejection of the entire political system.

We also need greater knowledge of the consequences of cynical attitudes at both the micro and macro levels. In particular, we require an empirical assessment of the familiar contention that low levels of political trust inhibit the capacity of political elites to make necessary decisions and commit public resources to collective ends. Stated so abstractly this proposition appears plausible, but its confirmation in a concrete case surely depends on such factors as the number of

Rejoinder to "Comment" by Jack Citrin: Political Discontent or Ritualism?

ARTHUR H. MILLER
The University of Michigan

In 1958 only 22 per cent of the total population felt that they could not "trust the government in Washington to do what is right" all or most of the time. By the fall of 1972 that figure had climbed to 45 per cent. Furthermore, the percentage of eligible voters participating in the 1972 presidential election was the lowest it has been since 1948; crises and scandals have continually plagued the government since the Watergate revelations; and the economic conditions of the country have provoked widespread uncertainty and anxiety among the populace. There is good reason, then, for the intense current interest in attitudes of political disaffection and alienation.

Present U.S. conditions demonstrate that political alienation is a phenomenon of fundamental significance in political processes. Feelings of political cynicism and alienation may substantially diminish the willingness of citizens to participate in politics or to support programs directed at resolving the social problems that stimulate discontent. Attitudes of political alienation have likewise been related to public demands for radical political reforms during trying periods of social or economic discontent. Alienation and non-participation, however, go beyond just questions of voluntary compliance with policies or the possibilities for radical change; they strike at a very basic democratic norm. Democratic theory emphasizes voluntary consent as the basis of political obligation and legitimacy. Democratic government assumes—indeed, requires—widespread participation, political equality, the accountability of leaders and protection of the individual citizen's constitutional guarantees. The full attainment of these values is only possible when the relationship between the leaders and the public is based on mutual understanding and reciprocal trust rather than on the use of coercive and arbitrary authority.

Political distrust has varying significance for different political systems. Distrust obviously has other implications in a two-hundred-year-old democracy than it would in a dictatorship that has been in existence for only a brief period. Because of these differences a concern with questions of legitimacy in the United States seems more reasonable when focused on satisfaction or dissatisfaction with the implementation of values rather than on the acceptance or rejection of those values. Distrust of government in the U.S. may, for some, be associated with the partisan hopes of "voting the rascals out;" for others, it may indicate a sense of enduring inequities in government decisions and outputs. It does not, therefore, necessarily imply a desire for a completely different form of government—although for some it may mean exactly this. These varying "foci" of distrust may be conceptualized as a continuum incorporating distrust of the leaders, various institutions, political processes, and democratic values. The behavioral consequences of political disaffection may be expected to divaricate from apathy to participation in reform or protest behaviors, depending on the efficacy and social conditions of the population subgroups under investigation. Likewise, we can expect the several causes or correlates of a complex attitude such as trust in government to be affected by sociological and historical circumstances.

Because of these complexities it is understandable that Professor Citrin should raise, in his "Comment," questions about the meaning, behavioral consequences, and political correlates of trust in government. A number of conclusions that his discussion leads to are, however, inconsistent with the data, and more importantly, hold disturbing implications for the relevance of empirical political research as social commentary. These inconsistencies, problems of interpretations and the questions raised in the "Comment" can best be explicated by a further discussion of the three major points of the "Comment."

The Meaning of Declining Trust in Government

Political trust is the belief that the government is operating according to one's normative expectations of how government should function. The concept is closely related to the notion of legitimacy, a statement that government institutions and authorities are morally and legally valid and widely accepted. Presumably, the behavior and decisions of trusted authorities are more likely to be accepted as legitimate and worthy of support than are those of distrusted leaders. At an abstract, conceptual level, trust in government—through the notion of legitimacy—thus becomes associated with questions of identification with, or estrangement from, political institutions, symbols and values. On a more practical,

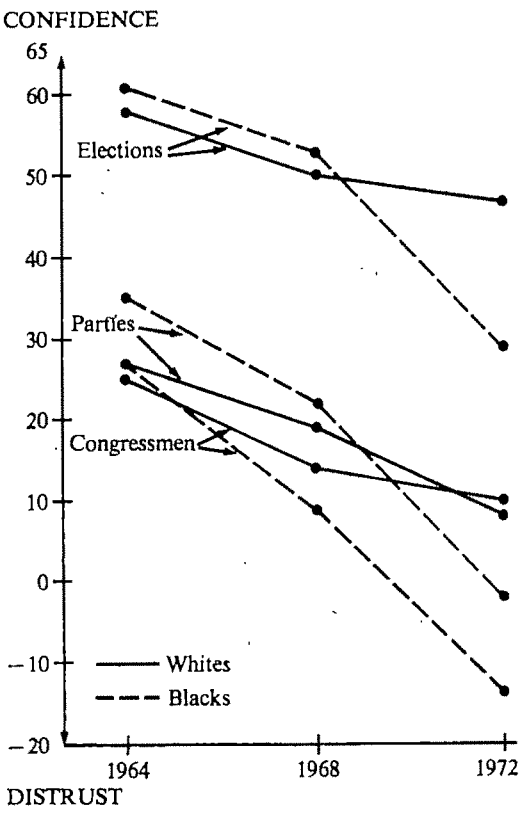


Figure 1: Confidence in Leaders, Parties, and Elections, 1964–1972

operational level, political trust may be treated as an affective orientation toward the "government in Washington"—the most salient level of government in the United States.[1]

The validity of the trust in government scale as a measure of political disaffection cannot be ascertained simply from the manifest content of the five items used to construct the scale. Nor can the specific target of the distrust be determined from the scale items alone. Questions of validity and focus can only be answered through an examination of the relationship between the trust scale and other political indicators.

As noted in "Political Issues and Trust in Government," political alienation has been predominantly conceptualized in terms of negative affect for the existing political process, and it has been viewed as composed of two distinct attitudinal components—political efficacy and political trust.[2] Political efficacy, besides showing declining trends similar to that of political trust,[3] has become increasingly intercorrelated with trust, going from a Pearson *r* of .17 in 1964 to .35 in 1972. Furthermore, political trust has also been correlated with attitudes of government responsiveness (.35 in 1964 and .39 in 1972).

Prior to 1972 CPS surveys were not well suited to an examination of the extent to which distrust has been generalized across various governmental institutions. The trend lines for three government responsiveness items dealing with confidence in congressmen, parties and the institution of elections are, however, somewhat informative about the focus of political discontent; they are thus presented in Figure 1.[4]

It is clear from Figure 1 that by 1972, discontent had been generalized to several different aspects of the political system. As with the political trust scale, the trend is more severe among blacks, who changed from the more trusting to the less

[1] Support for the notion that the federal government is the most salient level of the political system can be found in M. Kent Jennings and Harmon L. Zeigler, "The Salience of American State Politics," *American Political Science Review*, 64 (June, 1970), 523–535.

[2] A more complete conceptual and theoretical statement of the different dimensions of alienation is presented in Ada W. Finifter, "Dimensions of Political Alienation," *American Political Science Review*, 64 (June, 1970), 389–410, and William A. Gamson, *Power and Discontent* (Homewood, Illinois: Dorsey Press, 1968).

[3] For a discussion of the efficacy trend, see Philip E. Converse, "Change in the American Electorate," in Angus Campbell and Philip E. Converse, eds., *The Human Meaning of Social Change* (New York: Russell Sage Foundation, 1972).

[4] The three government responsiveness questions were:

1. How much do you feel that political parties help to make the government pay attention to what the people think: a good deal, some, or not much?
2. And how much do you feel that having elections makes the government pay attention to what people think: a good deal, some, or not much?
3. How much attention do you think most congressmen pay to the people who elect them when they decide what to do in Congress: a good deal, some, or not much?

A percentage difference index was computed for each of the three questions by subtracting the percentage responding "not much" from the percentage answering "a good deal." Positive values of the PDI therefore specify the degree of confidence, whereas negative values denote a lack of confidence. These PDI values are plotted in Figure 1 for whites and blacks.

It should be noted that these items indicate the respondent's confidence in elections, parties and congressmen to make government responsive and not the respondent's confidence, *per se*, in the institutions and leaders referred to by the questions. The interpretation of the items as indicators of "diffuse public support" was suggested by Jack Dennis in "Support for the Institution of Elections by the Mass Public," *American Political Science Review*, 64. (September, 1970), 819–835.

Table 1. Relationship Between Confidence in the Responsiveness of Elections and Parties and Political Trust, 1972

Elections make government responsive	Trusting	Cynical	Parties make government responsive	Trusting	Cynical
A Good Deal	72%	41%	A Good Deal	37%	18%
Some	25	45	Some	53	53
Not Much	3	14	Not Much	10	29
	100%	100%		100%	100%
(*N*)	(825)	(776)	(*N*)	(803)	(763)
	Gamma = .40			Gamma = .35	

trusting group between 1964 and 1972. The data for blacks suggest that, in general, discontent is directed *first* at political leaders and parties and later becomes generalized to such institutions as elections. The persistently high degree of confidence in elections exhibited by whites is consistent with such a pattern. Their discontent with leaders and parties may not yet have reached the critical level at which it becomes broadly generalized to the most fundamental institution of all, that of elections.

The data of Figure 1 confirm what other scholars have suggested about political discontent, namely, that it is not necessarily an undifferentiated malaise directed with equal intensity at the leadership and at all political institutions. Dennis, for example, has reported that public evaluations not only vary across different institutions, but that the major governmental institutions are differentially rated with respect to trustworthiness, power and competence.[5] Gamson also hypothesized that discontent might first be directed at the authorities and then later become generalized to distrust of institutions if a change in leadership brought no reduction in discontent.[6]

One point which apparently needs emphasis is that while elections enjoy a greater degree of confidence than parties as mechanisms for making government responsive, evaluations of both these institutions are related to the general attitude of trust in government.[7] The strength of this relationship can be ascertained from Table 1 which presents the crosstabulation of the trust scale and the two responsiveness items. The data demonstrate that trusting respondents were substantially more likely to express a "good deal" of confidence

[5] Jack Dennis, "Public Support for American National Political Institutions," (paper presented at the Conference on Public Support for the Political System, Madison, Wisconsin, August 13–17, 1973).

[6] Gamson, *Power and Discontent*, p. 51.

[7] This statement is very straightforward; it is emphasized here because Citrin has ignored the strong correlation between the "change in form of government question" and political trust, choosing instead to call attention to only a few cells in the crosstabulation of the two variables.

The reader should also be aware of an important difference between the trust in government *index* that Citrin uses and the trust in government *scale* used in "Political Issues and Trust in Government" and in this "Rejoinder." The scale scores reflect the individual response pattern for the five trust in government items ordered according to the proportion of cynical responses. Individuals with no more than two missing data responses or scale errors were assigned the score that is the median value of the possible scale scores for that particular response pattern. Respondents with more than two errors or two missing data responses were excluded from the analysis. Citrin, however, simply formed an index by adding up responses to the five trust items, thereby ignoring the cumulative nature of the items. This method difference affects the overall distribution of the trust measure as indicated below for 1972:

	Trust Index[a]	*Trust Scale*
Low	42.8%	36.8%
Medium	36.2	24.0
High	21.0	39.2
	100.0%	100.0%

[a] Figures taken from Citrin's Table 2, this issue of the *Review*.

The index and scale scores are no doubt highly correlated but they are not identical. Citrin's analysis, therefore, cannot be viewed as a replication of "Political Issues and Trust in Government," nor is it strictly comparable to the analysis presented in this "Rejoinder."

The conceptual distinction between an additive index and a scale is well established in the empirical literature; for example, see Helen Peak, "Problems of Objective Observation," in Leon Festinger and Daniel Katz (eds.), *Research Methods in the Behavioral Sciences* (New York: Holt, Rinehart and Winston, 1953), chapter 6. For a more recent statement of the response models underlying unidimensional scales, see Herbert F. Weisberg, "Dimensional Analysis of Legislative Roll Calls," (Ph.D. dissertation, The University of Michigan, 1968) or Herbert F. Weisberg, "Dimensionland: Excursions into Spaces," *American Journal of Political Science* (forthcoming, November, 1974).

in both elections and parties as a means of producing responsive government than were cynical respondents. Despite the differences in the distribution of responses to the elections and parties questions, both items were strongly correlated with the trust in government scale.

It is not surprising to find relatively more public support for the institution of elections than for congressmen. Similarly, it is not surprising, nor inconsistent, to find that 45 per cent of the 1972 respondents felt that they could "trust the government in Washington to do what is right only some of the time," while at the same time 41 per cent thought that a change in "our whole form of government was needed to solve the problems facing our country," and only 14 per cent said "I can't find much in our form of government to be proud of."[8] The importance of differences in marginal distributions for more or less extremely worded survey questions should not be overstated. What is critical here is the relationship between these relatively more extreme items and the trust in government scale, as well as the determinance of what respondents mean, substantively, when they say that a change is needed in our form of government.

Both the "pride in our form of government" and "change in our form of government" questions are somewhat biased as indicators of political discontent because, unlike the trust and responsiveness scale items, they are more sensitive to discontent among those with a social change orientation than among those expressing a social control ideology.[9] While the trust and responsiveness items measure disaffection equally as well among both ideological groups, the "pride" and "change" questions can be expected to exhibit a larger proportion of discontented responses for individuals with a leftist rather than a rightist orientation. For example, 51 per cent of those with a social change orientation say that at least "some change" in our form of government is needed, whereas only 36 per cent of those on the right say "some change" is needed. The correlation between the "change" item and the trust scale is so strong, however, that despite this question bias, the two measures are substantially related even after controlling for ideology (see Table 2). In fact, the "pride" and "change" items have loadings of .77 and .67, respectively, on the single factor (which explains 61 per cent of the item variance) obtained from a factor analysis of these and the five individual trust in government items. Thus, the "pride" and "change" items can be viewed as measuring the same underlying attitudinal dimension that is tapped by the trust in government scale.

Further support for this conclusion comes from the open-ended responses which give a good indication of what respondents had in mind when they replied that a change was needed in our form of government. These responses hardly intimate a strong desire among the populace for change from democracy to some other form of government: Less than 1 per cent of the respondents proposed a change toward a socialistic government. What *was* expressed by the open-ended statements was a discontent and dissatisfaction with the performance of the system and the need for reform to make it more responsive. A majority of the respondents commented: that the government pays more attention to some people than to others; that parties and the government bureaucracy are not responsive; that politicians are out of touch, unresponsive and untrustworthy; and that the electoral system is antiquated and in need of reform. The recommended changes included an increase in the representativeness of government, an increase in popular participation and control, a decrease in the power of the rich and an increase in the power of the poor, a more efficient government, and more governmental efforts directed at the problems of social welfare, crime control, unemployment, inflation and taxes. About one quarter of the responses suggested some type of institutional reform, such as changing the power relations among the President, Supreme Court and Congress, or reforming the parties, electoral system and government bureaucracy —but all with the intention of increasing popular control and government responsiveness.

To summarize, political cynicism is related to feelings of political inefficacy, to the belief that government is unresponsive, and to an apparent desire for structural and institutional reform. The trend toward increased distrust, therefore, reflects a growing dissatisfaction and discontent with the performance of government in the United States. In general, the trend indicates an increasing sense that the government is not functioning in a manner consistent with the normative expectations of how a democratic government ought to function. We should *not*, however, conclude that this trend simply reflects an increase in superficial, "ritualistic" responses just because distrust is not synonymous with the desire to replace democracy with another form of government. On the contrary, the trend is a serious statement that government in the U.S. is perceived as falling far short of democratic goals. We might expect, therefore, that the radicalism of desired change and reforms, as well as the methods used to effect that change,

[8] This is the same item referred to by Citrin. The question was contributed to the 1972 CPS election study by J. Merrill Shanks.

[9] The operational definition of the ideological orientations used here is the same as that used in "Political Issues and Trust in Government."

Table 2. Relationship Between Desired Change in Our Form of Government and Political Trust by Ideological Orientation, 1972

	Social Change Ideology			Social Control Ideology		
	Trusting	Cynical	Total	Trusting	Cynical	Total
Desired Change						
Big Change	9%	35%	22%	4%	22%	11%
Some Change	24	33	29	17	29	25
Keep As Is	67	32	49	79	49	64
	100%	100%	100%	100%	100%	100%
(*N*)	(91)	(103)	(255)	(185)	(142)	(441)
		Gamma = −.47			Gamma = −.46	

would depend on the profundity and duration of political discontent.

Some Behavioral Consequences of Political Disaffection

It is suggested in the "Comment" that attitudes of political distrust and disaffection have no independent correlation with political participation, direction of vote, candidate evaluation, or support for protest behaviors; according to Citrin, this therefore implies that political cynicism is largely a ritualistic expression of fashionable clichés.

Space limitations did not allow the inclusion of an investigation of the behavioral consequences of political disaffection in "Political Issues and Trust in Government." Moreover, a few pages in a "Rejoinder" would hardly suffice as an introduction to the subject since one would expect this to be a complex set of relationships affected by social conditions and psychological motivations. The discussion in this section will therefore simply elaborate on some of the relationships briefly referred to in the "Comment."

Previous studies linking alienation and behavior have been limited in several respects: in their focus on a narrow set of behaviors; in their contradictory findings; in their failure to investigate racial differences—a very important factor in both alienation and behavior. In addition, they generally provide little insight into the conditions under which alienated individuals will or will not display certain types of behavior. Much of the confusion appears to arise from the complexity of these relationships and the failure to specify the conditions that affect them.

One would expect, for example, that the relationship between efficacy and political trust would be a critical determinant of whether one acts politically and of the direction that action takes. Indeed, in the 1972 presidential election, there was an 89 per cent turnout among the efficacious and trusting, while only 62 per cent of those who felt inefficacious and distrusting voted.[10] Furthermore, whereas only 21 per cent of the efficacious and trusting respondents cast a Democratic vote for president, 49 per cent of the inefficacious and distrusting voted Democratic.[11] Both the rate of turnout and percentage of Democratic vote figures were different than would have been expected when related with efficacy or trust singly.

The 1972 election study also demonstrated that the relationship between distrust of government and the vote is not a simple linear relationship but one heavily affected by political ideology. McGovern obtained 82 per cent of the vote among cynical respondents with a "leftist" ideology—15 per cent more than would normally be expected from this group.[12] Cynical respondents with a "rightist" ideology, however, deviated further *below* their normal Democratic proportion of the two-party vote than did trusting respondents with a "rightist" ideology. Political cynicism thus had a substantial independent effect on the vote among social change advocates but actually worked against McGovern among social control advocates. This result not only demonstrates how ideology conditions the effect of political

[10] For the complete relationship among the four variables (political efficacy, political trust, turnout and two-party vote), see Table 4 of Arthur H. Miller, Warren E. Miller, Alden S. Raine and Thad A. Brown, "A Majority Party in Disarray: Policy Polarization in the 1972 Election," (paper presented at the 1973 Annual Meeting of the American Political Science Association, New Orleans, Louisiana, September 4–8).

[11] Miller, Miller, Raine and Brown, Table 4.

[12] For a detailed discussion of these data, see Miller, Miller, Raine and Brown, pp. 47–53. The concept of the normal vote is discussed in Philip E. Converse, "The Concept of a Normal Vote" in Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes, *Elections and the Political Order* (New York: John Wiley and Sons, 1966), pp. 9–39.

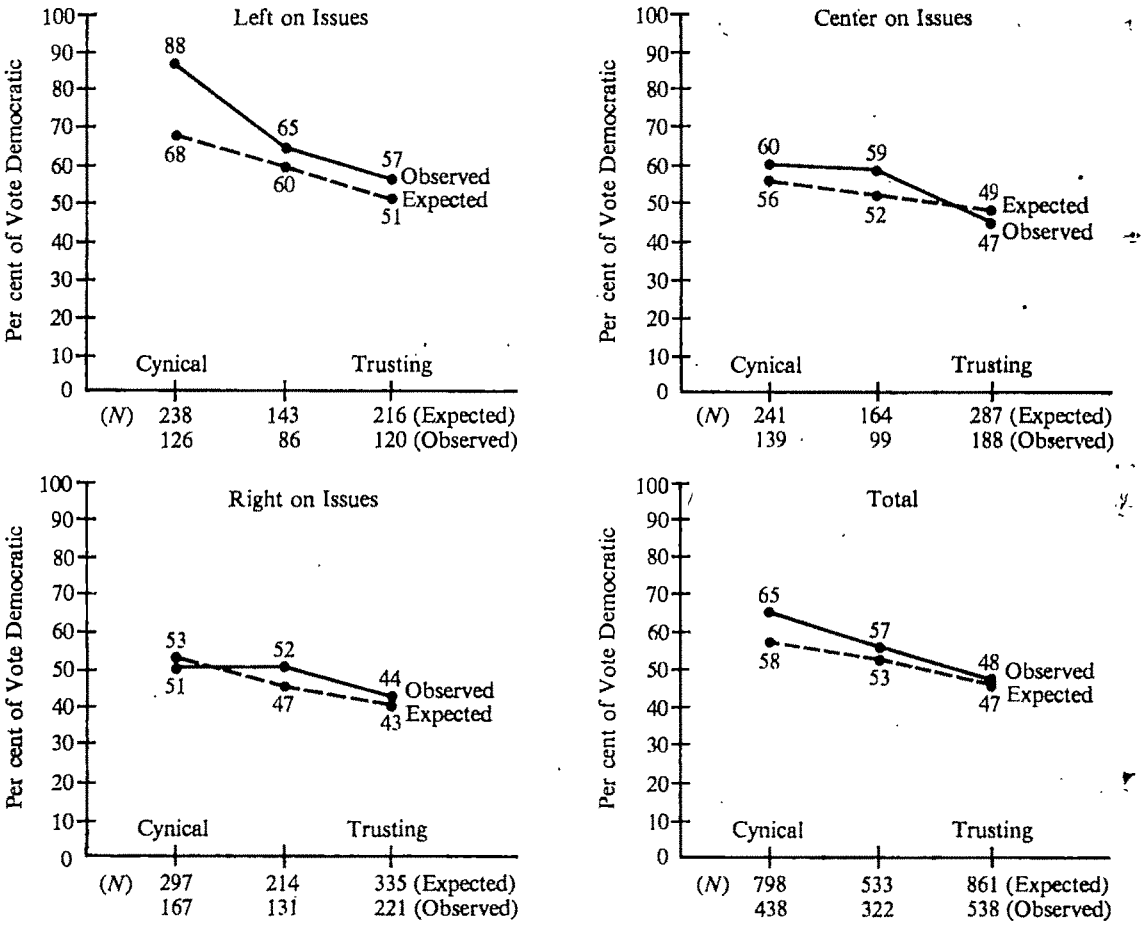


Figure 2. Expected Vote and 1972 Congressional Vote by Political Trust and Ideology

disaffection, but it also refutes the notion that distrust is simply directed at incumbent authorities.

It might, of course, be argued that this independent effect of political distrust on the vote among liberals is merely a function of the presidential race and the candidate personalities. To test this hypothesis, the above analysis was replicated with the 1972 congressional vote. The results (see Figure 2) demonstrate that for the total population cynics voted only slightly more Democratic than would normally be expected. Dividing the total population into ideological subgroups, however, results in a substantial augmentation of the independent effect of political distrust on the vote. Again, this effect appears primarily among liberals, thereby substantiating the hypothesis that ideology conditions the behavioral consequences of political disaffection.

Political distrust was also found to have had a direct impact on the voter's image of the candidates in the 1972 election.[13] Yet even this relationship may be conditioned by ideological and racial effects. Displayed in Table 3 are some data that contradict the hypothesis that " . . . political cynicism reflects a sense of identification with critics of the status quo."[14] George Wallace is surely a critic of the status quo, and yet it is quite obvious that among whites his affective ratings depended upon ideological orientations, not political trust. Among blacks, the Wallace ratings did differ by degree of political trust, but the differences are exactly opposite to those predicted by the hypothesis. In addition, black ratings of McGovern were uniformly high regardless of ideology and degree of political trust (his increased rating by black cynics of the left was not significantly higher). The relationship between political trust and evaluations of political leaders is clearly influenced by racial differences. Furthermore, the effect that ideology has on this relationship is suggested by the somewhat larger differences in McGovern and Nixon ratings by level of trust for

[13] This relationship is discussed in Miller, Miller, Raine and Brown, p. 64.

[14] Jack Citrin, "Comment," this issue of the *Review*, p. 976.

Table 3. Mean Ratings of Political Leaders by Race, Political Ideology and Trust in Government, 1972

	WHITES			
	Social Change Ideology		Social Control Ideology	
	Trusting ($N=194$)	Cynical ($N=163$)	Trusting ($N=330$)	Cynical ($N=295$)
Political Leaders				
George Wallace	37	36	62	64
George McGovern	56	68	35	42
Richard Nixon	62	43	78	65
	BLACKS			
	Social Change Ideology		Social Control Ideology	
	Trusting ($N=34$)	Cynical ($N=88$)	Trusting ($N=10$)	Cynical ($N=23$)
Political Leaders				
George Wallace	19	11	35	24
George McGovern	75	80	77	76
Richard Nixon	59	36	64	55

those whites with a social change ideology compared to those with a social control ideology.

Likewise, ideology can be expected to condition the predisposition to participate in protest behaviors. The CPS questions[15] that deal with these behaviors refer principally to protest activities acceptable to individuals with a social change orientation. We thus find large differences in responses to these items by ideological orientation (see Table 4). Given that these types of protest behaviors are more acceptable to social change advocates, one would expect stronger relationships between the protest items and political cynicism for that ideological group than for those with a social control orientation. This is clearly confirmed by the data of Table 4.

The relationship between political cynicism and support for protest behaviors that is found for the social change group may, of course, possibly be a "spurious reflection of the association between mistrust of government and policy dissatisfaction."[16] That is, policy dissatisfaction may be a direct cause of both political cynicism and support for protest behavior. Thus, controlling for policy dissatisfaction would cause the relationship between cynicism and attitudes toward protest behavior to vanish. Indeed, this is what Citrin found when ideological orientations were ignored and the total sample was analyzed. Contrary to this, controlling for policy dissatisfaction *among social change advocates* did not reduce the strength of the relationship between trust and the predisposition toward protest behavior. Furthermore, the additional control actually clarified the relationship between trust and protest attitudes for social control advocates (see Table 5). Although social control advocates who were dissatisfied with Republican policies were, as a group, only slightly less negative toward protest behaviors than those with a control ideology who were satisfied with the policies, cynicism and support for protest were more strongly related among the former group than the latter.

Tables 4 and 5 demonstrate that the relationship between political trust and support for protest actions depends on a number of conditions and interactions. For individuals with a social

[15] The political protest questions were worded as follows:

> There are many possible ways for people to show their disapproval or disagreement with governmental policies and actions. I am going to describe three such ways. We would like to know which ones you approve of as ways of showing dissatisfaction with the government, and which ones you disapprove of.
>
> 1. How about taking part in protest meetings or marches that are permitted by the local authorities? Would you approve of taking part, disapprove, or would it depend on the circumstances?
> 2. How about refusing to obey a law which one thinks is unjust, if the person feels so strongly about it that he is willing to go to jail rather than obey the law? Would you approve of a person doing that, disapprove, or would it depend on the circumstances?
> 3. Suppose all other methods have failed and the person decides to try to stop the government from going about its usual activities with sit-ins, mass meetings, demonstrations, and things like that? Would you approve of that, disapprove, or would it depend on the circumstances?

[16] Citrin, p. 982.

Table 4. The Relationship Between Political Protest and Political Trust by Ideological Orientation, 1972

	Social Change Ideology			Social Control Ideology		
	Trusting	Cynical	Total[a]	Trusting	Cynical	Total[a]
Legal Protest Marches:						
Approve	31%	44%	38%	12%	13%	12%
Depends	44	40	42	33	37	35
Disapprove	25	16	20	55	50	53
	100%	100%	100%	100%	100%	100%
(*N*)	(178)	(233)	(536)	(390)	(330)	(960)
Refusal to Obey Unjust Laws:						
Approve	22%	35%	30%	10%	13%	11%
Depends	42	44	43	33	36	35
Disapprove	36	21	27	57	51	54
	100%	100%	100%	100%	100%	100%
Disruptive Sit-Ins, Demonstrations:						
Approve	9%	23%	16%	4%	6%	5%
Depends	46	46	46	20	26	22
Disapprove	45	31	38	76	68	73
	100%	100%	100%	100%	100%	100%

[a] Total distribution is for all cases with the specified ideological orientation including those in the middle category of trust. The center category on the trust scale was excluded from the table because the relationships are linear.

change ideology, the interaction of the ideological orientation and distrust of the government was sufficient to result in positive predispositions toward protest behavior. For those with a social control ideology, however, a moderate degree of policy dissatisfaction was also needed before distrust of the government was translated into a more positive attitude toward protest actions. Most assuredly, dissatisfied "cynics of the right" were not as favorably disposed toward protest participation as dissatisfied "cynics of the left," but they were more supportive of illegal protest behaviors than their ideological counterparts who were satisfied with Republican policies.

There are numerous other social and psychological conditions that can be expected to affect the trust/behavior relationship. The preliminary analyses presented here demonstrate that a superficial investigation of these complex relations would only serve to confuse the issue. Perhaps the brief analysis of this "Rejoinder" will stimulate greater concern for those conditions that affect the translation of political disaffection into political behaviors.

Political Trust and Public Policy

Political disaffection is a complex phenomenon that almost certainly depends on the convergence of different explanations rather than on one single cause. Thus, while policy dissatisfaction has been isolated as a strong correlate of political cynicism, no one would claim that it is the only explanation of political distrust. Yet, in an age when government policy impinges directly on the citizen's life in so many ways, it seems reasonable that public policy itself would weigh heavily as a determinant of discontent. Furthermore, in a period of increased issue politics and issue ideology it is not surprising that individuals would evaluate government performance with respect to a broad range of public policies. This relationship between judgments of governmental policy performance and distrust is affected by a number of social and political conditions which were discussed throughout "Political Issues and Trust in Government." It seems appropriate, however, to state some of these conditions more explicitly, especially as they apply to dissatisfaction with the policies of both parties.

Policy dissatisfaction may be conceptualized as a function of the discrepancy between one's own policy preference and the policy alternatives offered by political authorities on salient issues. Under this definition, an individual may prefer centrist policies and yet be dissatisfied if the policy alternatives he identifies with the authorities are either more liberal or more conservative than he prefers. Policy dissatisfaction, in other

words, is not simply dependent on the individual's own policy position. In a polarized society where the political authorities are perceived as offering centrist policies we should find, however, that both "liberals" and "conservatives" are more dissatisfied than "moderates." If there is a correlation between policy dissatisfaction and political cynicism, we should also expect these groups to display similar relative degrees of distrust. These relative degrees of aggregate dissatisfaction and distrust will, therefore, be a function of the extent to which the various sets of authorities offer viable policy alternatives. For example, if Democrats and Republicans offer nearly identical policy alternatives, dissatisfaction and discontent should be directed equally at both of them.

Generally, the relationship between dissatisfaction with the policies of both parties and distrust of government will be affected by several factors: the saliency and relative importance of particular issues, the degree of polarization on each issue, and the relative acceptability of the policy alternatives offered by each party. The hypothesis proffered by Citrin that policy satisfaction and political trust are synonymous with support for the incumbent president and that changes in the partisan focus of distrust reflect "out-of-power" disgruntlement can also be considered an explanation of the relationship.

The attempted replication of Table 8 from "Political Issues and Trust in Government" in the "Comment" demonstrates that in 1972 political cynicism was more strongly related to dissatisfaction with Republican than Democratic policies. In 1970 dissatisfaction with Democratic policy alternatives had been almost as strongly correlated with cynicism as was dissatisfaction with Republican policies. The change in this relationship can be partially understood by disaggregating the sample into the racial and partisan subgroups analyzed in Tables 6 and 7 of "Political Issues and Trust in Government." Mean cynicism by dissatisfaction with the policy alternatives identified with the Democratic and Republican parties is presented for these groups in Table 6.

The data of Table 6 demonstrate that the reduction in the relationship between dissatisfaction with Democratic policies and cynicism occurred primarily for Republicans and Independents. The 1972 relationship for white Democrats, however, was only slightly weaker than that found in 1970. Similarly, while there was a reduction from 1970 to 1972 in the relationship for black Democrats, the correlation remained moderately strong and almost equal to that found for dissatisfaction with Republican policies.

Apparently, the policies of the Democratic party had become less viable between 1970 and

Table 5. The Relationship Between Political Protest and Political Trust by Policy Dissatisfaction for Social Control Ideologues Only, 1972

	Satisfied with Policies			Dissatisfied with Policies		
	Trusting	Cynical	Total[a]	Trusting	Cynical	Total[a]
Legal Protest Marches:						
Approve	14%	14%	14%	9%	15%	12%
Depends	36	37	37	30	38	36
Disapprove	50	49	49	61	47	52
	100%	100%	100%	100%	100%	100%
(*N*)	(195)	(102)	(409)	(94)	(126)	(295)
Refusal to Obey Unjust Laws:						
Approve	8%	10%	9%	5%	16%	10%
Depends	32	33	33	34	39	38
Disapprove	60	57	58	61	45	52
	100%	100%	100%	100%	100%	100%
Disruptive Sit-ins, Demonstrations:						
Approve	3%	8%	5%	4%	9%	6%
Depends	16	15	16	20	22	22
Disapprove	81	77	79	76	69	72
	100%	100%	100%	100%	100%	100%

[a] The total is for all cases with the specified ideological orientation and level of policy satisfaction.

Table 6. Mean Cynicism by Dissatisfaction with Democratic and Republican Policies

	White Democrats		Black Democrats		Republicans		Independents	
	Mean[a]	%	Mean	%	Mean	%	Mean	%
Distance from Democratic Party								
0	1.98	(11.4)	4.00	(16.1)	2.27	(6.0)	2.36	(7.9)
1	2.45	(44.0)	3.19	(46.2)	1.89	(29.1)	2.46	(37.2)
2	2.40	(27.8)	3.60	(31.3)	1.97	(34.8)	2.35	(31.4)
3	2.78	(11.0)	4.35	(6.4)	2.17	(17.5)	2.42	(16.3)
4–6	2.66	(5.8)	[b]	—	2.40	(12.6)	2.63	(7.2)
Group Mean and Total Percent	2.51	100.0	3.59	100.0	2.10	100.0	2.45	100.0
(*N*)	(909)		(180)		(612)		(874)	
Eta	.15		.24		.09		.04	
Distance from Republican Party								
0	2.00	(13.0)	[b]	(0.8)	1.76	(26.5)	1.44	(12.6)
1	2.04	(31.7)	2.67	(11.5)	1.92	(49.0)	2.14	(39.8)
2	2.54	(31.0)	3.25	(23.6)	2.73	(17.6)	2.51	(26.8)
3	2.89	(13.6)	3.59	(21.1)	2.11	(5.9)	3.06	(11.9)
4–6	3.71	(10.7)	3.86	(43.0)	3.25	(1.0)	3.66	(8.9)
Group Mean and Total Percent	2.51	100.0	3.59	100.0	2.10	100.0	2.45	100.0
(*N*)	(909)		(180)		(612)		(874)	
Eta	.29		.32		.20		.35	

[a] The cynicism scale ranged from 0 = least cynical to 5 = most cynical.
[b] *N* was less than 5.

1972 as alternatives to Republican policies for Republicans and Independents. These groups were slightly less satisfied with Democratic policy alternatives in 1972 than they had been in 1970 and slightly more satisfied with Republican alternatives, resulting in higher trust, on the average, for Republicans and Independents. Both groups perceived the Democratic party as further *left* on the issues in 1972 than either group had perceived them in 1970.[17] With the leftward shift of Democratic policy alternatives, both parties were thus no longer equally centrist and Democratic alternatives therefore became less meaningful than Republican alternatives as a reference for policy judgments among Republicans and Independents. In this regard it is most important to note that from 1970 to 1972 the association between dissatisfaction with Republican—or Nixon's—policies and political cynicism remained basically unchanged for Republicans, not surprisingly since that party is the focus of their partisan identification. The correlation increased, however, for Independents—as would have been predicted by the shifting reference hypothesis. These data support the "viability of alternatives" argument and illustrate the weakness of the "out-party" disgruntlement hypothesis, namely, that it does not explain distrust among Republicans.

Further evidence of change in the conditions that would affect the relationship between policy dissatisfaction and cynicism comes from attitudinal shifts occurring among Democrats. Between 1970 and 1972 Democratic attitudes became somewhat more liberal on a number of important issues, especially the Vietnam war. These shifts did not, however, parallel the change in the perception of the Democratic policy alternatives,

[17] The average location of the Democratic party on the five issues of Vietnam, Urban Unrest, Campus Unrest, Protecting the Rights of the Accused, and Government Aid to Minorities was 3.49 in 1970 and 3.25 in 1972 for the total population. In 1970, Independents had placed the Democratic party, on the average, at 3.58 and in 1972 they located it at 3.22. Republicans placed the Democratic party at 3.20 in 1970 and 3.02 in 1972. For a further discussion of these shifts, see Miller, Miller, Raine and Brown, pp. 13–16.

nor did they eliminate the profound issue polarization that had existed in Democratic ranks since 1968. In fact, the net result of these aggregate shifts was, for Democrats, an increased dissatisfaction with Democratic party policy alternatives. Black Democrats, in particular, exhibited a substantially higher degree of dissatisfaction with Democratic policies in 1972 than they had in 1970. They also registered a large increase in dissatisfaction with Republican policies and an aggregate *increase* in political distrust. Among white Democrats, on the other hand, there was a decrease in satisfaction with Democratic policies, a slight enlargement in the proportion most satisfied with Republican policies, and no change at all in the aggregate level of distrust. In general, then, aggregate shifts in mean cynicism coincided closely with aggregate changes in policy dissatisfaction.

The strength of the 1972 relationship between dissatisfaction with Democratic policies and cynicism was only slightly weaker for white Democrats than it had been in 1970, and there was no noticeable change at all in the relationship with the Republican policy dissatisfaction measure. Similarly, for black Democrats it was not surprising to find that cynicism was still related to dissatisfaction with Democratic *and* Republican policies since for them the policies of both parties had declined in acceptablity. The strength of the relationships had, however, decreased from 1970.[18] Despite these slight fluctuations, the relationships remained significant demonstrating the importance of dissatisfaction with Democratic policy alternatives as a source of political discontent for certain subpopulations.

Fluctuations in the strength and partisan focus of these relationships may also reflect the increased importance of other issues or factors having an independent effect on political trust. The relative saliency and importance of various political issues may influence the relationship between policy dissatisfaction and political trust.

[18] Other factors, such as increased group identification among blacks, were substantially affecting black cynicism in 1972. Blacks who were not necessarily dissatisfied with the particular policy items analyzed were becoming distrusting for other reasons, thereby reducing the clarity and linearity of the policy dissatisfaction/cynicism relationship. The curvilinearity of the 1972 relationship between dissatisfaction with Democratic policy alternatives and trust partially reflects strength of party identification and the associated desire to replace Republican policies with Democratic policies. The curvilinearity also results, however, from perceived discrimination and patterns of social integration among blacks. For a discussion of these sources of black political cynicism, see Arthur H. Miller, Alden S. Raine and Thad A. Brown, "Racial Trends in Political Estrangement, 1958–1972," (unpublished paper, The University of Michigan, Center for Political Studies, 1974).

Table 7. Regression Analysis Predicting Political Cynicism, 1972

Independent Variables	Beta
Dissatisfaction with administration policies regarding:	
Vietnam	.11
Pollution	.01
Urban Unrest	.07
Campus Unrest	.07
Rights of Accused	.07
Assistance to Minorities	.02
The Economy[a]	.26
Nixon thermometer rating	.10
Party identification	.02
	$R^2 = .24$

[a] This is a restricted variable in the 1972 election study. The wording of the question was "How do you feel about what our government is doing about the economy—jobs, prices, profits?" The responses ranged from 1 = Delighted to 7 = Terrible.

For example, by 1972 the Vietnam war had, at least for Democrats, become "Mr. Nixon's" war —a change that could be expected to alter the partisan focus of policy dissatisfaction. Similarly, if trust in government was primarily a statement of support for the incumbent authorities—in this case President Nixon—we might expect an increased partisan effect during presidential campaigns. Some brief but very persuasive data relevant to these points are presented in Table 7.

The standardized regression coefficients presented in Table 7 clearly demonstrate that although they are intercorrelated, both support for the President and policy evaluations are independent predictors of political trust.[19] Obviously, policy dissatisfaction is not synonymous with presidential support, nor is it simply a statement of partisan identification. Likewise, trust in government is not just a function of presidental approval or party identification; it is, rather, primarily associated with policy evaluations. The policy performance and dissatisfaction measures alone can account for 20 per cent of the variance

[19] The Nixon thermometer rating, rather than the performance evaluation item Citrin used in his Table 2, was used for the regression analysis because the performance question was only asked of half the 1972 sample and thus could not be correlated with all of the issue measures included in the regression analysis. It should be noted that the Nixon thermometer and the performance evaluation questions were strongly correlated ($r = .68$) for that half-sample that presented both items; and the zero-order correlations of each item with trust were nearly identical (.31 and .32, respectively). It is therefore unlikely that the regression results would have differed had the performance question been used in place of the thermometer.

in political trust. At the zero-order level Nixon's rating, however, explains less than ten per cent of the variance in trust ($r = .31$), and it adds less than four per cent of explained variance to the multiple regression.

Clearly, support for Nixon in 1972 was only partially dependent on the policy performance of his administration, whereas political trust was strongly affected by policy evaluations, thus making possible "the coexistence of widespread support for the President and a pervasive mistrust of 'government.'"[20] This "coexistence" of presidential support and mistrust of government cannot be explained by what Citrin contends is a "short-term rationalization of an anti-McGovern vote."[21] because the same phenomenon occurred in 1970, long before McGovern was even a candidate. Since the political focus of discontent was obviously not just the incumbent authorities, there is no inconsistency in finding the coexistence of a popular president, as Nixon was in 1972, and pervasive mistrust of government. Much more than a friendly face in the Oval office is necessary for government to be judged responsive and trustworthy.

"Out-party disgruntlement" and partisan identification appear to be weak explanations for shifts in the partisan focus of political discontent. The regression analysis of Table 7 suggests, however, that a partial explanation may be found in the relative importance of various issues. In 1970, the social issues were the primary focus of policy discontent. In 1972, dissatisfaction with the policy alternatives offered as solutions to social problems (which included Vietnam) remained an important source of distrust. At the same time, although the 1972 survey predated the energy crisis, economic problems had also become a substantial source of discontent. A regression analysis including only the 1972 social issues explained 15 per cent of the variance in trust; an additional five per cent was explained by incorporating evaluations of how well the government was handling economic problems.

We would expect economic discontent that arises during the term of a particular administration to be principally directed at the performance rating of that administration. Only if the economic dissatisfaction persisted across different administrations or became part of a generalized policy dissatisfaction would we expect economic discontent to be directed at both parties equally. An increase in the relative importance of economic dissatisfaction between 1970 and 1972 may thus be a partial explanation for the shift in the partisan focus of political discontent that occurred during that period.

In summary, the parties were perceived as having shifted in their issue positions so that they were no longer seen as equally centrist; there had been a slight reduction in the polarization of the *total* population on some issues; and the relative importance of various issues had apparently changed. These factors ostensibly affected the partisan focus of policy dissatisfaction and, in turn, the relationship between policy judgments and political trust. A decline in the relationship between dissatisfaction with Democratic policy alternatives and political cynicism was, therefore, not unexpected. Furthermore, some groups still felt that the policies of both parties were somewhat unacceptable, and for them dissatisfaction with the policies of *both* parties was still related to distrust. This further analysis, therefore, confirms the conditions previously hypothesized in "Political Issues and Trust in Government" as affecting the relationship between policy dissatisfaction and political cynicism. More importantly, the analysis reconfirms policy dissatisfaction as a major source of political discontent.

Conclusion

Professor Citrin has suggested that declining trust is just a reflection of dissatisfaction with the incumbent; that political trust should correlate with political action but that the trust index does not; and that centrist policies are not a cause of political distrust. The analysis presented in this "Rejoinder" demonstrates, however, that while distrust of the incumbent authorities exists, other forms of discontent predominate and a lack of citizen fervor to replace the regime does not mean that distrust is ritualistic. Furthermore, the trust scale does correlate with political actions and support for illegal protest behaviors under appropriate conditions, but care must be taken to apply the relevant controls. Simple relationships may not be present, but relationships emerge when properly examined. Finally, the change between 1970 and 1972 in the relationship between policy dissatisfaction and trust is due to the Democrats being *perceived* as less centrist, which explains why trust no longer correlates with dissatisfaction with their policy alternatives. Again, careful indication of specific control conditions are essential to clarify the relationship. In sum, the analysis results demonstrated the validity of the trust in government scale as a meaningful measure of political discontent. Moreover, they also confirmed the political relevance, importance and validity of the ideological orientations specified in "Political Issues and Trust in Government." Ignoring these orientations not only detracts from our recognition of the serious social and political implications of distrust, but it also limits our comprehension of the behavioral implications of political discontent.

A little common sense and a great deal of data

[20] Citrin, "Comment," pp. 976–978.
[21] Citrin, "Comment," p. 978.

suggests that there is much discontent with numerous facets of government in the U.S. today. To conclude, therefore, that a ten-year trend of declining confidence in government indicates little more than a shift in superficial or "ritualistic" verbal responses is not to just call into question particular survey methods. To do so is, rather, to ignore the political turmoil that has polarized this country since the mid-sixties, and, more importantly, it suggests that the turbulence and distrust spring from superficial rather than real social problems. The civil rights movement, the peace movement, and the women's movement express a discontent that grows out of repressive and discriminatory practices which require institutional and structural changes if permanent rectification of the situation is to be attained. Both positive attitudes towards these movements and negative reactions to them are related to distrust of government; to deem these attitudes and reactions "ritualistic" is tantamount to treating the events, experiences and evaluations of government performance that cause the distrust as fictions and abstractions.

More extensive work is needed if we are to fully explain how individual experience is translated into societal and political discontent. This is a timely and relevant problem that deserves serious consideration. A more complete understanding of political discontent can best be attained through careful analysis of trend data. Analogies between baseball and politics are weak and rarely help us understand either, although I would remind Professor Citrin that the endless changes of baseball managers and players in which many teams engage have not brought back the fans who became disinterested in the game because of its inherent structural flaws—for example, its lack of speed. Furthermore, the visible actors in baseball have little if any impact on the rules of the game; in politics, they make the rules. A change in the political leadership, therefore, holds the potential for profound systemic political change which may in turn increase confidence in government. On the other hand, a replacement of political leaders with no subsequent improvement in the performance of the government may generate a new spiral of political distrust. As noted at the outset of this rejoinder, democracy rests upon the trust that citizens extend to their government; when that trust is undermined, the whole system of government is threatened.

Theory and Voting Research: Some Reflections on Converse's "Change in the American Electorate"

WALTER DEAN BURNHAM
Massachusetts Institute of Technology

Background to a Controversy

In his brilliant and justly famous monograph, *The Structure of Scientific Revolutions*, Thomas S. Kuhn has illuminated the sociology of scientific discovery.[1] Developing an antinomy between "normal science" and "scientific revolutions," he places great stress upon the crucial importance of the discovery of anomalous data—data which do not "fit" and cannot be made to fit in preparing the ground for the overthrow or supersession of currently dominant scientific paradigms.[2] This dialectical pattern—paradigm, anomaly, resistance, revolution—appears to dominate the history of science.

No one pretends, or should pretend, that most areas of political science, including voting-behavior research, have advanced to the point that true paradigms have been constructed, or that professional controversies over the nature of political reality have more than a closely analogous relationship to the kinds of natural-science dynamics which Kuhn has brought to our attention. Ours is a more modest enterprise than this. But the analogies seem close enough to provide a starting-point for this explicitly controversial (or controversy-oriented) paper.

In 1965 the *Review* published a paper of mine, "The Changing Shape of the American Political Universe."[3] This paper presented a theory of American electoral change, which stands in some contrast to the then-current professional emphasis on political system. Moreover, it presented data which were anomalous in light of the then-received theories of American voting behavior—many if not most derived from survey research on the contemporary electorate. Such anomalies included the steep decay in post-1900 voting behavior by nineteenth-century American or contemporary European standards, sharp increases in partisan volatility, and other indicators of growing "marginality" preceding chronologically *pari passu* with the shrinkage of the "voting universe." From this it was argued that the establishment of industrial-capitalist political hegemony after the realignment of 1896 was closely and causally associated with these remarkable movements, movements which were markedly retrograde to electoral development elsewhere in the Western world. The inference was drawn that this erosion of the mass base of American politics was a consequence of the tension between capitalism and democracy, and of the ascendancy of capitalism.

This presentation brought forth other anomalous findings in terms of contemporary survey-research theory. (1) "Landslides" in the nineteenth century tended, as did much else in the period, to be inverses of the contemporary behavioral phenomenon—they were not the product of "surges" as much as of one-sided abstentions by a normally involved partisan electorate. (2) Rural voting behavior in the contemporary period tends to reveal eccentricities in turnout and in partisan alignment. In the last century, by contrast, agrarian turnout tended to be more invariant and normally a good deal higher than in cities; rural partisan shifts were correspondingly very small except during realignment sequences. (3) The oscillatory or "sawtooth" pattern in turnout between presidential and off-year elections tended to be much smaller in relative amplitude before 1900 than is the case today. This is another way of saying that the relative net proportion of marginal or "in-and-out" voters was much smaller then than it is now, despite the much higher turnout rates in presidential elections. (4) The educational level of the American population has steadily increased over the past century. Yet despite this increase, voting participation went into a steep decline early in this century from which it has only marginally recovered. As is well known, educational level is a strong predictor of voter turnout in the contemporary era. An anomaly exists here which is disturbing to the SRC's paradigm.

This list could be extended, but it is at least indicative of the analytical problem. Perhaps the most disturbing empirical finding of this piece was that the indicators of nineteenth-century voting behavior, taken as an integrated set of characteristics, point to an electorate much more involved in democratic politics than its present counterpart, and vastly more so than that of the 1920s.

[1] Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970).

[2] *Ibid.*, pp. 52–65.

[3] *American Political Science Review*, 59 (March, 1965), 7–28.

At the very base of these anomalies and the conclusions drawn from them is the author's belief that sociological and historical contexts profoundly shape both political consciousness and political behavior. This suggests a plasticity, as well as a sense of potential cognition of political objects within a mass electorate, which is most uncongenial to the semi-paradigms of contemporary survey research.[4] In particular, such a suggestion casts serious doubt upon the generalizability of many—though far from all—survey findings based on contemporary American (and other) voters. Resistance to incorporation of these anomalies and their implications has been correspondingly strong. It has, moreover, been greatly facilitated by a crucial deficiency in the original (1965) presentation: that work failed specifically to incorporate changes in electoral mechanics after 1890, which are of obvious significance in shaping subsequent voting behavior and nonbehavior.

Now, when one is confronted with anomalies, it is possible to assimilate them into an existing paradigm in several ways. For example, those anomalies which have to do with change toward a present condition can achieve explanation through incorporation of straightforward and "undramatic" structural change unaccounted for in the original presentation of the anomaly. Similarly, those anomalies which have to do with the nature of reality before the onset of system change, in cases where their empirical reality cannot be disputed, may be explained away through hypotheses positing that they are artifacts or "optical illusions." Such efforts will, of course, be resisted by those who discover anomalies. The conflict which necessarily arises is most certainly not a matter either of ill will or of differences in intelligence: it is a matter of differences in reality perception. The conflict between anomaly and existing paradigm is a key vehicle for the development of fruitful scientific controversy, and such controversy in turn is a most valuable, indeed necessary, step in the development of better approximations to reality than any which currently exist.

Philip E. Converse has recently written an essay, "Change in the American Electorate," which has the perceptiveness and elegance of style and insight which we have come to expect from him.[5] The first half of this absorbing study consists of a detailed exposition and critique of my 1965 "Changing Shape" article and some more recent reformulations of its thesis. The differences between us, while most cordial, are profound and substantive enough to merit an examination by me of this counter-hypothesis.

Converse's hypotheses fall into two fairly discrete sets: those dealing with intervening legal or rule variables which may explain most of the observable change, and those which raise questions about the politicized character of the nineteenth-century voting universe.

The first set of hypotheses includes the intervening variables of (1) women's suffrage (1914–1920); (2) the introduction of the Australian ballot (ca. 1889–1896); and (3) the introduction of personal-registration requirements for voting (ca. 1900–1920). Since the first variable, that of women's suffrage, was mentioned and in a sense "controlled for" in the 1965 article, the only point discussed here has to do with its possible contribution to the notorious electoral volatility of the 1920s. The second has partially been discussed elsewhere in light of Jerrold Rusk's extensive review article on the subject in *The American Political Science Review* and will receive limited treatment here.[6] The third—the personal-registration requirement—will receive much closer analysis in this paper.

The second set of Converse's hypotheses, if I have read him correctly, includes: (1) the primitive level of social bookkeeping in the nineteenth century, which casts some doubt upon turnout figures themselves; (2) the widespread existence of corruption at the polls, especially that involv-

[4] It is becoming increasingly apparent that the survey-research profile of individual voters laid down by the Michigan team in *The American Voter* (New York: Wiley, 1960), and in their *Elections and the Political Order* (New York: Wiley, 1966), must be significantly modified for the post-1964 period—and on the basis of survey analysis itself. See, for example, Gerald Pomper's two articles, "Toward a More Responsible Two-Party System? What, Again?", *Journal of Politics*, 33 (November, 1971), 916–940, and "From Confusion to Clarity: Issues and American Voters, 1956–1968," *American Political Science Review*, 66 (June 1972), 415–428; also, Norman Nie, "Mass Belief Systems Revisited: Political Change and Attitude Structure" (mimeo draft, August 1972); and especially Arthur H. Miller, Warren E. Miller, Alden S. Raine and Thad A. Brown, "A Majority Party in Disarray: Social and Political Conflict in the 1972 Election" (paper presented at the 1973 meeting of the American Political Science Association). In view of these findings, which are of course congenial to the arguments made in this paper, it seems about time that we had a complete retesting or replication of *The American Voter*, and one which is relevant to the contemporary electoral scene.

[5] Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change*, ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), chapter 8 (pp. 263–337). The controversy at issue here is centered in the first half of Converse's essay, pp. 263–301.

[6] Jerrold G. Rusk, "The Effect of the Australian Ballot Reform on Split-Ticket Voting: 1876–1908," *American Political Science Review*, 64 (December, 1970), 1220–1238. Also see the exchange of views between Rusk and Burnham in this *Review*, 65 (December, 1971), 1149–1157.

ing fraudulent surplusage of votes, a corruption which is suggested to have been very widespread in rural as well as urban America before the turn of this century; (3) discussion of the causes of higher rural than urban turnout in contemporary underdeveloped countries (the Philippines and Turkey), based on survey work there, is presented as though somehow relevant to the American nineteenth-century case.[7] Let us evaluate each of these in turn, with such evidence as is currently available.

The Way Things Were? An Exploration of Three Structural-Change Hypotheses

The Influence of Personal-Registration Statutes: An Examination of Turnout Decline in Four States, 1900–1916. There is an extraordinary messiness in the analysis of many variables affecting historical American voting behavior. In the first place, since social bookkeeping in this country is in many respects primitive compared with Europe or even Canada, we are left in ignorance as to the precise number of potential voters, or even of eligibles, in any election.[8] If we are to make any sense of the data at all, it is necessary to construct after-the-fact estimates of the potential electorate at given times, controlling as best we can for large potential inflators, such as noncitizen males in states where only citizens had the right to vote. These estimates must in most cases be based on linear interpolations between decennial federal census years. Thus, in addition to the serious problem of accuracy in enumeration (most serious, apparently, in the censuses of 1870 and 1920), it seems reasonably certain that such estimates of the potential electorate are most likely to be relatively more accurate the nearer in a decade one is to the preceding or succeeding census, and least likely to be so at midpoints. Moreover, as any analyst of historical data can attest, it is not easy to identify the precise legal status of election regulations in the past. Still more problematic, once one has identified at least the approximate date and coverage, say, of personal-registration statutes, is the issue of the local effectiveness of such requirements. These aspects of American bookkeeping present such difficulty that one could well be pardoned for abandoning any effort to define change in electoral participation and its components in any way which could generate broad acceptance of the accuracy of the results. Still, best approximations are considerably better than nothing, even if they are relatively crude; and one may hope that whatever errors exist tend randomly to cancel themselves out.

With all of these limitations, the initial structure of the personal-registration legislation adopted at the turn of the century permits a straightforward statistical testing of the hypothesis that it accounted for the lion's share of the post-1900 decline in American voting participation. Such laws, of course, must themselves be integrated into a conceptual pattern of system change in response to stress: they, too, represent change produced by concrete men acting out their ideological biases in concrete legislative form. This is nowhere more evident than in the fact that, in a great many states, the first such laws were applied initially against the populations of cities while nonurban areas were left without such coverage.[9] But this pinpointed registration coverage permits us to decompose changes in the turnout rate by county and thus to establish some relatively satisfactory measures of differential change. Indeed, it permits us to make a first, if crude, approximation to the probable relative contribution of personal-registration requirements to post-1900

[7] Converse, "Change in the American Electorate," pp. 287–288.

[8] To take but two *contemporary* examples of this problem: As of the 1970 election, there were no personal-registration requirements at all in 37 (or 42.0 per cent) of Ohio's 88 counties, partial registration for small cities in another ten (11.4 per cent), and countywide registration in the remaining 41 (46.6 per cent). In such a case, of course, no uniform standard exists even at the level of enumeration of electors provided by registration lists. Moreover, while Ohio gives the total number of ballots cast for its cities, it provides no such information for its county returns. In Massachusetts, on the other hand, complete information concerning the numbers of registered voters is published for each town and precinct; and the total number of votes cast, including blanks, is reported by town along with the vote for each candidate. Even in this case, of course, there is no indication of the number of potential voters—those who met legal requirements for the suffrage, but who did not register in the first place. In most European elections and in Canadian elections as well, it is possible to identify: (a) the total eligible electorate; (b) the total vote cast; (c) the number of invalid ballots cast; (d) the total valid vote cast; (e) the partisan distribution of that vote. For a more detailed discussion of heterogeneity and estimation problems in American Electoral Reporting, see the author's "Estimates of Potential Electorate," Introduction for Section Y, 1974 edition of U.S. Census Bureau, *Historical Statistics of the United States* (forthcoming).

[9] This urban/nonurban stratification has remained quite widespread down to the present day, particularly in Midwestern states with many low-density rural counties. In addition to the Ohio case mentioned in Footnote 7, it is worth noting that as late as 1960, 87 of Missouri's 115 counties had no personal-registration requirements. As a group, these counties had turnout rates about 12 per cent higher than those of the state's two major metropolitan centers. In New York, as of 1949, 10 counties had no personal-registration coverage at all, while a majority of election districts in 35 additional counties also had no such coverage. In 1967 a permanent personal-registration statute was adopted for the entire state.

Table 1. Registration and Voting Turnout: Pennsylvania Presidential Elections, 1889–1968, By Extent of 1920 Personal-Registration Coverage

	Means				Variances				
Year	M_1	M_2	M_3	M_4	V_1	V_2	V_3	V_4	$F=$
1880	90.0	91.8	93.2	91.3	23.67	24.23	30.18	34.27	0.405
1884	81.0	84.7	85.3	86.9	72.77	43.23	13.16	61.68	1.417
1888	76.7	82.0	84.0	85.6	113.86	66.05	9.93	51.48	3.253
1892	73.7	74.7	78.6	79.5	79.43	67.94	18.95	50.28	2.338
1896	82.2	83.1	86.3	85.5	49.84	70.57	5.29	36.58	1.024
1900	75.3	78.4	79.3	81.2	87.97	23.94	10.71	37.58	2.323
1904	72.9	74.2	75.6	76.5	66.96	47.59	56.68	81.12	0.538
1908	68.1	73.5	75.8	77.7	61.25	41.71	130.45	41.14	4.340
1912	61.2	67.0	66.5	70.4	31.96	24.75	76.01	32.45	5.841
1916	60.6	64.3	64.9	69.2	26.78	23.50	23.68	43.69	5.912
1920	40.4	41.3	45.2	48.2	36.67	16.45	25.25	43.65	7.099
1924	42.4	46.5	48.5	52.8	10.23	31.50	10.91	49.68	8.473
1928	58.9	63.0	64.1	65.8	24.56	25.93	79.20	47.12	2.629
1932	48.1	51.4	55.2	61.5	15.07	29.76	16.27	60.13	14.070
1936	68.5	69.5	73.9	79.9	61.13	28.86	10.38	55.15	12.048
1940	65.1	63.3	65.1	68.8	52.58	24.46	11.67	52.80	2.947
1944	57.9	55.9	55.6	58.7	41.45	13.44	13.45	44.93	1.095
1948	53.1	51.1	52.1	52.1	80.54	34.36	26.52	56.83	0.153
1952	64.4	62.9	64.8	64.3	58.60	16.63	26.37	59.22	0.210
1956	63.1	62.8	63.4	66.9	48.32	21.15	14.12	63.70	1.759
1960	69.5	68.4	70.6	71.9	49.61	18.38	20.11	52.99	1.132
1964	66.7	63.1	65.7	67.0	44.48	21.82	7.92	47.69	1.455
1968	64.6	61.2	65.1	64.7	39.29	28.70	8.98	46.54	1.300
$N=$	8	15	7	37	d.f.= 7	14	6	36	

turnout declines in the metropolitan areas themselves. Four states have been examined by this method: Pennsylvania, New York, Ohio, and Missouri.

One major point about which we must be clear in measuring longitudinal change is that statistical measures will produce quite discrete findings depending upon the observer's analytical focus. This is not to say, with the conventional popular wisdom, that "there are lies, damned lies, and statistics," but rather that two apparently discordant readings can often be reconciled by, for example, bearing in mind the distinction between longitudinal and cross-sectional orientations. Thus, in the case of Pennsylvania, we have chosen to begin the discussion with analysis of variance. The analysis of variance is based upon a stratification of the state's 67 counties in terms of the proportion of their populations covered in 1920 by the state's 1906–1937 personal-registration requirements.[10] (Tables 1 and 2).

[10] Pennsylvania is a typical example of the personal-registration transition which is occurring gradually in Ohio and has been completed in New York.

While there are some occasional "sports" in the presidential-election array, the values of the F ratio are generally gratifyingly high for the 1908–1936 period, that covered by the 1906 statute. Moreover, these values tend to show longitudinal increase from the beginning to the end of the period. They are correspondingly lower —usually well below the level of statistical significance—both for the 1880–1904 and for the 1940–1960 periods. On the presidential level, at least, it can be taken as extremely probable that selective personal-registration requirements did produce a statistically significant effect in differentially depressing turnout in the state's urban areas. There is very little doubt that similar analysis-of-variance tests performed for New York, Ohio, and Missouri would show comparable results. So we

The 1906 personal-registration statute applied these requirements to cities of the third class and larger, and was replaced by a statewide registration requirement in 1937. For text of the 1906 law, see any volume of *The Pennsylvania (Smull's) Manual* between 1907 and 1935. For text of the subsequent statute, see the 1937 or succeeding volumes of this publication.

Table 2. Registration and Voting Turnout: Pennsylvania Gubernatorial Elections, 1890–1918, By Extent of 1920 Personal-Registration Coverage

	Means				Variances				
	M_1	M_2	M_3	M_4	V_1	V_2	V_3	V_4	$F=$
1890	69.4	73.2	77.7	79.1	18.33	18.41	5.60	59.72	4.273*
1894	66.6	70.0	72.2	74.4	16.62	32.23	4.90	73.10	1.666
1898	62.4	67.4	69.2	72.3	14.65	20.95*	7.29	62.15	3.751
1902	65.1	66.2	68.3	71.3	11.21	21.67	9.93	68.99	1.976
1906	54.1	59.1	60.3	63.7	13.89	21.27	9.95	74.18	2.721
1910	50.6	53.8	56.2	55.8	12.23	13.83	8.64	50.22	1.570
1914	50.7	56.5	59.5	61.6	7.96	15.88	9.18	48.37	7.525
1918	38.0	43.8	43.8	48.7	8.75	13.06	6.02	57.05	6.513
$N=$	8	15	7	37	d.f.= 7	14	6	36	

Throughout this period, d.f. = 3, 63. Values of the *F*-ratio at the several levels of significance with d.f. = 3, 63 are:

level	*F*
.05	2.76
.01	4.13
.001	6.17

Note: The stratification is based on registration coverage as of 1920. M_1 and V_1 are derived from counties 50% or more of whose populations were covered by personal-registration requirements. M_2 and V_2 correspond to counties 20.0 to 49.9% of whose populations were so covered; M_3 and V_3 correspond to counties 0.1 to 19.9% of whose populations were so covered; and M_4 and V_4 correspond to counties which were left unaffected by the 1906 statute (37 in all).

certainly have an intervening variable of statistical significance for subsequent outcomes. But cross-sectional analysis of variance does not tell us anything about *trends* in any of the categories analyzed. How large a share of the post-1900 turnout decline can be attributed to this variable? How important is it?

Considerations of length preclude an elaborate discussion of Pennsylvania's party history from 1890 through 1932, but we may wish to begin analysis of this question with the Pennsylvania gubernatorial data of Table 2. This is a much shorter analysis-of-variance series, but the table contains a number of obvious anomalies, and masks others. Despite the 1906 statute, the values of the F ratio do not climb to the significant range until the third election after its enactment, 1914. Indeed, the 1910 value is the lowest of the entire 1890–1918 series. The explanation of this phenomenon ties in closely with Converse's discussion. Let us summarize.

(1) Presidential years were generally years of harmony within the hegemonic Republic majority after 1896, largely because no really important statewide offices were at stake. (2) The profile of off-year electoral politics between 1898 and the First World War was one of extreme party fragmentation and massive but unsuccessful uprisings of Progressives against the machine Regulars who controlled the state's political process. (3) Within Philadelphia particularly, the 1906 registration statute utterly failed to curb fraudulent vote surplusage during the locally important off-year elections. In fact, methods of controlling votes reached new heights of sophistication and comprehensiveness precisely during this period. One result of this was that, in the 1902–1918 period, this city—which was 100 per cent covered by the 1906 statute—had a higher turnout rate in all elections but one than did a *majority* of the 37 counties where no personal registration was required at all[11] (Table 3).

[11] The effectiveness of machine manipulation of the vote in Philadelphia can best be appreciated by examining turnout and voting in two wards: the 12th, a river ward populated heavily by immigrants and solidly controlled by the city Republican organization, and the 21st, a middle-to-upper class and politically independent suburban ward.

A. Turnout and Machine Control in Philadelphia, 1922–1930

Year and Office	Ward 12 Turnout	%D	Ward 21 Turnout	%D	City Turnout	%D
1922-Gov.	61.8	6.6	30.7	28.8	31.7	20.5
1926-Pres.	74.2	3.8	47.3	15.7	43.0	12.1
1926-Sen.	78.2	1.7	37.4	29.4	35.9	19.1
1928-Pres.	63.9	28.3	78.6	35.8	63.2	39.5
1930-Gov.	61.6	96.5	49.9	60.1	41.2	75.6
1930-Sen.	61.6	6.8	49.9	23.6	41.2	21.0

Another major anomaly can be sensed in the array of Table 2, and it is important enough to require further discussion. The strenuous state conflicts of 1902–1914 pitted insurgent or Progressive Republicans against organization Regulars. The Democratic party as such was a hopeless minority throughout the post-1896 period and played an ambiguous role. In 1910, the high-water mark of local Progressive insurgency, it fielded a candidate at the behest of the Republican state machine to siphon off enough votes to permit the organization's candidate for governor to eke out a plurality.[12] On other occasions, 1906 and 1914, the party fused unsuccessfully with insurgent Republican fragments.

One could expect in this situation that elections would be locally most competitive in areas with partial or no registration, with the machine producing lopsided organization majorities in the big cities to win statewide victory. The partisan data reveal that this was exactly the case (Table 4).

We can accept that a strong machine could overcome the intentions of a personal-registration statute. But the literature also emphasizes that closeness of competition in elections tends to facilitate turnout. Here we find the opposite. Despite the closeness of contests in the nonregistration areas—indeed, all four elections from 1902 through 1914 revealed opposition *pluralities* in these counties—their decline in turnout was huge and, except for a small upswing in 1914, continuous from 1890 onward. In fact, the turnout decline accelerated markedly in 1906 and 1910, despite the intensity of local party competition, and opposition success in most of these counties. If we take all eight of the counties, more than half of whose populations were subject to registration requirements, and compare their negative 1890–1918 regression slope with that of the 37 nonregistration counties, we find that the steepness of the former is only 14.5 per cent greater than the latter on a standardized measure.

It is also worth noting that this gubernatorial regression line for the nonregistration counties of Pennsylvania is steeper than the 1900–1916 presidential election regression slopes for any category of counties in any other of our three states except for New York City; and that city, in addition to being 100 per cent covered by per-

Statewide turnout rates during this period were 32.5 per cent in 1922, 45.8 per cent in 1924, 30.9 per cent in 1926, 62.6 per cent in 1928 and 40.3 per cent in 1930. The inversion of usual criteria of turnout—especially extreme nationally in the 1920s—is as remarkable in the contrast between lower-class Ward 12 and upper-middle class Ward 21 as in the extraordinary control of partisan outcomes in the former. Without further data presentation, it is also possible to make one more assertion: the decline in turnout among middle-class "independent" wards such as 21 from 1890 through 1930 was vastly heavier than in the lower-class wards which the machine controlled, in the former case approximating the rate of turnout decay in the state's 37 nonregistration counties. If the 1906 statute was designed to protect middle-class and native-stock interests against the big-city machines and their ethnic clienteles, it quite failed to achieve its purpose. In any event, there is a striking parallelism between post-1890 declines in most of those wards of Philadelphia where anti-organization sentiment was strongest and similar behavior in the nonregistration territories of the state at large. Once again—and forcefully—it suggests the intervention of a behavioral variable which can plausibly be linked to political alienation.

[12] One notes the relevant parts of the Keystone (Progressive-Republican) party platform of 1910: "A political trust, managed by cunning politicians, threatens the Commonwealth. Some of the conspirators are labeled Republicans and some Democrats, but they are all in league against the people and act in harmony with one treasonable purpose and under the orders on one head. . . . Both of the tickets nominated and both of the platforms adopted, it is well understood, were dictated by the same authority and were intended for the delusion of the voters and the further confirmation of the power of the political machine." *The Pennsylvania (Smull's) Manual,* 1911, p. 520. It is perhaps also worth noting that the Socialist party reached its off-year apogee of 5.3 per cent of the total vote in this election, with the Keystone candidate winning 38.3 per cent, the Democrat 13.0 per cent and the Republican Organization's candidate 41.6 per cent.

Table 3. Extreme Cases: Philadelphia and Non-Registration County Turnout Comparisons, 1890–1918

Year	Number of Non-Registration Counties with Turnout: Lower than Philadelphia	Higher than Philadelphia	% of *N-R* Counties with Lower Turnout than Philadelphia
1890	9	28	24.3
1894	15	22	40.5
1898	5	32	13.5
1902	22	15	59.5
1906	19	18	51.4
1910	33	4	89.2
1914	17	20	45.9
1918	20	17	54.1

Table 4. Partisan Distribution of Pennsylvania Gubernatorial Vote, 1890–1918, by 1920 Registration Coverage

Area and *N*	1890	1894	1898	1902	1906	1910	1914	1918
Philadelphia (1)								
Dem.	44.3	27.6	22.3	29.8	42.4	6.8	25.2	27.5
Rep.	55.2	71.1	65.1	68.9	56.1	54.4	72.6	70.7
Prog.			12.2			35.9		
Other	0.4	1.3	0.5	1.3	1.5	2.8	2.2	1.9
50.0–99% Reg. (7)								
Dem.	52.6	33.2	39.7	39.0	42.1	12.2	41.6	39.6
Rep.	45.5	61.1	42.0	56.1	51.7	44.2	48.6	53.3
Prog.			12.4			32.3		
Other	1.9	5.8	0.9	5.0	6.2	11.2	9.8	7.1
20.0–49.9% Reg. (15)								
Dem.	49.8	37.1	39.6	45.1	47.0	13.8	46.2	34.4
Rep.	47.9	57.3	45.1	48.9	48.6	38.4	47.4	59.8
Prog.			15.1			41.9		
Other	2.3	5.6	0.3	6.0	4.4	5.9	6.4	5.7
0.1–19.9% Reg. (7)								
Dem.	53.5	39.3	41.7	46.3	45.9	12.7	44.9	37.9
Rep.	44.4	55.5	42.1	46.2	48.2	33.7	47.2	56.7
Prog.			15.5			40.5		
Other	2.1	5.5	0.7	7.6	6.0	13.0	7.9	5.4
No registration (37)								
Dem.	51.3	38.3	41.6	48.1	49.6	20.1	48.3	32.2
Rep.	46.6	56.2	45.4	47.3	46.5	32.2	46.0	61.1
Prog.			12.9			41.7		
Other	2.1	5.5	0.1	4.6	3.9	6.0	5.8	6.7

Counties falling in each classification:

100%: Philadelphia
50.0–99.9%: Allegheny, Berks, Erie, Lackawanna, Lawrence, Lehigh, Venango.
20.0–49.9%: Blair, Butler, Cambria, Clinton, Crawford, Dauphin, Delaware, Lancaster, Lebanon, Luzerne, Lycoming, McKean, Mercer, Northampton, York.
0.9–19.9%: Chester, Clearfield, Fayette, Northumberland, Schuylkill, Washington, Westmoreland.
0%: All other counties ($N = 37$)

sonal registration, was subject to a periodic re-registration requirement as well. The turnout decline in the rural, heavily native-stock Protestant areas of Pennsylvania not covered by the 1906 statute constitutes a major limiting case on any registration-variable hypothesis. So far as we know, it was wholly "behavioral," not an artifact of rules change. Somehow, despite the intensity of insurgent activity, the terms of politics had been redefined from those of the late nineteenth century in such a way as to discourage participation in elections. What other barriers stood in the way for these voters?

From this point we can move to an examination of the longitudinal trends of the 1900–1915 period in our other states. Unfortunately, the criteria of county stratification are not entirely uniform. Accordingly, comparisons may have more limited value than the data might suggest. Still, all of these states permit some differentiation. Until the late 1960s, New York had registration provisions similar to those of Pennsylvania before 1937, and relevant volumes of the state's legislative manual provide a useful breakdown of the registration characteristics of precincts by county. Similar data are available for Ohio back at least to 1932. In the case of Missouri, the turn-of-the-century registration statute initially and for a long time afterward was for all practical purposes confined to the state's two metropolitan areas. Moreover, we are fortunate in being able to deal with secular trends of large size and some uniformity. So far as county-by-county analysis of 1900–1916 turnout rates can be made, we find no exceptions to the *fact* of decline in any of the 217 counties of New York, Ohio, and Pennsylvania during this period. There are numerous exceptions in Missouri, but these may find a subsequent and, I hope, convincing behavioral explanation.

Table 5 presents regressions on time for the

Table 5. Turnout Data for Four States, 1890–1918: Regression Metrics

I. Pennsylvania (Gubernatorial), 1890–1918				
Registration Coverage, 1920 and N		$Y_c =$	$S_{y.x} =$	$bX/a =$
0	(37)	83.75–3.98X	3.516	−.0475
0.1–19.9	(7)	82.14–4.16X	3.569	−.0507
20.0–49.9	(15)	78.46–3.82X	3.023	−.0487
50.0–100.0	(8)	75.63–4.12X	3.659	−.0544
II. Pennsylvania (Presidential), 1900–1916				
Registration Coverage, 1920 and N		$Y_c =$	$S_{y.x} =$	$bX/a =$
0	(37)	84.03–3.01X	1.745	−.0358
0.1–19.9	(7)	83.79–3.79X	2.051	−.0452
20.0–49.9	(15)	82.10–3.54X	1.191	−.0431
50.0–100.0	(8)	79.95–4.11X	1.472	−.0514
III. New York (Presidential), 1900–1916				
Mean Registration Coverage of Precincts, 1948, and N		$Y_c =$	$S_{y.x} =$	$bX/a =$
0	(10)	90.84–2.16X	1.914	−.0238
16.7	(14)	90.89–2.23X	2.256	−.0245
55.1	(33)	91.80–3.12X	2.427	−.0340
100.0	(5)*	85.31–4.25X	1.182	−.0498
IV. Ohio (Presidential), 1900–1916				
Registration category, 1932, and N		$Y_c =$	$S_{y.x} =$	$bX/a =$
None	(57)	94.44–2.26X	3.874	−.0239
Partial	(28)	93.78–3.58X	5.873	−.0382
"Full"**	(3)	88.25–3.19X	3.401	−.0361
V. Missouri (Presidential), 1900–1916				
Area and N		$Y_c =$	$S_{x.y} =$	$bX/a =$
Nonmetropolitan	(112)	84.67–0.29X	3.358	−.0034
Metropolitan***	(3)	74.68–0.72X	5.280	−.0096

* New York City. In this case, to avoid problems associated with the creation of Bronx county in 1914, the citywide total is the base.

** The three metropolitan counties, Cuyahoga, Franklin, and Hamilton. Because most suburbs and outlying areas were not initially covered, these are "relatively" full registration territories.

*** St. Louis city and county of Jackson. For all intents and purposes these were the only initial areas of personal registration in Missouri.

turnout rate during this turn-of-century transitional period, stratified by the relative extent of personal-registration coverage in groups of geographical areas (counties). In all categories and in all four states, the regression slopes are negative. But the differences among states in the slopes are considerable. Pennsylvania stands at one extreme, that of maximum decline, and Missouri reveals almost no decline at all, even in its three metropolitan counties. The other two states fall in the middle and approximately together, although somewhat closer to the Pennsylvania end of the spectrum. Indeed, a contemplation of the standardized regression slopes for these states and categories (bX/a) suggests that the *between-state* differentials are at least as significant as the registration-determined *within-state* differentials. In all of these states, to be sure, the posited relationship holds: very generally, the larger the registration coverage in a given set of counties, the steeper the negative regression slope is. But it should also be noted that the relationships are not quite monotonic either in Pennsylvania or in Ohio.

The idiosyncracies of the early personal-registration statutes make it possible to derive a first approximation of the relative contribution of the personal-registration statute to this nearly universal turnout decline. Leaving Converse's theory of apparently general rural corruption aside for the moment, the assumption is very strong that any secular trend in voting participation to be found in counties without personal-registration requirements must be attributed to behavioral factors. By definition, no known changes in electoral mechanics could be causes of such change in turnout. This being the case, let us assume for each state that the proportion of turnout decline in any area which corresponds to the rate of decline in nonregistration areas also corresponds

Table 6. A Crude Estimate of the Contribution of Personal-Registration Statutes to Turnout Decay: Pennsylvania and New York

A. Pennsylvania: Gubernatorial Elections, 1890–1918 (T to $T+7$)

Cat. of Reg. Coverage 1920	Regression line est.: "Expected" 1918	"Observed" 1918	$E(18)$–$O(18)$	T–$T+7$	$\frac{E(18)-O(18)}{T-T+7}$
0	51.95	51.95	0	27.83	0
0.1–19.9	50.78	48.83	1.95	29.15	6.7
20.0–49.9	48.60	47.87	0.73	26.76	2.7
50.0–100.0	46.57	42.71	3.86	28.80	13.4

B. Pennsylvania: Presidential Elections, 1900–1916

Cat. of Reg. Coverage	Regression line est.: "Expected" 1916	"Observed" 1916	$E(16)$–$O(16)$	T–$T+4$	$\frac{E(16)-O(16)}{T-T+4}$
0	69.0	69.0	0	12.0	0
0.1–19.9	68.2	64.8	3.4	15.2	22.4
20.0–49.9	66.9	64.4	2.5	14.2	17.6
50.0–100.0	64.6	59.4	5.2	16.4	31.7

C. New York: Presidential Elections, 1900–1916 (T to $T+4$)

Cat. of Reg. Coverage 1948 (Mean for each)	Regression line est.: "Expected" 1916	"Observed" 1916	$E(16)$–$O(16)$	T–$T+4$	$\frac{E(16)-O(16)}{T-T+4}$
0	80.04	80.04	0	8.64	0
16.7	80.02	79.74	0.28	8.92	3.1
55.1	80.04	76.20	3.84	13.48	30.8
100.0	73.16	64.06	9.10	17.00	53.5

D. Ohio: Presidential Elections, 1900–1916

Cat. of 1932 Registration Coverage and N	Regression line est.: "Expected" 1916	"Observed" 1916	$E(16)$–$O(16)$	T–$T+4$	$\frac{E(16)-O(16)}{T-T+4}$
None (57)	83.1	83.1	0	9.1	0
Partial (28)	81.3	75.9	5.4	14.3	37.8
"Full"* (3)	76.7	72.3	4.4	12.8	34.4

E. Missouri: Presidential Elections, 1900–1916

Grouping of Counties and N	Regression line est.: "Expected" 1916	"Observed" 1916	$E(16)$–$O(16)$	T–$T+4$	$\frac{E(16)-O(16)}{T-T+4}$
Metropolitan** (3)	83.2	83.2	0	1.4	0
Other (112)	73.0	71.1	1.9	2.9	63.8

* Cuyahoga, Franklin, Hamilton
** St. Louis City, St. Louis County, Jackson

to a "pure-behavioral" component of change. Thus, whatever residue is left can be provisionally attributed to the personal-registration requirement as an intervening causal variable.

The steps to be followed in analysis are these. First, the 1916 regression-line turnout value in nonregistration territory is calculated as a proportion of the 1900 value. Second, an "expected" 1916 value is computed for all other categories, based on a multiplication of this proportion by the 1900 regression value for each. Third, the differential between the "expected" 1916 value, based on the calculations in the second step, and the actual 1916 regression-line value is derived. Fourth, the absolute difference between the 1900 and 1916 values is computed by subtraction for each category, including the nonregistration category. Finally, the absolute loss on the regression line is divided into the expected-observed differential by a formula such as

$$C_r = \frac{E(T+n) - O(T+n)}{T - T + n} \times 100.$$

The resulting percentage can be described as that part of the regression-line decline in a given area

which can be attributed to factors other than the behavioral ones operative in nonregistration areas[13] (Table 6).

Most of these percentages are remarkably small. Leaving aside the Missouri metropolitan counties for the moment, the highest contribution for the 1900–1916 period is found in New York City, where the residue is 53.5 per cent. Here we deal with a case of exceptional stringency. In addition to the personal-registration requirement, the state legislature—with malice aforethought—imposed a periodic requirement as well, one which was applied to this city alone until its elimination in the 1950s. For reasons discussed above, the percentage figure in Pennsylvania gubernatorial elections within the eight most urban counties is extremely small. Otherwise, nonbehavioral variables as measured by this indicator tend (except in Missouri) to explain about one-third of the gross turnout decline in urban areas.

The Missouri case is special, and leads us to the next part of this discussion. Here we have a gratifying high figure of 63.8 per cent contribution of nonbehavioral components in the metropolitan areas of the state, but the longitudinal movement is extremely slight, falling far below the standard error of estimate for the regression equation. So small is this 1900–1916 turnout shift in Missouri that it may even be nothing more than the product of random errors in data. We are left with the question of positioning all four of our states on a continuum which will help us understand the very wide interstate differentials in our "sample"; Missouri is at one end and Pennsylvania is at the other. What kind of continuum would this be?

My original discussion of the turn-of-the-century shrinkage in the American electorate placed heavy stress upon the immense changes in partisan alignments which followed the critical-realignment sequence of 1893–1896.[14] This realignment led, as is now very well known, to several profound consequences for party politics in the United States. One of these was the destruction of competition between the major parties in many states, and a more or less pronounced movement away from close two-party competition in many others. The second, closely related to the first, was the achievement of corporation-capitalist dominance over public policy. This dominance reached its peak in the 1920s. The relationship between elite hegemony, one-party politics, and suppression of the electorate is so conspicuous in the post-1890 South as to require no further discussion here.[15] But perhaps this was a localized, if extreme, relationship. My original argument suggested otherwise, since the focus of attention was on turnout decline in non-Southern states. But perhaps personal-registration requirements vitiated the force of the party-competition argument outside of the South.

The four states we have analyzed here cannot be said to be a representative sample of post-1896 voting behavior outside of the South, though they did contain among them 36.7 per cent of the total non-Southern population in 1910. With only four composite data points, the posited relationship can only be said to be suggestive. Still, they remain of sufficient interest and visibility to merit presentation here. If one examines the mean partisan percentages for the five presidential elections before 1896, and the five after for these four states, the following pattern appears (Table 7). The relationship between changes in competitiveness and in the magnitude of the mean turnout slope is both obvious and quite close.[16]

The 1896 realignment produced very different partisan aftermaths in different states. The Democratic party was virtually destroyed in Pennsylvania. Yet it retained great post-1896 vitality in New York City; and, as Thomas Flinn and John Fenton have shown, nineteenth-century alignments were not submerged in Ohio until well into the present century.[17] In Missouri, as in the other Border States, the realignment of 1896 had partisan effects exactly the opposite from those affecting states north or south of that region. It produced close two-party competition for the first time since Reconstruction, and this intense party competition endured until the New Deal. The creation of close party competition seems closely related to the virtual absence of major

[13] It is obvious that this metric is based upon an assumption of intercategory homogeneity in every respect other than that of the personal-registration variable. Such an assumption may or may not be partially or wholly valid. More elegant techniques of decomposition are undoubtedly called for. Even so, this is a possibly useful effort at *first* approximation.

[14] Burnham, "Changing Shape," pp. 23–26. See also Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton, 1970), pp. 71–90; and E. E. Schattschneider, *The Semisovereign People* (New York: Holt, Rinehart & Winston, 1960), pp. 78–96.

[15] The best discussion still remains that of V. O. Key, Jr., *Southern Politics* (New York: Knopf, 1949). See also Paul Lewinson, *Race, Class and Party* (New York: Grosset & Dunlap, 1965 ed.).

[16] It would be closer still if the partisan means included the Democratic percentages of the total vote cast for all statewide offices in the two periods.

[17] Thomas A. Flinn, "Continuity and Change in Ohio Politics," *Journal of Politics*, vol. 24 (August, 1962), 521–544; John H. Fenton, *Midwest Politics* (New York: Holt, Rinehart & Winston, 1966), pp. 117–154.

Table 7. Shifts in Partisan Competition and Turnout: Four States

State	1876–1892 % Dem.	% Rep.	% Other	1900–1916 % Dem.	% Rep.	% Other	Mean Partisan Lead 1876–92	1900–16
Pennsylvania	45.7	51.6	2.7	34.3	60.2	5.5	3.9R	25.9R
New York	49.0	48.3	2.7	42.3	52.8	4.9	0.7D	10.5R
Ohio	47.5	50.0	2.5	43.5	51.2	4.3	2.5R	7.7R
Missouri	52.7	42.6	4.7	48.8	47.8	3.4	10.1D	1.0D

State	Net Shift in Party Competitiveness: 1876-92/1900-16	Mean Standardized Turnout Regression Slope, 1900–1916 bX/a
Pennsylvania	−22.0	−.0439
New York	−10.5	−.0330
Ohio	− 5.2	−.0327
Missouri	+ 9.1	−.0065

turnout decline in Missouri before 1920. If it be true that personal-registration qualifications in the state's metropolitan centers explained nearly two-thirds of their 1900–1916 decline, the decline there was miniscule compared even to the secular trend in nonregistration territories of New York, Ohio, and Pennsylvania. Similarly, one doubts that the relatively very steep decline in Pennsylvania's participation—and in all categories of registration coverage in its counties—is in any way accidentally related to the destruction of major-party competition there.

Let us summarize the discussion so far. Examining the four states which form our data base, we can conclude that the intervention of personal-registration requirements involved the intrusion of a highly significant intervening variable. Of course, this variable must henceforth be explicitly included in any future model of change in the American electorate after 1890. But we can also conclude, if tentatively, that it explains very much less of the decline in turnout during the first two decades of this century than might be supposed. If our inferences from the cross-time political behavior of nonregistration territories are correct, this variable contributed between one-fifth and one-third at the most to the 1900–1916 statewide turnout declines in New York, Ohio, and Pennsylvania. The extremely rapid rate of decay in Pennsylvania counties not covered by registration, particularly when studied together with the local intensity of political opposition to the Organization and the closeness of elections, would reinforce a theory of cumulative political alienation—at least for that state.

Finally, the evidence is suggestive here of the relationship between the gross effects of the 1896 realignment on party competition and the relative magnitude of turnout decline in presidential elections from 1900 through 1916. Here, Missouri seems in particular to be a limiting case, since virtually no decline occurred, while two-party competition became extremely close during the era of the fourth party system. It may also be noted that there was an enormous gap in the relative level of economic development between states such as Missouri and Pennsylvania; and, in politics, Missouri retained (and still retains) a nineteenth-century element which has disappeared in more "developed" parts of the country.

Thus, on the basis of the foregoing analysis, it must be assumed for now that the bulk of the decay in participation from the end of the last century through the enfranchisement of women must be explained in behavioral terms. The magnitude and—except for Missouri—the universality of this decay is such that "nondramatic" variables cannot begin to explain it all away. The same is almost certainly true for our other indicators of electoral decomposition. Something in the redefined agenda of politics after 1896 drove large masses of voters away from active participation in electoral politics—most conspicuously where the immediate partisan effects of the "system of 1896" were most conspicuous, as in states like Pennsylvania. Alternatively, it may well be that there was a severe decay in age-cohort inputs into the active electorate after 1900; we cannot know for sure at this stage, if ever. Whatever that something was, it was profound and system-wide—mediated, of course, by the interposition of new structural variables which were themselves the result of system stress and system adaptation. I find no difficulty on this record in adhering to the view that an explicitly urban, bourgeois elitism—partly corporate-conservative, partly Progres-

sive-"reformist"—was intimately and causally associated with these remarkable behavioral transformations.

Effects of Women's Suffrage on Electoral Decomposition in the 1920s. The preceding analysis points to the conclusion that decomposition in the American electorate had proceeded quite far by the end of the First World War. Other evidence has been presented elsewhere which points to the same conclusion.[18] If so, and if one can assume that these measures taken together are indicative of an overwhelming process of "political desocialization," it would follow that this process would have been well along toward completion before the emergence of a second intervening variable, the enfranchisement of women. It has been customary to accept the view that the addition of so quantitatively huge a variable explains subsequent anomalies in the American behavioral universe. One survey-research finding which has apparently universal generality is that women tend to be less politically conscious than men, are less "socialized" into the world of active political participation, and tend to participate less.[19] Similarly, the further decline in participation during the 1920s finds very much of its explanation by the sheer doubling of the electorate by the addition of this weakly socialized component. Even today, as Converse points out, the rate of female voting in the United States runs about 5 to 10 percentage points below male turnout.[20]

While these female political characteristics are well known, it might be useful to raise some pointed questions about the influence of women's suffrage in its early years, particularly in areas of voting behavior other than turnout. In addition to stressing its manifest importance for subsequent turnout rates, Converse evidently uses it to account in part for the peaks in such indicators as split-ticket voting and rolloff which developed in the 1920s. But does it? If the Burnham analyses of the changing American electorate in this period are valid, it could be expected that partisan decomposition and other evidences of "desocialization" were so far advanced by the 1915–1920 period that the still more weakly-socialized women would add little more to it. This would imply, of course, that most of the observable electoral volatility reached in the 1920s would have been present even if women had not received the vote until the advent of the New Deal.

One notes in the first place one very curious anomaly in statewide turnout rates during this period. In Kentucky—one of the Border States which became intensely competitive during the lifetime of the "system of 1896"—the highest recorded turnout rate in the entire period from 1920 to the present was achieved in 1920, the very first year of women's suffrage in that state.[21] But this curiosity, intriguing as it is, is also unique for statewide presidential-election turnout arrays in the 1920–1968 period. There is, however, one place in the United States where detailed and sex-stratified aggregate data are available during this transition period, and to this we now turn.[22]

Fortunately for researchers, Illinois gave partial but not complete suffrage to its women, effective with the 1915 Chicago mayoral election. As a result, detailed breakdowns of voting behavior by sex are available for Chicago from 1915 through 1920, and for the suburban towns of Cook county from 1916 through 1920. This series reveals certain attributes of female voting behavior which are well known. One which was shared in common with the behavior of women in other countries was a marked aversion to voting for leftist parties (Labor, Socialist, Farmer-Labor) by comparison with men. Another, of course, was a stratified differential in turnout. Suburban Cook county was (and is) much more native-stock, middle-class, and Republican as a whole than Chicago. Thus, the number of women voting in the city was 58.0 per cent of the number of men voting in 1915, 59.4 per cent in 1916, 58.5 per cent in 1919, and 59.7 per cent in 1920. By comparison, the female percentage in the suburbs was 69.0 per cent in 1916, and 69.2 per cent in 1920. These figures correspond to a sex differential in turnout of some 30 per cent in the city and 23 per cent in the suburbs, compared with the 5–10 per cent national differential today. So far, so good; one hardly needs to add that the turnout rates in both jurisdictions underwent a sharp decline when women were enfranchised.

[18] Burnham, *Critical Elections*, pp. 91–134, and Appendix Tables I-V.

[19] For a classic presentation of this point for mostly European data before 1937, see Herbert Tingsten, *Political Behavior* (London: P. S. King, 1937), pp. 10–78; see also Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), pp. 483–493; and Stein Rokkan, *Citizens, Elections, Parties* (New York: David McKay, 1970), pp. 385–394.

[20] Converse, "Change in the American Electorate," pp. 269–270.

[21] Kentucky's 1920 turnout was 71.8 per cent, more than 20 points above the national average, and falling midway between a mean of 81.2 per cent for the 1900–1916 period and a mean of 63.1 per cent for the period 1924–1940. It is perhaps worth noting that the 1920 presidential election was closer in Kentucky than was any subsequent election except for 1952.

[22] The sex-stratified data—available down to the precinct level in the city of Chicago—are found in the 1916, 1917, 1920, and 1921 volumes of the *Chicago Daily News Almanac*.

Table 8. Sex and Partisan Swing: Chicago and Its Suburbs, 1915–1920

Pair of years and sex	Office	Shift in % of Total Vote			Defection Ratios
		Dem.	Rep.	Other	
		I. Chicago			
1915–19 Men	Mayor	− 3.4	−20.5	+23.9	34.8 (Rep.)
1915–19 Women	Mayor	− 0.1	−18.9	+19.0	31.8 (Rep.)
1916–20 Men	President	−22.1	+18.6	+ 3.5	48.4 (Dem.)
1916–20 Women	President	−24.5	+23.0	+ 1.5	51.8 (Dem.)
		II. Suburban Cook			
1916–20 Men	President	−17.3	+15.2	+ 2.1	52.9 (Dem.)
1916–20 Women	President	−19.0	+19.8	− 0.8	58.2 (Dem.)

But the rest of the data point to very different conclusions about the electoral significance of this variable in the Chicago metropolitan area. Let us begin by a comparison by sex of the partisan swings from the 1915 to the 1919 Chicago mayoral elections, and from the 1916 to the 1920 presidential elections in both areas (Table 8).

There is no doubt that the relative velocity of pro-Republican movement from 1916 to 1920 was somewhat greater among women than among men, and greatest of all among suburban women, Nevertheless, stratified by sex, the swings are clearly of the same order of magnitude in each area. Between the two mayoral elections, indeed, the male Republican defection actually *exceeded* that for women. In absolute terms, shifts of 20 per cent or so between one election and the next are symptomatic of considerable electoral volatility by any longitudinal standard, particularly where—as here—they are not part of a critical-realignment sequence. If the mean female partisan swing in the 1920 landslide was only 16.5 per cent greater than the male swing for both major parties and in both areas, what significance should we attach to women's suffrage as an intervening variable promoting dramatic increases in electoral volatility? The difference between the 1916 and 1920 swing in the total vote and what it would have been had men alone voted can be easily computed. When this is done, the mean sex-related increment for both parties in both areas works out to 5.6 per cent (2.9 per cent for the Democrats, 8.4 per cent for the Republicans). While this variable thus produces a visible differential effect and, in the predicted direction, it is hard to get very excited about it.

The data become more interesting still when one examines split-ticket voting and rolloff, or net aggregate "ballot fatigue" (Table 9). Once again the posited relationships duly materialize. Women in each jurisdiction do reveal somewhat greater propensities to vote split-tickets than men, and rather more marked rates of ballot fatigue. But these differences within each area, particularly in split-ticket voting, are not very large. *Moreover, they pale into insignificance by comparison with the very wide differences in behavior between Chicago and its suburbs.* Again, such differential behavior is anomalous, conspicuously so where rolloff is concerned. For it might well be expected that the suburban areas—relatively affluent, native-stock, and so on—would display less rather than much more ballot fatigue. One quite orthodox hypothesis suggests itself: this may well reflect the workings of the machine in the city, and of Post-Progressive independence in the suburbs.

Whatever the explanation, one thing seems clear. The sex differential is quite small in each

Table 9. Sex and Indicators of Split-Ticket Voting and Ballot Fatigue: Chicago and Its Suburbs, 1920*

Area	Sex	Mean % D	Variance	Standard Deviation	Rolloff
Chicago	Men	29.5	28.20	5.31	3.8
Chicago	Women	29.9	33.04	5.75	5.0
Suburban Cook	Men	21.8	82.99	9.11	13.8
Suburban Cook	Women	21.4	102.22	10.11	16.2

* Based on percentage vote for 7 offices: President, U.S. Senator, Governor, State's Attorney, Recorder, Clerk, and Coroner.

area, while the absolute size of the split-ticket and ballot-fatigue indicators *among men* appears quite substantial by comparative standards, and is conspicuously so for the rolloff indicator. With data which is this seedy along the posited sex dimension of political behavior, we may well suspect that there is less than meets the eye in this famous woman-suffrage variable. It is a pity that so little relatively hard data exist pertaining to this set of issues. What we have forcefully suggests that—if these large values may be taken as evidence of weak political socialization—those women *who voted* in Cook county between 1915 and 1920 were only very marginally weaker in political socialization than men.

Moreover, it is widely agreed that female turnout did not begin to move much closer to male participation rates in the United States until the 1928–1936 realignment sequence. Accordingly, one should find further increases in these measures of decomposition during and shortly after this sequence. But precisely the opposite happened: In general, levels of split-ticket voting and ballot fatigue *declined* markedly as turnout sharply *increased.* This 1928–1936 influx of new voters was heavily concentrated among working-class and women—particularly ethnic women—voters in large industrial states like Pennsylvania and Illinois. Such voters were hitherto exceptionally weakly socialized; yet their addition to the electorate was associated with *declines*, not increases, in measures of electoral volatility. When these two data points are reviewed together, the argument that behavioral rather than statutory change was the crucially important development in the evolution of the American electorate from 1900 to 1940 seems, if anything, to be strengthened.

The Introduction of the Australian Ballot. The analytic ground entirely shifts when we turn to this posited intervening variable. Since I have recently discussed it at some length in response to a *Review* article by Professor Jerrold Rusk, commentary here will be brief.[23] Unlike the other variables discussed here, the Australian Ballot, when adopted, tended to be applied universally within each state from the beginning. Moreover, it was adopted virtually nationwide in a very short period. Massachusetts was the first state to adopt it generally, in 1889; by 1896, far more than three-quarters of the states had followed suit. Hence, the analytic strategy of subunit differentiation cannot be pursued in this case.

The basic issues at controversy concerning this variable are conceptual in nature. They boil down to the question: What are we primarily interested in measuring? In this sense, there are close analogies to the problem of analysis posed by the Pennsylvania turnout arrays in Tables 2 and 3. If one is interested in detecting the existence of a statistically significant intervening variable, one can perform a series of cross-sectional analyses of variance and compute the appropriate F-ratios for each of the election years 1 through *n*. If one is interested in examining longitudinal trends in each grouping used in the preceding analysis, the matrix must be transposed so that regressions on time can be performed for each grouping. As we have seen, the first approach yields the conclusion that turnout decline was of very large magnitude even in counties where no personal-registration requirements were introduced. Both approaches seem to be essential to describe change in this universe with any adequacy.

We are on similar conceptual if not statistical ground when we contemplate the effects of the Australian ballot. Unlike either of the other variables discussed so far, this is in the first place a crucial and universal intervening variable of the *sine qua non* variety. But in the second, it is a *permissive* variable rather than one which is deterministic or directly influential as such in shaping subsequent behavior.[24] So far as the first point is concerned, the change from the printed party-distributed ticket to the official consolidated ballot in any of its forms was by definition an indispensable precondition for large-scale ballot-splitting in the electorate. The old partisan ticket did not make it impossible for voters to split their ballots, but it unquestionably and systematically constrained such behavior in ways which the official ballot was in fact designed by its authors to overcome. This dichotomous change is of such transcendent importance that elaborate statistical demonstrations of it become almost supererogatory.

But if this intervening variable was indispensable to all that followed, it also was clearly less immediately shaping in its effects on partisan voting than either of the other two examined here. This point can, like many others, be most easily perceived by the analysis of specific and probably extreme cases. To take just one of several examples, Massachusetts was the first state to adopt the Australian ballot. Moreover, it chose the office-block format, which has been in use ever since. This format was explicitly designed to confront the voter with each individual candidate

[23] Rusk, "The Effect of the Australian Ballot Reform."

[24] Except with regard to "rolloff," or ballot fatigue, which in Massachusetts underwent a sudden and substantial increase with the introduction of ballot reform. The failure of the partisan indicators to undergo any major change in the same reform-era period suggests the probability that ballot fatigue affected the followings of both parties about equally.

on a long ballot, and hence made it as hard as possible for the individual to vote a straight party ticket. Yet there is virtually no change in the state's split-ticket indicators in the years after the ballot's first general use in the 1889 election. In fact, it took *fifteen annual trials* before split-ticket voting in Massachusetts suddenly began to rise sharply above traditionally low levels.[25] From this fact it must be inferred that traditionally high levels of partisanship survived for half a generation after the ballot was changed, and despite the exceptional procedural difficulties which that change put in the way.

The Australian Ballot in this state thus opened the way to much later electoral change, which deeply affected the intensity of partisanship in the electorate. *But that is all.* The events of 1904 and subsequent years must find their explanation in profound change in popular attitudes and behavior, not in any directly measurable partisan effects of the Australian ballot itself. Nor, as I have tried to show elsewhere, is this a unique case in point.[26] There appear to have been three waves of increase in split-ticket behavior throughout the country before the most recent post-1950 upsurge. Each of these three crested at a higher peak than its predecessor, and the present fourth wave has moved to higher levels still. The first of these three occurred in the 1890s, concurrent with the establishment of the Australian ballot in most states. The second fell in the 1904–1912 period, and the third during the 1920s. All of them except the first must be explained on grounds other than those of changes in election mechanics; all except the first represent significant behavioral changes within the electorate. If one is primarily concerned with long-term trends within an electorate, he must, of course, be vitally concerned with such structural changes as the introduction of new ballot forms. *But we are also primarily concerned with the whys and wherefores of subsequent changes in measurable behavior.* These changes—all of them evidently of much greater magnitude than those of the ballot-reform era itself—cannot receive explanation through structural hypotheses.

Moreover, there is a problem even with the first or ballot-reform-era upsurge in split-ticket voting, one which may well turn out to be extremely refractory to quantitative analysis. This concerns the timing of the reform. Unhappily for analytic certitude, the period 1890–1896 is not only one of ballot reform but also of nationwide critical realignment. In his recently published study, *The Winning of the Midwest*, Richard Jensen makes the important point that each of the elections in this period—1890, 1892, 1894 and 1896—involved unprecedented partisan volatility which primarily reflected itself in landslides, the first two in favor of the Democrats, the second two in favor of the Republicans.[27] These closely spaced but divergent landslides themselves drastically loosened the bonds of partisan allegiance which had dominated the preceding third party-system era. Moreover, political independence took a second significant form during the period in the "colonial" regions of the country: massive support for the Populist party which ended only with the Bryanite fusion of 1896. It is reasonable to suppose, as Jensen suggests, that these behavioral upheavals themselves produced some irreversible consequences for partisanship in the succeeding era. It would thus be reasonable to posit a cause or causes arising within the American sociopolitical system in this period which generated Populism, massive landslides, critical realignment and the Australian ballot reform. This of course does not deny the permissive significance of the ballot-reform variable for subsequent electoral change. But it once again calls our attention to two propositions. First, reform was the consequence of behavior undertaken in response to systemwide strain. Second, an interlocking network of unprecedented behavior at the mass base occurred at the same time, and in response to the same systemwide political strain.

The Australian ballot and the Populist revolt grew out of the same primordial sociopolitical reality: the rapidly accelerating dysfunctions of a system of mass electoral politics—including electoral mechanics—which originated in preindustrial, preurban times. In particular, it grew out of the "swamping" of this system by the concentrated power of what our forebears called "combinations" in both politics and economics: the urban and statewide political machine on the one hand, the corporation and the "trust" on the other, often enough with close links between the two. As the Australian ballot and subsequent reforms of election machinery were promoted by a moralistic urban *bourgeois* revolt against the effects and beneficiaries of urban Machines, so the Populist revolt was aimed against the subversion of the national preindustrial political economy by concentrated corporate capital. The two events are, unfortunately, precisely synchronous with each other, and both occur at the approximate terminal date of a dying preindustrial nineteenth-century universe of electoral politics.

[25] Burnham, *Critical Elections*, p. 115.
[26] *Ibid.*, especially pp. 52 and 109–110.

[27] Richard Jensen, *The Winning of the Midwest 1888–1896* (Chicago: University of Chicago Press, 1971), especially pp. xii–xiv. In the present author's view, this seminal work is "must" reading for anyone interested in diachronic analysis of American electoral politics.

Thus, one has more than a little reason for supposing that a presently indefinable but probably large component of split-ticket increase even during the first (reform-era) wave can be attributed to behavioral change. While the ballot reform of course facilitated it, this change was at its roots attitudinal rather than structural in origin, as the failure of split-ticket voting in Massachusetts to increase significantly from 1889 through 1903 rather clearly indicates. It would probably be quite fruitful to stratify and analyze the post-ballot-reform split-ticket measures both by region and by the extent of Populist voting in 1892 and 1894, and perhaps as well by the extent and direction of partisan swing in the 1896 election. For in Massachusetts, Populism and "colonial" revolt against Eastern finance capital were vanishingly small. Perhaps this is why we have to wait until the Progressive-era wave of 1904–1914 to see any convincing partisan effects of this reform in the Bay State.

Implications: Toward a Comparative Sociology of Past Voting Behavior

By this point, the reader may have some sense of enlightenment concerning some specific issues in the controversy but still be quite unpersuaded of its overall substantive importance. If we accept the view that these rules changes should be regarded as wholly mechanical intervenors with no origins of their own in system stress and system adaptation, even so we could surmise that taken together they explain no more than half of the decompositional changes discussed in the 1965 article. But assuming this is true, so what? A reply to this legitimate query must take us to Converse's discussion of the electorate which existed before these changes took place. For not only is this discussion the weakest and most ambiguous link in an otherwise splendid and provocative paper, it also discloses the heart of the controversy.

Converse devotes considerable attention to the possibilities for, and occasional realities of, voter fraud, including repeating votes, in nineteenth-century America.[28] That this was occasionally notorious and even locally widespread cannot be denied. It comes out with compelling vividness in those hidden gold mines for historical electoral research, congressional hearings on contested elections in the South and elsewhere between 1868 and 1901. This, too, is a controlling variable which, to the extent it was a reality, would serve to depress the magnitude of change after the 1890s by shrinking the initial baseline of measurement. The problem, of course, is the virtual impossibility of ascertaining what, if any, fraudulent component existed at a given time and place, particularly in the rural reaches of nineteenth-century America.

However titillating the exploits of "repeaters," "floaters," and other venal hangers-on appear in the official and unofficial literature of contemporaries, it is simple prudence to avoid a precipitate surrender to Progressive-era political clichés. As the example of Philadelphia and other cities reveals down through the 1920s and later, the many urban machines which continued to thrive after turn-of-the-century reform movements had no difficulty in generating surplus voters on demand despite the statutes. Moreover, outside of the South and border areas, most of the preoccupation with corruption was focused, with good reason, on the cities rather than the countryside. When one considers that turnout rates in rural areas tended to be high over very long periods of time down until about 1900, any speculations on corruption as an artificial inflator of general rather than local significance must be open-ended in time as well as space. The assumption must be made that not only were corrupt surpluses more or less universal on territorial lines, but that these surpluses remained a more or less uniform proportion of the total reported vote for two generations and then, for reasons left most obscure, declined toward insignificant levels after 1900.

The first assumption may have some validity for some nineteenth-century statewide political systems outside the South. In Indiana in particular, a "floating vote" of some ten per cent of the total electorate appears to have existed from the 1870s through the 1890s. Even here, however, it should be pointed out that by no means all of this purchasable vote produced artificial inflations of turnout: a vote which is for sale is not necessarily a case of repeating or fraudulent surplusage. Richard Jensen's recently published study of the issue raises an appropriately cautionary note:

> It is easy to accept the fulminations of embittered losers at face value . . . But the historian has to be more careful, especially in view of the long history of a "corruption" theme in American politics . . . It is vastly easier to make blanket allegations of illicit, secret activities than to disprove them; almost always there is a grain of truth in the charges . . . The myth of massive corruption so cleverly conceived that it cannot be detected is a ghost story.[29]

[28] Converse is particularly concerned to create the impression of endemic and massive voter frauds in nineteenth-century rural areas—not surprisingly, in view of his basic hypotheses—though part of the discussion of the fraud problem is based on Progressive-era formulations by Joseph Harris and others whose focus was urban rather than rural. Converse, pp. 288–293, 300.

[29] Jensen, pp. 34–35.

The conclusion which Jensen derives from a massive sifting of the available evidence is that traceable corruption, being a dangerous enterprise for its practitioners, was at most a marginal phenomenon. Most particularly it was marginal in national elections: the motivation of politicians to engage in such practices was at its highest point in local elections of direct importance to local machines—precisely, it may be added, as the Philadelphia story from 1900 through 1930 amply demonstrates. More significant, in Jensen's view, was the amount of error which could creep into the tabulation and counting of votes. Even so, the overall conclusion which Jensen makes after his survey of the evidence is that corruption was episodic and marginal in the Midwest rather than universal. The implication is strong that "militaristic" mobilizations of voters and an extreme strength and intensity of party identification among the mass electorate were overwhelmingly responsible for the phenomenal participation rates found in the 1880–1900 period.

If corrupt surplusage or deliberate tabulation errors could explain *a part* of Indiana's phenomenal turnout rate between 1876 and 1900, such explanations are both less obvious and—for reasons of local political culture and party strategy alike—much less persuasive in states which politicians considered "safe" for one party, and where moralism was a conspicuous part of the local political culture. States such as Iowa and Wisconsin come to mind. Similarly, it is particularly doubtful—and supremely so in the absence of empirical findings—that rural Pennsylvania or the counties of upstate New York were dominated by pervasive vote frauds and overcounts. If one were to ask for a better and more generally plausible explanation for this mass mobilization, the answer must surely lie in the direction which Jensen suggests: in the rechristianization of the country through pietistic religious revivals between 1800 and 1850, and the development of increasingly polarized ethnocultural conflict in which political and religious perspectives became closely fused into a coherent framework for individual electoral decisions. One may indeed surmise that pietistic revivalism provided both the absolutism and the moral energy required to bring about the realignments of the 1850s and, hence, the war against secession and slavery.

It seems clear enough that the civil war trauma was of tremendous polarizing significance in its own right—particularly in the lower Midwest and states like Pennsylvania—and hardly surprising that most elections from 1864 through 1888 were of the "maintaining" variety. It is now still more certain that the struggles between pietists and liturgicals, not only over the Civil War but over local variants of the "social issue" of the time, were intense down until the 1890s. They were central to the building and erosion of political coalitions down to the beginning of the present century. They also were part of a sociological and political context which optimized the probability that any individual member of that "voting universe" would actually cast a ballot; and it is this largely vanished context which seems to explain much of the difference between 19th and 20th century American electorates and their behavior.

We shall return to this point in a short while. Before doing so, we should examine Converse's curiously opaque references to contemporary survey findings concerning rural voting participation in underdeveloped countries such as Turkey and the Philippines. Since the discussion in this version of Converse's chapter is so brief, our discussion will be correspondingly limited. It should be noted above all that the *general* relevance of these twentieth-century studies in the underdeveloped world to American rural politics in the nineteenth is not merely "limited,"[30] but negative. To be sure, politics in the South was marked by a number of "underdeveloped-nation" characteristics. In this region, overwhelming evidence attests to corruption and fraudulence in closely contested elections. Similarly, the voting of hordes of black *campesinos* by their white *jefes* was a conspicuous feature of post-Reconstruction Southern politics. In Alabama, for example, blacks were voted *en bloc* by local white elites to beat back a statewide Populist challenge in 1892 and 1894, and in 1901 they were similarly led to the polls to vote themselves out of political existence.

But these are a few of the many ways in which the South, particularly the Deep South, has been a deviant case in American politics since the Civil War. Even the implication that modern third-world survey evidence might have any applicability to most non-Southern rural voting behavior in the nineteenth century betrays a deep misconception of the historical sociology of the United States. Here the "conventional wisdom" of "impressionistic" American historians and such students of American political culture as Louis Hartz surely leave us on sounder ground.[31] That view argues that except for the very special case of the black population and perhaps some locally significant cases, the nineteenth-century American rural population was composed not of peasants in traditionalist social relations with local aristocracies, but of independent yeomen who were

[30] Converse, p. 287.

[31] Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace, 1955); see also Louis Hartz, ed., *The Founding of New Societies* (New York: Harcourt, Brace and World, 1964), especially pp. 1–22.

thoroughly bourgeois and "modern" in values and personal goals.

A challenge to the general accuracy of this view, as of any other, is surely possible. But such challenge would seem to require empirical validation as a first step to acceptance. If Converse does not intend to make such an argument, then inclusion of these references to Turkish and Philippine voting behavior is irrelevant, and irrelevant in an important way. For its inclusion makes literally no sense except as a strained attempt to explain away anomaly. The particular anomaly in question here is evidently the educational variable. The levels of formal education in the American electorate a hundred years ago were vastly lower than they are today. Yet, by all aggregate measures, that electorate was obviously far more politically mobilized and active than it is today. And since a majority of Americans lived in rural areas until about 1880, this activity was associated both with low formal education and rurality.

So a problem exists. This problem is a real analytical conundrum only in the context of a particular mode of reality perception. While Converse duly disclaims that modern survey findings are "immutable,"[32] the implicit assumption which informs his entire critique is that of the basic uniformity and generalizability of such findings, at least for the United States. In particular, the broad sociopsychological model of the contemporary American electorate which is laid down in *The American Voter* and in Converse's subsequent works is implicitly adhered to from beginning to end. But this model obviously does not fit the data of change in the American electorate. It is inherently static, without historical depth and, for a variety of reasons relating to the intellectual history of the discipline, as weak in its sociological dimensions as it is strong in its social psychology.[33] Since it does not fit the data of long-term change, and since above all its findings in the here and now can give us rather little guidance in the analysis of past political upheavals such as that surrounding the Civil War, any longitudinal analysis made on its premises will turn up a very long list of very important anomalies.

Above all, the shape of the aggregate data for the last century—and particularly for the crises of the 1860s and 1890s—almost compels the conclusion that the level of political cognition in mass electorates is not more or less uniformly constant and low, but is subject to great fluctuations which are heavily dependent upon historical and sociological contexts. Such a conclusion, of course, leads to relativist skepticism, not about the methods and techniques of contemporary survey research, but about the relevance of the findings derived from them at a given point in time and space to other times, places, and contexts. More than this, however, it requires a complete rethinking of the celebrated pluralism and overarching harmony in the American political system of today, and to necessarily cautionary reflections on the health of that system. It is hardly surprising that such a conclusion would be stoutly resisted, or that major efforts should be mounted to discount or explain away the data which seem to support it.

Let us make the assumption that when politics matters, when it is not a spectator sport but is widely perceived to raise life-and-death issues, people behave as though it matters. In other words, let us assume that a significantly larger fraction of the electorate will be politically active and relatively politically aware in crisis situations than surveys of contemporary American presidential elections would lead us to believe, and that the deeper the crisis the more politicized the electorate.[34] Let us finally assume that the sociological context of electoral politics in America was quite different a century ago from the situation today. When we make such assumptions, the anomalies which have perturbed both Converse and Rusk vanish. In the first place, we find ourselves on the trail of a useful explanation of that immensely important American peculiarity, the periodic critical-realignment sequence. Second, we can accept with entire ease the numerous behavioral properties of nineteenth-century electorates which are inverses of the American electorate's behavior in the twentieth century. Third, we can cheerfully get to work on constructing a better model of change than the one which I stated in 1965—a model which incorporates structural and behavioral components, gives

[32] Converse, p. 289.

[33] For an extremely valuable discussion of the intellectual background of American political science generally and of voting analysis in particular, see Richard Jensen's two articles, "History and the Political Scientist" and "American Election Analysis: A Case History of Methodological Innovation and Diffusion," in *Politics and the Social Sciences*, ed. Seymour Martin Lipset (New York: Oxford University Press, 1969), pp. 1–28 and 226–243.

[34] One perhaps suggestive quantitative characteristic of American electoral history has been the close association between periods of critical realignment such as those of 1856–60, 1894–96, and 1928–36 and waves of electoral mobilization or participation increases. Analysis of extreme-polarization situations in current American electoral politics likewise suggests inversions of "conventional wisdom" when racial polarizations are involved: for example, in the Cleveland mayoral election of 1967 the mean turnout in wards more than two-thirds nonwhite was 69.2 per cent, compared with a citywide average of 54.8 per cent; and the Republican (white) candidate received about 80 per cent of a workingclass white vote which has typically been Democratic by margins of 2 or 3 to 1.

each its proper weight, and integrates both into theory.

Let us take a concise example of where this might lead us, the Lincoln-Douglas debates of 1858 and their contextual setting in the American electorate of the time. Historians have repeatedly brought to our attention the intensity of both oral and written political communications in that period, and the great expenditures of time and money which practical politicians put into both. As is well known, practical politicians viewed Horace Greeley's *Weekly Tribune* as indispensable to the Republican mobilization in the 1850s. As is equally obvious, the ideational content of the speeches which Lincoln, Douglas, and others delivered was of a richness and complexity which, say, the Kennedy-Nixon debates of 1960 did not begin to match. These debates and speeches were reprinted in diamond-point type in the *Tribune*—as likely as not along with dispatches on European politics filed by Greeley's European correspondent, Karl Marx.

If we assume that political elites make informed and rational efforts to achieve maximum gain with minimum expenditure of scarce resources, we can deduce a good deal about the people they were attempting to reach and influence. While—like Lincoln himself—they had little formal education, they were mostly literate. Otherwise, the major investment in densely packed written communication which politicians made was an irrational investment. The wide spread of weekly newspapers for which subscriptions had to be paid bespeaks as well some active interest in the external world among this largely rural electorate. The massive physical movements of farmers from miles around to the central points of rallies such as the Lincoln-Douglas debates likewise suggests political motivation, as do occasional monster rallies in cities from the "log cabin" election of 1840 through the "sound money" and other parades of 1896. Moreover, the relative richness of content in written and oral political communication presupposes a judgment by the politicians of the time that the voter was both able and likely to pay considerable and sustained attention to the communication. Finally, it goes without saying that the political communications of the 1850s concerned political issues of transcendent importance.

All of this is compatible with an electorate having certain properties which no longer exist. First, voters were, for the most part, immensely dispersed across a continental countryside. Second, most of the voters lived on the land, or were at most a generation removed. Third, the primitive communications technology of the time sharply limited the voters' options for entertainment and diversion. This would help to ensure that politics would absorb a much greater share of the interested individual's attention than is now the case. Fourth, religion was primordially important in social and individual life. It now seems extremely likely that the clash between what Jensen describes as "pietist" and "liturgical" ethnoreligious subcultures was basic to the formation, maintenance, and transformation of partisan coalitions, at least in the Midwest and throughout the nineteenth century.[35] The mutual reinforcement of religion and politics which appears to have existed throughout the nineteenth-century political system was undoubtedly promoted by the extent to which Americans tended to settle in "island communities"[36] composed largely of people like themselves. Thus, a visit to a Congregational or Methodist church on Sunday and a Republican rally on Monday in the Ohio Western Reserve a century or more ago would tend to produce cognitive consonance and partisan reinforcement. It would also be likely to produce intense party identification, highly stable party voting, and very high levels of political participation—at least so long as the military model of political campaigning was ascendant.

One very brief and partial, but instructive, example of the relation beteeen religion, party identification and party voting may be taken from a survey of Hendricks county, Indiana, which was made in 1874 and coded by Jensen a century later.[37] This was a county in which the pietist-liturgical ratio was lopsided in favor of the former, and one in which non-Protestant European immigration was extremely limited. It was correspondingly much more Republican than the state as a whole. Reworking the Jensen data and joining the global results of the 1874 survey with presidential voting behavior thereafter, we find the pattern described in Table 10. A comparison of this 30 per cent 1874 sample's aggregate of individual party preferences with the voting behavior of the county's entire electorate in the next five presidential elections reveals a correspondence so close that statistical demonstration is unnecessary. The mean Democratic percentage of the 1876–1892 sample was one per cent less than the 38.5 per cent of 1874 preferences: the mean Republican voting percentage was 0.6 per cent

[35] In addition to Jensen, see also Paul J. Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850–1900* (New York: Free Press, 1970). For a review of this and two other recent works in the "new history," see Walter Dean Burnham, "Quantitative History: Beyond the Correlation Coefficient," *Historical Methods Newsletter,* 4 (March, 1971), 62–66.

[36] The term was developed in detail by Robert Wiebe, *The Search for Order, 1877–1920* (New York: Hill & Wang, 1967), a nonquantitative but extremely useful reference for this critical transition period.

[37] Jensen, *The Winning of the Midwest,* pp. 59–60.

Table 10. Religious Attachments, Partisan Preference and Voting Behavior: the Case of Hendricks County, 1874–1908

A. Party Preference by Broad Religious Classification, 1874 Survey

Religious *Tendence*	% Dem.	% Rep.	% Other	% of Total Sample in Category
Pietistic	23.8	72.3	3.9	49.5
No Denomination	48.4	47.1	4.5	43.2
Liturgical	79.7	14.4	5.9	7.3
Total County	38.5	57.2	4.3	

B. Party Preference and Aggregate Voting Behavior, 1876–1908[a]

Year	% Dem.	% Rep.	% Other	*N*
1874 Survey	38.5	57.2	4.3	1618
1876	37.1	58.4	4.5	5156
1880	36.9	59.1	4.0	5408
1884	38.9	56.4	4.7	5322
1888	37.0	58.6	4.4	5624
1892	37.8	56.4	5.8	5359
1896	40.3	58.1	1.6	5872
1900	39.7	57.6	2.7	5943
1904	37.1	58.8	4.1	5838
1908	43.2	54.3	2.5	5953

[a] Presidential elections.

more. The central relevance of religious *tendence* in the largely native-stock population of this county seems clearly established here, as elsewhere in the Midwest where Jensen could generate similar microlevel data.[38] In this respect the nearly equal division of the large nondenominational vote between the major parties suggests nothing quite so much as the *absence* of a profoundly constraining group characteristic, a kind of aggregate "cross-pressure" situation.

I have argued elsewhere that a crucially relevant variable for the analysis of European political behavior may be the presence or absence in specific social strata of *political confessionalism*.[39] Such confessionalism, in European contexts, has tended to be closely associated with the development of specific religious-clientele parties (such as the *Zentrumspartei* in Germany between the 1870s and 1933), and also with the rise during industrialization of Marxist working-class parties with explicit ideological commitments which stand more or less sharply in opposition to the existing political and economic order. Strong confessional identities of this kind appear both to stimulate participation among the relevant sociological groups in the larger population, and to insulate them more or less thoroughly from electoral appeals arising from their traditional political oppositions on one hand or new and often extremist political movements on the other. In such cases, parties themselves become "churches," as it were, or are secular components of formally religious organizations.

We are used to thinking of such categories in terms of the class struggle in modern politics, or the epic conflicts between Catholic and Protestant in Ireland, but the work of a new crop of American political historians is now making it increasingly clear that American nineteenth-century electorates mediated their social experience and political goals through an intense, if typically American, form of political confessionalism. This confessionalism involved sharply antagonistic conflicts over right belief and right behavior, informed by highly divergent and intense polarizations over the "proper" form of Christian relatedness to society and politics. Religion and politics tended to constitute an extremely large part of the total formal communication likely to be received by any individual in that vanished political universe; and such communications heavily reinforced each other. The basis was thus laid for the mass participation and the political enthusiasm which defined nineteenth-century electoral politics, particularly during periods of critical realignment. Such linkages provided both ideologies and a coherent vehicle through which the individual could make sense of his world and his proper place in it. They likewise tended to produce an electoral structure whose thorough mobilization, extreme long-term stability and other characteristics resemble nothing so much as the behavior of twentieth-century European confessional electorates. Finally, they provided the

[38] *Ibid.*, pp. 61–62, 310–314.

[39] Walter Dean Burnham, "Political Confessionalism and Political Immunization," *Journal of Interdisciplinary History*, 3 (Summer, 1972), 1–30.

basis for a moral energy without which the great Republican revolution of the 1850s and 1860s could have been neither launched nor completed.

Indeed, elite strategies of the 1850–1890 period make no sense except in the context of a high level of something very like political confessionalism at the mass base: the rallies, the dense packing of political information into party newspapers and broadsides, the staging and content of performances like the Lincoln-Douglas debates. These and other elite responses to politics bespeak an electorate which was intensely politicized, which had the opportunity, the capacity and the motivation to pay relatively close attention to political argument, which participated—not only by voting—at rates phenomenal by contemporary standards, and which adhered tenaciously to partisan commitments once forged in the heat of realignment.

Thus, our explanation is reduced to what would be a truism in any other context than this. High levels of political involvement existed in nineteenth-century America because the historical and sociological conditions of that time were at an optimum for such involvement. If modern religious or Marxist political confessionalism did not exist in this electorate, something very analogous in its political effects almost certainly did. It is hardly surprising that the grass-roots organizational effectiveness of the parties reached its peak in the last half of the nineteenth-century, or that the existence of a Republican/Union *party* organizational structure in the northern states was so profoundly important to the success of the Union cause during the Civil War.[40]

A crucial measure of the comparative difference between electoral change in the United States and party development elsewhere in the Western world is the fact that this primordial quasi-confessionalism was progressively dissolved after the turn of this century *without being replaced by any other variety*. As the old cosmos of "island communities" and pietist-liturgical cleavages dissolved under the pressure of transition to urbanization and industrial capitalism, the modern shape of the American electoral universe emerged —a shape in which merchandising techniques tended to replace the old military campaign style, in which masses of Americans were selectively excluded, or excluded themselves from participation in politics at any level, and in which party linkages between voters and their decisions at the polls suffered severe erosion.

If I were to make any comment on the original 1965 hypothesis which sought a coherent explanation of these remarkable developments, it would be to suggest that this process was largely unconscious and sociologically determined at all levels. It was certainly not a matter of conspiracy but of circumstance that things turned out as they did during and after the realignment of the 1890s: capitalist and party elites, like everyone else, were fumbling for the ball during that chaotic decade. Moreover, it would only be fair to emphasize how much of the demobilization and anti-partisanship of the post-1900 era was achieved by a common mass consensus—at least among those who were still left in the shrinking electoral universe. As for the dissidents and dropouts, what viable alternatives did they have? Granted the manifest absence of conditions favoring class-solidaristic substitutes for the political confessionalism of the vanishing nineteenth-century religious cosmos, what other vision of the commonwealth and human liberty than one or another variant of liberal capitalism could survive the transition?

But when all is said and done, we would return to two basic points made in 1965. First, American electoral politics since 1900 has pursued a course which is retrograde to change elsewhere in the developed Western world, and the cumulative effect has been profoundly elitist. Second, the American electorates of the nineteenth century were *in fact*—and not in artifact—extensively different from contemporary American electorates, though perhaps similar in some of their attributes to some contemporary European ones. If this is indeed the case, then it is necessary to say once again that contemporary survey-research models of the American electorate should be used to explain voting behavior only at the times and places for which supporting survey evidence is available. They should be extended elsehwere only with great care. This may disturb paradigms, but it will also yield great benefits; for it will permit us to recover the historical and sociological dimensions of American electoral politics. These, in turn, will make it possible for our discipline to develop far better, far more complete empirical theories of democratic linkage and of American politics than any which we now have.[41]

[40] For a lucid and illuminating discussion of this party contribution to Union victory, see Eric McKitrick, "Party Politics and the Union and Confederate War Efforts," in *The American Party Systems*, ed. William N. Chambers and Walter Dean Burnham (New York: Oxford University Press, 1967), pp. 117–151.

[41] To a significant extent, this effort may require pursuit of exemplary "case studies" down to a very microscopic, "local-history" level. A good case in point is that of corruption in rural voting among the northern states. Here we may choose the case of Adams County, Ohio, which received extensive dis-

cussion in Jensen, *The Winning of the Midwest* (see also sources cited there). One may summarize this case concisely. (1) Between about 1870 and 1911 the county was the scene of "undoubtedly the worst example of the corruption of the ballot ever known in American history . . ." (Jensen, p. 38). (2) About 90 per cent of the county's voters came to be involved, with each receiving on the average of $8.75. (3) The parties evidently wanted to curb the practice, but the voters would not let them. (4) In 1911, 1690 men were finally disfranchised for five years by a judge for illegal vote selling (about one-fifth of the electorate). (5) At *no* time did this lead to turnout figures in excess of 100 per cent, or to landslides for either party. As to the former, it may be inferred that the local (corrupt) voters had no incentive to bring in outsiders to share the spoils; and there were very possibly normative constraints on "repeating" by local voters as well.

This is a curious case indeed; but systemwide inferences about "corruption" in earlier periods must be subjected to such micro examination. Between 1872 and 1908, the mean turnout rate in Adams County was 93.1 per cent of the estimated potential electorate, only 2.9 per cent higher than the statewide mean turnout rate. Moreover, in 1916—after elections had supposedly been "purified" but the 1,690 disfranchised culprits of 1911 had been restored to their voting rights—the county's turnout rate was 87.3 per cent, now more than 10 per cent above the state's participation rate, but less than 6 per cent below its corrupt pre-1911 mean.

It should also not go without remark that—as is well known—notorious examples of electoral corruption were widely used by the Progressives to obtain enactment of personal-registration statutes. But this *rural* county, perhaps proportionately the most corrupt of all, was entirely uncovered by such requirements until 1962; and a very large part of it is uncovered still.

Comment on Burnham's "Theory and Voting Research"

PHILIP E. CONVERSE
University of Michigan

I am very grateful to Dean Burnham for his labor in checking out a number of "plausible rival hypotheses" that I proposed some years ago might account for the bundle of changes in American voting statistics he had first described in his seminal 1965 paper. I had pointed out these possibilities with some vigor, since if they could be shown to have empirical merit, they would seem to put a rather different light on those historical changes, relative to the interpretations Burnham originally provided.

I now read the current paper as indicating that for the areas covered, changes in registration practices do appear to have affected turnout adversely, following one of our original rival hypotheses. It also suggests, however, that this registration change does not begin to account for all of the decline in turnout that occurred around 1900, particularly in states like Pennsylvania. It goes on to imply that some other familiar factors affecting turnout, such as party competition, also contributed to that change. In effect, the turnout decline not accounted for by structural change seems greatest where the realignment of 1896 moved states from competitive to rather noncompetitive status (the more frequent case); but states moved by the same events from noncompetitive into competitive status, like Missouri and, in fact, numerous other upper midwestern states, do not show the same pattern as Pennsylvania.

All this is well and good, particularly as Burnham in his penultimate paragraph sets aside the 1965 conspiratorial interpretation of the change that had originally prompted our own brief inquiries as to probable causes. Therefore we feel little need to generate either challenge or defense in this note where the main lines of argument are concerned. There are, however, two or three lesser issues which are worth highlighting.

First, I suspect Professor Burnham would agree that the data he presents are both partial and preliminary estimates of the effect of registration measures on turnout in the period under discussion. In some ways this is obvious: four states do not give us a very definitive picture of this period. Less obvious, but fully as important, is the marginal correspondence between the timing of registration coverage data and the periods over which turnout trends are actually examined (see Table 5). In the extreme case of New York, the date cited for the definition of registration coverage is 1948, although the trend examined is from 1900–1916. I assume that changes in coverage were limited enough that there would be a reasonable correspondence between coverage differences at a later date and areas affected by such legislation in the period for which the turnout trend is examined. However, any lack of perfect correspondence could be expected to muddy the estimates of actual effects, leaving the Burnham data as likely *under*estimates on the turnout decline of the period.

Perhaps more important still, I remain uncomfortable with an assumption which Burnham makes to provide a baseline for all of his estimation. Indeed, this is a problem mischievous enough that we already devoted a paragraph to it in our original "Change . . . " essay. Burnham observes that "any secular trend in voting participation to be found in counties without personal-registration requirements must be attributed to behavioral factors. By definition, no known changes in electoral mechanics could be causes of such change in turnout." Now it is surely true that in a narrow and limited sense, a change in electoral laws cannot be invoked as a causal factor for other change in areas where no legal changes were in fact imposed. There are looser senses, however, in which exactly this can be true; and these seem so plausible to me that I feel they should be made clear. We verge here on the subject of rural corruption in voting in the nineteenth century, a point at which my treatment bothers Burnham considerably.

I originally argued that various kinds of corruption such as the adding of fraudulent votes or the purchase of votes from persons unlikely to have otherwise voted, well-known for urban metropolises of the period, were not unknown in rural areas as well, and in fact produced a significant artificial boost to apparent rural turnout in the nineteenth century. This surmise, if true, would only address the Burnham argument if such practices were not limited to a few states or a single region, but rather were "widepsread," in the sense of some such rural practices going on at a low level in all or most states. I have since been told by several historian friends that whereas there was clearly rural vote corruption in that period of the kind I described, it could not be described as "massive." When I ask, however, whether it might amount to a small but visible fraction of rural votes—say, some proportion between 5–10 per cent, or the amount that I

had originally roughly calculated to be necessary on the average to account for features of the Burnham changes not otherwise accounted for—they are much less willing to doubt the possibility. Therefore let me use this forum to point out that I had not intended to claim that a fifth or a quarter of rural votes in the late nineteenth century were manufactured by corrpution: one in twelve or one in twenty would address the case quite well, provided they were folded in on a "widespread" basis, sprinkled across all or most states.

With this said, we are now ready to address the possibility that rural fraud declined in magnitude around 1900 even where registration laws were not imposed on rural areas (the typical case). The legislative debates over coverage of the stricter registration laws being newly proposed around the turn of the century invariably pitted "out-state" or relatively rural forces against the urban machine. The out-state people had raised the issue of stricter control against fraudulent urban voting, and wanted tight registration laws for the city. The urban people initially tried to avoid such controls in general, although they were quickly beaten back to a position of demanding that any registration controls imposed in the cities should be imposed in the countryside as well. It is of high interest that the out-state forces resisted the latter possibility tooth-and-nail, on grounds that most voters were friends and neighbors in rural areas, and hence the urban recognition problem did not exist there. The out-state resistance to statewide controls is interesting because in most struggles where one protagonist suspects the other of cheating but is simon-pure himself, the obvious tactic is to suggest that *both* sides submit to more stringent surveillance. Indeed, had the out-state forces been willing to accept this equitable solution, they would in many instances have achieved stricter urban control much faster than they did. But this was not in general a palatable solution to them: they demanded that rural voting be left uncontrolled. It is hard to understand the vigor of this resistance unless uncontrolled rural voting were deemed a political advantage by those out-state elites most knowledgeable about it and dependent upon it. And it is not clear what that political advantage might be short of significant increments of votes that would not survive tighter rural controls.

Now let us imagine that such a debate begins to rage in the state capital, with out-state forces arguing that controls are unnecessary in rural areas because fraud is not a problem there. What would be more natural than to imagine that rural fraud would be considerably toned down during the period—often lengthy—that such debates were being conducted, and that this could be expected without actual controls ever having been imposed on rural areas? Surely there is evidence that voting fraud had become almost casual in many areas by the 1880s, meaning that concern about its detectability was at low ebb. The reform ferment of the 1890s and 1900s clearly increased concern again over detectability, which would imply that the more blatant forms of fraud were trimmed back even in areas without controls. For the out-state politician arguing that rural controls were unnecessary because fraud was urban, nothing would be more compromising than a rash of rural vote scandals turned up by the now-heightened community vigilance (although, of course, such did arise: see Burnham footnote 41). Hence it would make excellent sense to "lie low" in rural areas until indignation and debate had subsided, assuming that the new, postdebate state of play—stiff controls in the cities but none in rural areas—would be a fertile field for political gain.

We need not imagine that dynamics of this sort account for all of the Burnham rural turnout decline that accompanied the fraud and registration debates of the turn of the century. I would in fact doubt that they would do so. The important point is more simply that these dynamics are extremely plausible, and in whatever degree they registered in the situation at all, by that same degree the Burnham method of assigning weights to the effects of registration on turnout decline will again, and on a new score, produce an *under*estimate of the impact of the broader reform movement toward reducing *apparent* turnout, even though in fact he does find substantial effect.

None of the above should be taken as criticism of what Burnham offers us here. It takes extremely laborious digging to put together data even in this preliminary form, so that four states loosely reconstructed are a considerable accomplishment in itself. Moreover, the general Burnham strategy of asking how registration areas differed from nonregistration areas is the only obvious way of assessing such effects. We have no better to suggest at this point, although it does seem reasonable to keep in the mind the lively possibility that there was a reform-associated decline in turnout within a reformed state even in areas not immediately brought under reform laws. Surely there can be no question but that the public ethos concerning voting fraud is much different in 1973 than it was in 1873, and that during those hundred years fraud has declined from frequent practice to the rare event, although a city or two may remain lingering bastions of such "practice" to the present day. The fading of the frequent practice is fairly well charted for the cities, but when did it occur for the countryside? The turn-of-the-century time of indignation with its proliferation of court cases over voting frauds would

seem to be an obvious candidate as a dampening force, even for areas not thrust under any tight registration control. And if this period did not in fact produce such a dampening, what later times and causal agents do we then invoke for the unquestionable rural as well as urban decline in "repeating," vote-buying, ballot-stuffing and other malpractices that now seem so esoteric?

There is one other broad theme dominating this Burnham article that strikes us as a straw man. In several publications Burnham has been developing a caricature of the general body of work from the Survey Research Center in general, and *The American Voter* in particular, which has the following elements. An orthodoxy has been proclaimed, it is argued, based on studies of the 1950s, which purports to be entirely generalizable to all times and places. When anomalies arise which do not fit the terms of the 1950s, the investigators are traumatized, either into silence or into attempts to sweep the anomalous facts under one or another rug.

This caricature has been picked up by Pomper and others and deserves closer examination. We find it somewhat amusing where anomalies are concerned, since we see anomalies as the life blood of inquiry and the main winch for raising the generality of theories. Moreover, it seems that most of our time since *The American Voter* has been spent pushing outward in space to other countries or backward in time historically, all with an object of finding anomalies that require revisions and broadenings of perspective. Naturally, anomalies do not exist save as one has expectations, or proto-theories, capable of being countered. We have such expectations, as does Burnham. Where we appear to diverge is that Burnham feels current survey evidence somehow does not deserve the same status as common sense or available fragments of the historical record in setting expectations—and thus finding interesting anomalies to broaden theory—for treating past electoral events.

We simply disagree on this score. To be sure, current survey evidence would be a weak surrogate for survey materials directly out of the epoch involved, but since such data do not exist for the periods under discussion here, the point is academic. The problem is that the historical record, at least as mined thus far, is dreadfully thin, and needs every bit of illumination it can get, including at least those relationships from current survey work proving most robust cross-nationally and most reliable over a widening period of time. A good example is provided by the firm Burnham expectation that a record of high voting turnout is a manifest indicator of an underlying high political engagement and politicization. To some degree, current survey evidence supports this expectation, as does common sense. But the lengthy passage that I inserted in the "Change . . ." essay concerning cross-national data on turnout and involvement, a passage Burnham finds particularly upsetting, was included exactly to make the point that the simple equation Burnham (and many others) count on cannot be fully trusted: among the limited instances in which survey data underlying aggregate turnout data are available, there is a distressing proportion of cases showing high turnout to mean very nearly the opposite from the presumed politicized populace Burnham expects. This does not mean that Burnham is wrong or right in the specific case of the latter half of the nineteenth century, but it does mean that we should have more information before we leap to conclusions. We regret that this straightforward lesson continues to elude him, as he himself confesses, to the point that he argues it has neither logical nor aesthetic place in the essay.

As for the part of the caricature which charges us with overgeneralizing data from the 1950s, this was a piece of the growing myth that I was almost beginning to believe had been true in public print (if not in spirit) until I recently had occasion to re-read parts of *The American Voter*. I was pleasantly surprised at the liberal use of explicit caveats or other types of subordinate clause, all aimed at keeping the reader aware as to how situation-bound these data from the 1950s might be.

Indeed, there are lovely cases in point that address the immediate substantive issues Burnham emphasizes in this article. He raises the issue of generalizability most forcefully when it comes to the question as to whether "the level of political cognition in mass electorates" (is or) "is not more or less uniformly constant and low . . ." (p. 37). He attributes to us an argument of constancy and takes pains to instruct us that it would be reasonable to expect public attentiveness to surge forward in time of crisis, particularly "realigning" crises such as the 1860s and 1890s. When I look up the main discussion of low levels of public cognition of politics in *The American Voter*, I immediately find the point being made more than once in within the space of three or four paragraphs (pp. 255–256 of that volume), that of course the levels depicted from the 1950s' data would show some upward shift—magnitude unknown—in time of crisis.[1]

[1] Actually, I am just finishing a manuscript which treats, *inter alia*, the features of public opinion, attitude formation and ideology that showed marked change in the crises of the late 1960s, as opposed to those features which, while not immutable, turn out to have been relatively inelastic despite great gains in public attention to politics. The hunches in *The American Voter* as to what elements would be more or less elastic in this sense turn out to have been quite accurate.

Similarly, there is frequent mention elsewhere in *The American Voter* that the 1950s did not represent a realigning crisis, thereby leaving unanswered what data patterns might look like under such circumstances. There are conjectures of some length concerning the impact of great crises like the Civil War. Moreover, we devoted a fair amount of analytic time to an effort to locate the residual imprint of some of the catalyzing political events of the more recent Depression realignment, such as cohort effects in class consciousness and the like. All of this is plainly enough written up, yet Burnham announces that this general body of work "is inherently static, without historical depth," and wanting in relevance to past political upheavals.

It would be sad indeed if we did not know a good deal more now about earlier electoral history than was available to us fifteen years ago; and surely the catalyzing crises of the late 1960s in the United States are giving us a firsthand view of what survey data look like at such a juncture, information also not available fifteen years ago. But the allegation that *The American Voter* and surrounding work left no room for such possibilities is an amazing one. For *The American Voter* is explicit, and persistently so, on that score. If knuckles are to be rapped, we suggest that Burnham, Pomper, and others who have been erecting this straw man rap their own knuckles until they take the time to read the text a little more carefully.

Comment: The American Electoral Universe: Speculation and Evidence*

JERROLD G. RUSK
Department of Government, University of Arizona

In the last two decades the empirical approach to political science has been heavily preoccupied with the study of contemporary voting behavior. Few have been sufficiently curious about or motivated by the mysteries of our electoral past to sustain a concentrated research effort in this direction. Yet, as V. O. Key often noted, a knowledge of our electoral past provides us with a better understanding of our electoral present, with how our current system has evolved and changed over time.[1] Only recently have scholars begun to heed the call of Key and new historians like Lee Benson and Samuel P. Hays for an empirical analysis of historical voting behavior. The initial research efforts, although often crude and unsystematic in method, have prompted interesting speculation as to "divergences" in our thinking from what previously had been assumed about past electorates. They have discovered "anomalies" in several different contexts—in earlier unconfirmed theories of voting behavior, in data-oriented contemporary work on the American voter, and even among the various historical research efforts themselves. While the presence of such anomalies characterizes the first stages of any research effort in virgin territory, unraveling the apparent inconsistencies must always be part of the process of a more fully developed cumulative research program. What is the task of the moment is to examine these anomalies in light of the theories and research of Professors Burnham, Converse, and myself—the principal elements in this continuing dialogue about the proper interpretation of our electoral past.[2]

Burnham's Theory

Burnham's theory of past electorates is contained in several pieces, not all of which hew to the same interpretation. His initial theory was presented in a famous 1965 *Review* article, "The Changing Shape of the American Political Universe."[3] This article changed many scholars' ideas of our electoral past, from views which held that our system of pluralist democracy and constituent parties was working well to ones which portrayed such a system as markedly "retrograde" and "deteriorating." The "golden age" of politics, to Burnham, was the nineteenth century, particularly the post-Civil War period. During that era the electorate was presumed to be active, issue-conscious, and "enlightened," reaching heights of political involvement which no further electorates have matched. Issues were presumably more important, the parties were more active and intensely competitive, and the electorate responded accordingly by being acutely aware of the issues and voting straight-party tickets.

The collapse of this remarkable system of electoral politics came in 1896 through an odd assortment of circumstances. Burnham, being something of an "economic determinist," felt that the exploitative aspects of the Industrial Revolution would lead to an inevitable confrontation between the captains of industry and the working classes in the United States as it had in other industrialized nations. Such a confrontation was even more likely to occur in America since, unlike its Western European counterparts, this nation had extended the necessary weapons for the masses to take over the government—universal male suffrage—before the Industrial Revolution began. The tensions between corporate power and the strong democratic and pluralistic elements in the political system came to a head in the 1890s. At stake was the power of government to be used to

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[1] V. O. Key, "The Politically Relevant in Surveys," *Public Opinion Quarterly*, 24 (Spring, 1960), 54–61.

[2] The dialogue first started in an exchange of views between Professor Burnham and myself in the December, 1971 issue of the *American Political Science Review*. For a proper understanding of the history of this dialogue and some of the arguments used in the current paper, see Walter Dean Burnham, "Communications to the Editor," and Jerrold G. Rusk, "Communications to the Editor," *American Political Science Review*, 65 (December, 1971), 1149–1157. For the statement which prompted this exchange, see Jerrold G. Rusk, "The Effect of the Australian Ballot Reform on Split Ticket Voting: 1876–1908," *American Political Science Review*, 64 (December, 1970), 1220–1238. The latest addition to this dialogue is Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change*, ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), pp. 263–337.

[3] Walter Dean Burnham, "The Changing Shape of the American Political Universe," *American Political Science Review*, 59 (March, 1965), 7–28.

pursue either corporate or pluralist goals. Only the chance occurrence of two events prevented the inevitable confrontation from occurring: the depression of 1893 and the Bryan candidacy of 1896. The severe depression of 1893 alienated the working classes from their own party, the Democratic party, which was in power at the time, prompting their receptivity to the business-oriented Republican party. The Democratic candidate of 1896, Bryan, also was unable to relate to the needs and the demands of his party's urban constituency, further insuring that the workers would abandon their party. At the same time, McKinley preached the themes of a business-worker coalition and prosperity to entice the working class to the Republican banner.

The verdict of 1896 brought victory beyond expectation to the Republican Party. The business-Republican elite, according to Burnham, had succeeded in stealing the electoral system from the people through a series of fortuitous events. Once established in power, a realignment which was to leave them the majority party for more than thirty years, the Republicans proceeded to consolidate the Industrial Revolution, dictating the revolution's development without having to be subjected to mass pressures and demands. Corporate power had triumphed, and pluralist democracy suffered as a result. This elite capture of the electoral system would lead, in Burnham's words, to a major breakdown in party organization and competition in the aftermath of the 1896 election and, correspondingly, to an alienation of the voter from the political system. No longer would the voter feel his political participation was meaningful; no longer would he consider his party loyalty to be an important commitment to the system.

Burnham's second statement of his theory, his book on *Critical Elections and the Mainsprings of American Politics*, was more elaborate.[4] Drawing on the economic orientation of his previous work, Burnham expanded such notions into a more general theory of the interrelationships between the economic and political systems. The economic system was seen as constantly developing throughout history, while the political system was remarkably resistant to change and adaptation, partly because of the decentralized nature of the party structure and partly because of the pervading Lockian individualist ethic which implied a laissez-faire type of government. Unfortunately, the development of the economic system was not wholly smooth or beneficial, causing injury to various segments of society. Such economic dislocations, while harmful, were generally uncontrolled by the parties and government until they reached such a tension point that people tried to redress their grievances through political action. Once these economic dislocations spilled over into the political arena, a party realignment was usually inevitable, attempting to bring the antiquated party system into some kind of harmony with the more advanced and changing economic system. All realignments occur, according to Burnham, because of this fundamental tension between the developing economic system and the backward political system. Even the realignment of 1896 was a product of such tensions, although the direction this realignment took was largely unforeseen in the early 1890s. The fact that realignments occur periodically, according to Burnham's statistics, means that the problem of the interrelationships between the two systems is a continuing one, and only in realignment phases does the new majority party exercise some meaningful control over the economic system.

The notion of a periodic realignment cycle in politics caused by tensions between the economic and political systems leaves Burnham speculating about the possibility of a current realignment. While he hedges his bets, Burnham feels that such a realignment is probably occurring, possibly starting with the 1964 election. He sees the Democrats becoming the party of the technologically competent and superfluous strata—the top and bottom of the economic ladder—while the Republicans are moving more and more toward the defense of white "Middle America." Burnham, however, does not yet see a detonator to stimulate the massive tension needed to complete the realignment, although he considers the socioeconomic implications of both the Vietnam war and the urban crisis as possibly significant. At the same time, he advances another possible prediction from his theory—that the parties will decay and fade away rather than realign since they no longer perform the functions necessary to bind the voters to them. This is Burnham's well-known "end of parties" argument—an argument which, to Burnham, may become reality if the parties cannot control the economic system for the benefit of the people, cannot integrate the people into the political system, and cannot effectively ally the voters to the party. In this sense, he says, the party system may have already moved beyond the possibility of critical realignment because of the dissolution of party-related identification, leaving only the possibility of an era without parties as we know them.

Burnham's emphasis on economic causes of change in the electoral universe is so strong that it leaves little room for the consideration of other possible causal variables. Nevertheless, Burnham,

[4] Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton, 1970).

in his book, does consider some of the legal-institutional factors I have proposed (e.g., ballot, registration systems) as possibly useful for his theory in the crucial 1896 realignment period.[6] While noting that such variables had some effects on voting behavior, Burnham concludes that the breakdown in party organization and the decline in voting turnout in this period were due mainly to systemic rather than rules-related causes. Attempting to incorporate legal-institutional factors into his thesis rather than revise his central contentions, Burnham states that the changes in these rules (ballot, registration rules) were "devices by which a large and possibly dangerous mass electorate could be brought to heel and subjected to management and control within the political system appropriate to 'capitalist democracy.' "[6] In other words, implementation of new institutional requirements defining the electoral system could have effects on behavior in the direction predicted by Burnham's theory, but this was only natural since these laws had been deliberately enacted to have these effects by the capitalist elites after they came to power.

The last statement of Burnham's theory is the present article in the *Review*.[7] In many respects, it is similar to the two earlier pieces, but three distinctions remain. First, Burnham relies partially on another theory—Jensen's religious-ethnic theory—to explain the high mobilization and straight-ticket voting rates of the electorate prior to the 1890s.[8] (The reader will also note that Kleppner espouses a theory similar to Jensen's which will be referred to in subsequent sections of this paper.[9]) Prior to this time, Burnham depended mainly on an interpretation of an active and enlightened electorate motivated by issues and the party organization.[10] The portrayal of "militaristic drilling" of party loyalists to the polls by the party organization—an idea also borrowed from Jensen—characterizes the earlier Burnham work even before Jensen's religious-ethnic theory became salient.[11] The later adoption of the religious-ethnic theory represents a partial departure from Burnham's strong economic orientation. It will be shown later that too heavy a reliance on Jensen's theory leaves some internal contradictions between the two authors.

The second distinction in the present Burnham work is his shifting of crucial dates marking the deterioration of the electoral response. In his 1965 work, Burnham maintained that 1896 was the date of record:

> This revolutionary contraction in the size and diffusion in the shape of the voting universe was almost certainly the fruit of the heavily sectional[12] party realignment which was inaugurated in 1896. This "system of 1896," as Schattschneider calls it, led to the destruction of party competition throughout much of the United States and thus paved the way for the rise of the direct primary. It also gave immense impetus to the strains of anti-partisan and anti-majoritarian theory and practice. . . .[13]

In his latest work, in order to explain certain anomalies, Burnham pushes the crucial dates back to the beginning of the 1890 decade (or earlier) when he says: "Unhappily for analytic certitude, the period 1890–1896 is not only one of ballot reform, but also of nationwide critical realignment. . . . It would thus be reasonable to posit a cause or causes arising within the American sociopolitical system in this period which generated Populism, massive landslides, critical realignment, and the Australian Ballot reform."[14] Actually, the main new element in this statement allowing the moving back of dates is Populism. Once this is introduced, the new reference point becomes the birth of the Populist Party (1891).

The last addition to Burnham's theory is a matter of emphasis rather than idea. Considering legal-institutional variables once again in his theory, Burnham finds that they are substantially more important than his earlier 1970 reading—in this case, explaining at least one-third and sometimes up to two-thirds of the variance in voting behavior. Still, despite their upgraded importance,

[6] For a description of these legal-institutional factors, see Rusk, "The Effect of the Australian Ballot Reform."

[6] Burnham, *Critical Elections*, p. 90.

[7] Walter Dean Burnham, "Theory and Voting Research: Some Reflections on Converse's 'Change in the American Electorate,' " *American Political Science Review*, 68 (September, 1974), pp. 1002–1023.

[8] Richard J. Jensen, *The Winning of the Midwest: Social and Political Conflict, 1888–1896* (Chicago: University of Chicago Press, 1971).

[9] Paul J. Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850–1900* (New York: The Free Press, 1970).

[10] What the particular issues were is left largely undefined by Burnham.

[11] Burnham first referred to Jensen's notion of a "militaristic" campaign style in his book, *Critical Elections*. His source was Jensen's paper, "American Election Campaigns: A Theoretical and Historical Typology," delivered at the 1968 Convention of the Midwest Political Science Association. Three years later, Jensen incorporated this idea as a subsidiary element in his broader religious-ethnic theory, published as the book, *The Winning of the Midwest*. It is the latter source that Burnham quotes in his present paper.

[12] The reader should not be confused by Burnham's use of the word "sectional." As a reading of his 1965 article makes clear, the "sectional" realignment was actually underlined and hence caused by an "economic" realignment. See Burnham, "The Changing Shape," especially pp. 24–26.

[13] *Ibid.*, p. 23.

[14] Burnham, "Theory and Voting Research," p. 1016.

the behavioral or systemic theory is considered superior in explanatory power, although Burnham now maintains that both structural and behavioral theories must be included in any final theoretical determination of the forces involved in the crucial turn-of-the-century period: " . . . we can cheerfully get to work on constructing a better model of change than the one which I stated in 1965—a mode which incorporates structural and behavioral components, gives each its proper weight, and integrates both into theory."[15]

The empirical base on which the Burnham theory rests depends on which aspect of it one considers—the transition from the enlightened to the deteriorating electoral period, or the modern period with its alternate possibilities of a new emergent realignment or the end of parties. For the moment, our concern will be only with the transition from the enlightened to the deteriorating period—the main emphasis in Burnham's work. With regard to this transition, Burnham has looked at the changing distribution curves across time of five key variables of voting behavior—split-ticket voting, roll-off, turnout, partisan swing, and drop-off. Basically, Burnham noted that turnout levels were high while split-ticket voting, roll-off, drop-off, and partisan swing levels were low up to the 1890s. After that, the reverse became true—turnout started declining, and the other voting indices began to rise in value. Burnham believes these changes are linked to the 1896 election, the electorate showing a steadfast party loyalty and a high mobilization prior to this date and the exact opposite thereafter—the latter being interpreted as the subsequent peripheral and alienated nature of the electorate after the business-Republican elite captured the electoral system and reigned dominant for at least a generation thereafter. Following these curves across time, one also notes further increases in these indices of voter peripherality in the 1920s which were only reversed, to a certain extent, in the realigning period of the 1930s. Without much risk of error, one can say that almost the entire Burnham theory relies simply on how these five indices behave across time.

General Perspectives

By any definition of the term "theory," Burnham has to be classified as a large-scale or grand theorist. Theory construction at this level of abstraction has both assets and liabilities. On the one hand, it can extend our range of vision to the complex interrelationships between macro-level variables. The new insights and perspectives gained can often lead to specific, detailed research projects—selecting at given times small chunks of the much larger theory in order to nail down our empirical knowledge on a certain score. On the other hand, in its present state, such theory is too large-scale and broad to permit adequate empirical testing taken as a whole. Its very level of abstraction actually precludes some portions of the theory from ever being tested. Also, this type of theory is potentially all-inclusive—being able literally to encompass every facet of political life, whether it seems an apparent contradiction to the theory or not.

A specific example of the problems with such theory is the way in which Burnham can use it to explain any anomalies that other research poses. If the effects of legal changes are considered as an alternate and perhaps more viable explanation of the behavior Burnham predicts from his theory, the theory is sufficiently broad that it can be used to encompass such empirical facts and events, even if, on the surface, they do not seem so readily reconcilable with the Burnham theory. This is exactly what has happened when Burnham has included, at a later stage in his theoretical statements, the Rusk-Converse concern for the effects of legal-institutional variables such as ballot and registration systems.[16] The laws are incorporated into the theory, being viewed as merely another way in which the capitalists consolidated their power and weakened the opposition party. Whether in fact such laws were passed with this intent in mind is an empirical question which I will address in greater detail below, but at this stage it will suffice to say that Burnham's theory can subsume many such facts and events, reconciling them with his more general thesis. One does not know, however, if such a reconciliation paints an accurate picture of reality, and this point should be of some concern to empirically-oriented political scientists.

The broad and abstract nature of the Burnham theory also poses problems for empirical testing. Burnham is dealing with such concepts as business elite take-overs of voting systems, breakdowns in party organization, and psychological alienation of the voter from the political system. It is extremely difficult to operationalize such concepts, and it is equally difficult to find empirical information on them. Historical accounts of such matters are often scarce or of dubious validity, and the psychological aspects of past political behavior are difficult to probe, unlike their contemporary accessibility through sample survey techniques. Such problems put the empirical political scientist in an awkward position—calling for

[15] *Ibid.*, pp. 1019–1020.

[16] For another statement on this same point, see Richard W. Boyd, "A Review of Walter Dean Burnham's *Critical Elections and the Mainsprings of American Politics*," *Midwest Journal of Political Science*, 15 (August, 1971), 595–597, especially 596.

broader-ranged theories, such as Burnham's, to stimulate inquiry, while at the same time realizing that key aspects of these theories may be difficult, if not impossible, to verify by empirical means.

The actual evidence Burnham uses to support his theory points up the very problems of verification to which I have been alluding. The 1965 article by Burnham must be essentially viewed as laying out the distribution of five dependent variables and then attempting to set forth a theory to explain the systematic changes in them. The theory may be correct, partially or completely, or it may not, but it is obvious that there has been little attempt to operationalize the concepts in the theory (e.g., elite conspiracies, party organizational strength, voter alienation) as independent variables in order to test empirically their relations to the systematic changes in the dependent variables of split-ticket voting, turnout, etc. What one has is simply the distribution of five dependent variables—Burnham's theory is an attempt to explain the changes in these variables, but other theories, such as the Rusk-Converse institutional theory, can also be offered as explanations for them. The merit of the latter theory is that it can be subjected to empirical testing, i.e., the legal variables can be operationalized and tested against these dependent variables. Without an empirical grasp on some such independent variables, one is left speculating about the meaning conveyed by these dependent variables of voting behavior.[17] As Converse notes, " . . . most single time series of broad societal interest represent little more than the movement of superficial 'symptoms' whose true causes and genuine human significance remain quite unspecified."[18]

Even the data displays of these dependent variables show the limited empirical range within which Burnham is working. Except for his turnout figures, Burnham contents himself with presenting empirical information on only a few states —usually four or five. The same procedure is followed in his later works, his *Critical Elections* book and the present piece in this *Review*. Given such a limited data base—and the fact that this data base is only concerned with dependent variables of voting behavior (except when Burnham uses the Rusk-Converse legal variables)—the sweeping assertions derived from such data displays must be viewed with some caution. The apparent irony is that the most wide-reaching conclusions, the most general and sweeping of theories, are predicated on the most limited of data bases. This irony leads me to conclude that while Burnham has proposed a most interesting and ingenious theory—one which will stimulate research for years to come—it cannot be regarded as empirically verified at this point. One can speculate that most of the anomalies encountered in this dialogue are perhaps a natural consequence of dealing with a theory which is so revolutionary in outlook but so limited in its empirical foundation.

The Enlightened Electorate

The crux of Burnham's theory lies in the assumption that our political system had a golden age from which we have seriously declined. If this assumption of a golden age is inaccurate, the remainder of the theory concerning electoral deterioration falls by the wayside. In the early Burnham work, this assumption seems to be based on a survey of turnout and split-ticket rates for the period preceding the 1890s—high turnout and low split-ticket values apparently signaling an active, involved, issue-conscious electorate with strong party loyalties. In the later Burnham work, this assumption appears to rest on Jensen's and Kleppner's historical accounts which emphasize the primary influence of local factors on the electorate—particularly, religious and ethnic backgrounds.

The first step in analyzing Burnham's assumption is to determine if the electorate was really as active and involved as the turnout figures tend to indicate. If the figures are taken at face value, they do comment on the massive extent to which citizens participated in elections, although it is a major inferential leap to conclude from such statistics that the electorate was also highly issue-conscious and sophisticated. According to Converse, however, such figures cannot be taken at face value since a corruption factor also entered in during this period, artificially inflating the turnout values.[19] The key question is, of course, how important was the corruption factor of this period in terms of the sheer number of votes involved. Burnham may be right in saying that the exact figures for states will never be known, but his criticism of "colorful history" overplaying the prevalence of vote fraud leaves something to be desired in light of his failure to present any contradictory evidence. Also, a careful reading of Jensen's work fails to show documentation for his concluding, as Burnham did, that vote fraud was probably not a widespread or major phenomenon.[20]

One argument Burnham uses to support his claim of little fraud is that traceable corruption was a dangerous enterprise for its practitioners. My research on the unofficial or party ballot

[17] For a similar viewpoint, see Rusk, "Communications."

[18] Converse, "Change in the American Electorate," pp. 263–264.

[19] *Ibid.*, pp. 276–295.

[20] In any event, Jensen's analysis was only concerned with six states.

which existed in this period, and the electoral milieu in which it was dispensed and voted, indicates just the opposite.[21] The parties instituted and maintained the unofficial ballot system expressly to control the vote, to guarantee knowledge of how people voted. Bribery, intimidation, and corruption were facilitated simply because the ballot system required public voting rather than a secret vote. The vote buyer could guarantee by simple observation that his purchases were voted as he instructed.

Another condition existed which favored vote fraud—the weak or nonpersonal registration system. This system required no proof that the person presenting himself at the polls be the same as recorded in the poll book other than a simple declaration to that effect. This allowed such vices as "repeating votes," "voting the graveyard," and the like.[22] Without some proof as to identity, such as a handwritten signature, or a certain diligence in purging the poll books from election to election to eliminate those who had died or moved away, the attraction for fraud in the form of vote buying and selling was prevalent and, indeed, necessary if the competition was also known to be engaging in such activities.

Both the weak registration and the unofficial ballot systems had to exist in order for corruption to flourish. The registration system allowed people to vote who were not qualified, often permitting a single person to vote many times; and the ballot system with its open vote guaranteed the knowledge that purchased voters were voting as instructed. If either one of these elements had been missing, it would have been much more difficult for corruption to play an important part in the political process. But it was no accident that the two systems both existed—they had been expressly implemented by the parties for that purpose: to control the vote. It would only be with the widespread clamor from the public to end this corruption—in itself highly indicative of the prevalence of corruption at this time—that reform would come in the form of the secret ballot (the Australian ballot) and a strong personal registration system, both being enacted in the 1890s and both being successful in reducing the corruption of earlier decades.

The probability that considerable corruption existed in this period simply because the machinery for it was established and used casts some doubts on Burnham's claim of an enlightened era. The most anomalous finding of all would be that an enlightened electorate could have existed side-by-side with a massive corrupt element. Yet this may well have happened, the enlightened electorate eventually pushing for the ballot and registration reforms to end this corruption. But was this electorate as enlightened as Burnham wishes us to believe? What motivated this electorate to act as it did in the political arena? Would contemporary electorates be wise to follow the example of such an earlier electorate? Questions such as these prompt us to analyze critically some of Burnham's beliefs about such an electorate.

To Burnham, the nineteenth-century electorate, especially the post-Civil War electorate, was (1) active, (2) issue-conscious, and (3) party-oriented. Of these three assumptions, the third is easiest to believe. Despite Burnham's assertion that survey findings can rarely be extrapolated to time past without considerable risk of error, Burnham himself relies on one of the central concepts of contemporary voting studies—party identification—for his theory of the nineteenth-century electorate. According to Burnham, this electorate had a strong party loyalty which was reflected in its heavy straight-ticket voting. One would not deny that a sense of party identification was prevalent at this time as it is in the present day—hardly an anomaly—or that heavy straight-ticket voting occurred. It was not party loyalty alone, however, that dictated a straight-ticket vote in the nineteenth century—the party ballot in use at the time, as I have shown elsewhere, virtually prevented a split-ticket vote being cast.[23] The aura surrounding the polls was also one of heavy party pressure and intimidation.[24] In addition, many straight-ticket votes were cast by bribed voters. While party identification is an important variable in this era, it does not by itself explain the tendency toward straight-ticket voting. Even Burnham, in his later statements, acknowledges the presence of other factors—notably the ballot—when he says: "I take it as axiomatic that the pre-1888 absence of split-ticket voting was largely an artifact of the unofficial party ticket. . . ."[25]

Forgetting for the moment the importance of such factors as the party ballot and the bribed voter and concentrating only on party identification, one still must raise a central question: Would an enlightened electorate always vote a straight-ticket? One's conception of such an electorate would generally be that of rationally deciding its vote on the basis of the issues and the

[21] See Jerrold G. Rusk, *The Effect of the Australian Ballot Reform on Split Ticket Voting: 1876–1908* (doctoral dissertation, University of Michigan, 1968), chapter 2. Also see Eldon C. Evans, *A History of the Australian Ballot System in the United States* (Chicago: University of Chicago Press, 1917).

[22] Converse, "Change in the American Electorate," pp. 282–285.

[23] Rusk, "The Effect of the Australian Ballot Reform" (1970).

[24] Rusk, *The Effect of the Australian Ballot Reform* (1968), chapter 2.

[25] Burnham, "Communications," p. 1150.

candidates' stands on them, not on the basis of party. The fact that Burnham emphasizes the importance of issues in this period gives further credence to such a conception of an issue-voting public. Under what circumstances, then, could the public vote on the basis of the issues and still cast straight-party tickets? The only situation in which the two would seem to occur together would be if the "responsible parties" doctrine held for the late 1800s and if the parties kept their same distinct positions on the issues across the entire time span (since Burnham not only shows very little split-ticket voting within election years, but also very little crossover of party voting between election years). The fact that the 1865–1890 period was mainly one of very close elections would seem to argue against a responsible parties theme in favor of a Downsian notion of the two parties vying for the crucial center of the electoral distribution by making increasingly ambiguous and vague statements on the issues. The Downsian logic, however, would not work if the electoral distribution was bimodal (polarized) rather than the unimodal situation he conceives the American case to be.[26] The other alternative would be for the parties to change issue positions over time to gain new elements in their coalitions (hopefully not losing past ones as a result), something which actually happened with some of the religious-ethnic issues of the period according to Jensen and Kleppner. Yet, if the parties changed their issue positions, the electorate did not seem to respond, since Burnham's partisan swing index shows few such crossovers of party voting. In short, although issues may well have been prevalent in this period, a supposition to be treated below, the enlightened electorate did not seem to be responding to them, at least not in the magnitude presumed by the Burnham theory. Burnham's later adoption of Jensen's militaristic notion of campaigning—of merely drilling the enlightened to go to the polls to vote their party identification—seems more appropriate for this era.

The tenuous position of issue voting in the Burnham theory does not derive from a historical analysis alone. Current survey information shows that education is positively correlated with issue salience. If one can extrapolate this notion to time past, then it is difficult to explain how the earlier American electorate could have been so thoroughly issue conscious in the virtual absence of much formal education. Contrary to Burnham's claim that few were illiterate in this period, the figures show that 20 per cent were illiterate in 1870 and 40 per cent had less than a high school education.[27] Also, at this time, there was little formal development of a mass communications system which we take for granted today. As a result, it was more difficult to confront the voters with the issues, or for the self-motivated voter to obtain information on them. The general reply to these arguments is that local newspapers filled the mass media vacuum. But even if this is true, these papers seem to have been more propaganda rags stirring up party loyalty than conveyors of in-depth information on the issues.[28] One wonders, in any event, how much they were actually read given the lack of education among the people.[29]

In light of these anomalies, Burnham seems to rely on the assumption that the electorate had a strong intrinsic interest in politics. Visions of C. Wright Mills's "crackerbarrel society" come to mind—of an interested and enlightened electorate despite the twin handicaps of little educational training and little mass-media input.[30] In defending his intrinsic interest argument, Burnham, at times, even turns these handicaps into assets by particularly clever pieces of reasoning—such as when he says that the primitive communications technology actually worked for, not against, an interest in politics by virtue of not distracting the electorate from the only "spectator sport" in town, politics. Overall, however, the assumption of such a strong interest in politics depends on politics being central rather than peripheral in people's psychological life space. Contemporary theorists assert that the daily affairs of individuals —their family, friends, and jobs—and *not* their politics, are the primary concerns in their life space. In an earlier age when making a living was more difficult and taking care of the family probably more pressing, it is not easy to accept the notion that politics so deeply and constantly engaged the people's attention.

The prototype of Burnham's enlightened elec-

[26] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), especially chapter 8.

[27] See Philip E. Converse's original version of his paper, "Change in the American Electorate," p. 16.

[28] See Jensen's descriptions of the newspapers as "party presses" in *The Winning of the Midwest*, pp. 5–6, 172, 196–197.

[29] One imagines that a large part of the elite dialogue on issues that was carried in the newspapers attracted only the attention of a small segment of the population. Extensive elite dialogue does not necessarily imply a widespread interest on the part of the public. While the elites may or may not have known the limited audience to which they spoke (many still do not even have accurate notions on this score today), it seems most reasonable, given the lack of education, that only the elite and politically active, a small minority of the population, paid much attention to such debates.

[30] Burnham actually cites Mills's image of the "crackerbarrel society" in "The Changing Shape," p. 16. Also see C. Wright Mills, *The Power Elite* (New York: Oxford University Press, 1956), pp. 298–324.

torate—the essence of crackerbarrel society—was the rural folk who supposedly were more bourgeois and modern in outlook than earlier "peasant-oriented" theories have led us to believe.[31] It is in just such a context that Burnham's assumptions of intrinsic interest are most severely tested —since rural residents suffered more from the handicaps of little education and lack of access to communications media than did most other groups in society. To a certain extent, these conditions still characterize rural society, as contemporary survey evidence shows, with the corresponding results that rural people, in general, show less interest in politics and lower turnout rates than city dwellers.[32] What Burnham is asking us to believe is that an earlier electorate characterized by these same handicaps, only more so, behaved in just the opposite fashion from their contemporary counterparts. Burnham cites the high turnout values of a few select rural electorates of the past as his evidence for this point. While turnout does not necessarily imply the deep and abiding interest in politics that Burnham assumes, the more immediate question is why the rural turnout figures were high in the first place. Converse's corruption argument comes readily to mind, although Burnham contends that rural elections were basically not tainted with vote fraud. Harris disagrees: "It is sometimes supposed that election frauds are confined to large cities. The plain facts are that the elections in rural districts are conducted much more irregularly than in cities and often fraudulently."[33]

While it is an appealing explanation, probably fraud alone cannot account for the high turnout of rural electorates. Realizing this point, Converse maintains that a considerable amount of deferential voting also occurred, mobilized by rural political leaders.[34] In one sense, this argument supports Burnham since it relies on the importance of viable party organizations and leaders to mobilize the vote (and this, according to Burnham, was an era of strong party organizations), but, in another sense, it does not, since the product of the mobilization effort—deferential voting—severely questions the political interest and enlightenment of rural electorates. Once strong registration systems came to rural areas, one would suspect that they would not only eliminate the corrupt vote but also a considerable segment of this deferential vote since it would be heavily composed of the less interested, less involved elements of rural society. Important declines in rural turnout after the turn of the century could well be explained by the implementation of such registration systems. Burnham shows a few cases, however, in which registration was not implemented and yet turnout still declined. While the cases are too few to speculate about the generality of the trend, if the pervading ethos of corruption-free elections did not discourage the artificial aspects of rural turnout, then no simple logic exists to explain the political behavior of rural electorates in these particular cases.

These arguments concerning the causes of rural turnout can be applied to the urban setting as well. A definite, and probably sizable, corruption factor undoubtedly existed with the rise of the political machine. At the same time, the machines probably mobilized large segments of the voting population from the lower class, immigrant, and transient sectors of the city. Whether such a vote can be characterized as deferential may be questionable, but the fact that large numbers of lesser involved people voted who probably would not have done so without the impetus of the party machine speaks well for the deferential argument. A finding which tends to bolster both the corruption and deferential arguments is one which shows that in many cities the transient districts voted on a par with, or in greater proportion than, the silk-stocking districts.[35] There seems to be no plausible explanation for such behavior other than the one given here. All of this tends to show that while Burnham had a handle on one important variable —party organizational effort—he misinterpreted the effects it had on the electorate: it did not produce an electorate with a great interest in politics but only one which was large in size.

With these reservations in mind, Burnham has changed much of his theory of the nineteenth-century electorate in the present article. While there were slight inklings in his earlier writings, Burnham now comes out quite clearly and forcefully for the Jensen-Kleppner theory of religious-ethnic effects. He calls Jensen's book, in the present article, "a seminal work which is 'must' reading for anyone interested in diachronic analysis of American electoral politics."[36] One can understand Burnham's enthusiasm for Jensen's book by noting that, in certain important particulars, it might well free him from some of the criticisms of his earlier work. Since his theory is so broad, Burnham seems to be able to assimilate the

[31] Burnham, "Theory and Voting Research," pp. 1018–1019. Also see Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace, 1955).

[32] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley, 1960), chapter 15.

[33] Joseph P. Harris, *Election Administration in the United States* (Washington, D.C.: The Brookings Institution, 1934), p. 370.

[34] Converse, "Change in the American Electorate," pp. 287–288, 299–300.

[35] *Ibid.*, p. 289.

[36] Burnham, "Theory and Voting Research," footnote 27.

Jensen-Kleppner theory without apparent contradiction. The synthesis, however, actually involves Burnham in some major difficulties which will be treated below: the question of whether economic or ethnocultural factors are the primary influences on voting behavior and the problem of whether the consequences of the 1896 election are viewed as good or bad for the political system.

The Jensen-Kleppner theory views religious and ethnic conflict as determining voting behavior in the late 1800s. The intensity of religious-ethnic values would provide the impetus for voters to be active, attentive to the issues, and loyal to their parties. The basic conflict is usually summarized as that between the "pietists" and "liturgicals"—those who wanted to reform society morally in their image (ending drinking, parochial schools, etc.) and those who wanted freedom for their particular religious practices and social customs. Although the claim is made that religious factions usually coincided with distinct ethnic groupings, there was by no means a one-to-one correspondence between the two, and hence Burnham's assertion of "island communities" becomes somewhat less viable. To the extent that the theory has merit, however, it would seem, at least on the surface, to support Burnham's notion of an active, involved electorate.

But the theory seems to have several limitations, both in conceptualization and in research design. The assumption is initially made that religion is a salient factor for political behavior throughout the period studied. Correlations between religious affiliation and the vote are shown as evidence for this assumption, but mild correlations can often appear between these two variables when they have no meaningful causal connection between them. In contemporary research, we find correlations between the two, but usually they are explained by a third factor. Only in unique electoral situations—such as 1928 and 1960—does religion seem to take on a political salience of its own among a substantial portion of the electorate. This may not have been the case, of course, in the late 1800s—perhaps religion was salient politically throughout this period. From the evidence presented, we simply cannot know for sure. Granting for the moment that religion might have had an independent effect on voting, one then encounters another problem: the failure to employ a multivariate design to pinpoint the relative effects of religion vis-à-vis other possibly important variables.[37] One has the feeling that Jensen and Kleppner might have discovered one useful factor in this period, but that their exclusive reliance on it results in a large overstatement of its importance. The possibility of such an overstatement arises from the sample base as well—Jensen used only six states in the Midwest to test for religious effects and Kleppner used only three states in the same region. Any generalization of the theory to other regions would be tenuous at best.

If for the sake of argument the Jensen-Kleppner theory is accepted at face value, one can see that it reshapes the Burnham theory in certain key respects. The most obvious reshaping results from the diminishing role economic factors play in the Burnham theory. No longer are such conditions as the exploitative effects of the Industrial Revolution, the depression of 1873, and the agrarian plight the key factors of attention. They have been replaced in importance by ethnocultural factors on the local level. A necessary corollary of this is that party identification is no longer seen as rooted in the sectional-economic strife of the 1850s or in the economic exploitation of the late 1800s but instead is taken to have a local base in religious and ethnic groupings. National campaign issues in general and national economic issues in particular are not the important issues of the day, but instead a sort of pseudo-issue emerges which is actually nothing more or less than an extension of the value systems of religious-ethnic groups. The entire orientation of politics seemingly changes in the late 1800s, presenting Burnham with both anomalies and opportunities.

The opportunities would arise because presumably religious conflict was so intense in this period that it had to spill over into the political system. Religious groups so intent on preserving their value systems would be sufficiently motivated to search out the party sympathizing with their views and vote in large numbers for it. The intensity of the conflict would force the parties into a bipolarized situation, a situation in which responsible parties would exist and the unimodal Downsian pattern would have no applicability. Parties would have little trouble mobilizing the people to vote under such conditions, the militaristic drilling merely reinforcing people's natural inclination to vote. The credibility of this scenario could be bolstered by the simple argument that religion and ethnicity are more important to people than economics—that values are more difficult to compromise than interests—and Alford could be aptly quoted as supporting this point of view.[38]

The anomalies which this approach poses for

[37] For example, Burnham cites economic factors as important to his theory—a key question would be how important were such factors in relation to religion, something which could be tapped by multiple regression analysis. While Jensen and Kleppner do look at occupation and class variables, respectively, they do so only in a limited and cursory sense, never employing such variables in any multivariate analysis.

[38] Robert R. Alford, *Party and Society* (Chicago: Rand McNally, 1963).

Burnham comment on his entire view of the enlightened electorate. They raise such fundamental questions as the following: Is an electorate which plants itself in a noncompromising, polarized position an enlightened electorate, an electorate which we wish to emulate? Are the parties which foster and reinforce such patterns the type of responsible parties we want today? An era of conflict based on religious-ethnic values and prejudices hardly seems like a model for contemporary politics to follow. Even the issues surrounding these conflicts, which seem so crucial to Burnham, resemble more "style" than "position" issues—intensely held negative group referents in which there is only one side to the issue.[39] Morality and religious crusades often degenerate into style issues, but these seem just the opposite of the types of issues we need to stress today. The debate on local values—instead of national issues—seems to represent a retrograde, not a stable or developing trend in politics. Alford heartily agrees, saying that religious and ethnic conflicts are detrimental to the stability of a society because compromise is difficult to obtain, while debates on economic matters can eventually lead to a focus on the nation-state and a compromise on the issues.[40]

Finally, it must be emphasized that only if religion generated a tremendous intensity in the late 1800s could it be useful for the Burnham theory. The intensity component is the most important part of the argument because, without it, such a massive mobilization of the electorate could not have been obtained. Even with it, one wonders how much it could have motivated the lesser involved elements of the electorate—the worker, immigrant, and transient. Even a national survey of the parties at the time does not show the major "playing" of the religious issue that Jensen and Kleppner suppose occurred. It seems instead that the religious issue was a more local phenomenon —probably affecting local races more than national ones. With national elections so close, the parties may well not have wished to heat up such an issue for fear of alienating the middle-of-the-road votes they needed for victory. All of this presupposes the importance of a religious issue in the first place, a condition by no means fully documented. Instead, the political salience of religion in this period should probably be seen as more an interesting theoretical question than as a proven empirical fact. Even without this qualification, the religion variable still seems to present anomalies which, when joined with others mentioned for this period, seriously question Burnham's inference of an enlightened electorate. The anomalies discovered in connection with the Burnham theory, however, are not confined to the pre-1890 period alone, but continue to manifest themselves as the electoral scene proceeds to the 1890s and the crucial election of 1896.

The 1896 Election

Political behavior in the decade of the 1890s seemed to depart in significant ways from that of the earlier decades. Voting behavior became more volatile as turnout started to decline (save for the election of 1896) and split-ticket voting and partisan swing began to increase. The problem was to account for these changes in voting behavior. Burnham, in his early work, attempted to explain these changes as a result of the 1896 election. This particular election was extremely crucial to Burnham since the forces shaping it, as he perceived them, justified his theory of economic determinism and electoral deterioration. In his latest piece, Burnham has moved the dates somewhat backward in time, vaguely referring to the entire decade of the 1890s, but it is clear that his emphasis is still on the Schattschneider-defined "system of 1896." The only rationale for moving the dates back seems to be to account for changes which occurred in these voting patterns prior to 1896.

The Burnham explanation for the changes in these voting patterns rests with economic forces—the business-Republican elite capturing the electoral system in 1896 in order to consolidate the Industrial Revolution. This Republican-business hegemony was assumed by Burnham to have resulted in the decline of party organization and competition, with the inevitable consequence that the voter either withdrew from the political system (evidenced by low turnout figures) or vacillated in his choices between the parties if he stayed within the system (evidenced by high split-ticket and partisan swing figures). What enabled the business elite to capture the system was another economic force—the depression of 1893 which drove the mass base of the Democratic Party into the hands of the Republicans. If that was not sufficient cause, many disgruntled Democrats left the party because of either fear or hatred of their candidate, William Jennings Bryan. Workers thus united with the upper-class business elite to form one of the oddest coalitions in American history—a coalition which was dominated by and run expressly for the business elite.

Burnham faces a crucial dilemma in using this theory for the 1890s period. While the theory is consistent with his early work, which stressed an economic interpretation of elections and political history,[41] it seems at variance with his later adop-

[39] Kleppner also refers to these issues as "style issues." See Kleppner, *The Cross of Culture*, pp. 149–178, 371–375.

[40] Alford, *Party and Society*, chapters 10–11.

[41] See, in particular, Burnham, *Critical Elections*, chapter 7.

tion of the Jensen-Kleppner theory. Having adopted the Jensen-Kleppner theory for the pre-1896 period, Burnham suddenly has to drop it as he approaches the 1896 election to avoid downplaying (or even ignoring) his more basic economic-business conspiracy theory. The heated religious-ethnic battle of the previous decades suddenly dies away almost by fiat, disappearing from the political scene in the wake of economic forces. While this may have actually happened, it would seem, from the Alford perspective, that such a life-and-death struggle over religious values and moral issues could not suddenly come to a halt, even in the presence of an economic depression like that of 1893.

Kleppner agrees with the Alford point of view. He sees the religious strife continuing into the 1896 election, although in a somewhat different form from earlier elections. The two parties, in 1896, changed their religious bases of support—the pietists going to the Democrats under Bryan's evangelical leadership, while the liturgicals switched to the Republicans who were trying to create a new image of cosmopolitan integration. Previously, the two parties had conveyed exactly the opposite images.[42] While Kleppner notes that some of the religious shift was conditioned by the economic misfortunes of the 1890s, he neither places the emphasis Burnham does on this economic event, nor does he believe that the business elite captured the electoral system in 1896. Kleppner maintains the primacy of religious-ethnic variables in the 1896 election and, indeed, throughout the 1890s.

While Jensen agrees with Kleppner on several points, including some semblance of a pietist-liturgical division in 1896, his theory is much more complex. The predictions from his theory would lead to the same data findings as Burnham's predictions, but Jensen's interpretation of electoral history is fundamentally different. This is especially anomalous because Burnham quotes Jensen as if they are basically in agreement on the Burnham theory. In Jensen's eyes, a transition in politics was occurring in the 1890s—from the religious battles of the pietists and liturgicals, typified by militaristic campaigning, to a "politics of pluralism" ushered in by William McKinley, the Republican candidate, who believed that groups should not be antagonistic to one another but integrated into a broader coalition working together for security and prosperity for all. McKinley also believed the older militaristic campaigning should be abolished in favor of an "advertising" or "merchandising" style—trying to educate the people on the issues (such as the tariff, the issue for which McKinley was the chief spokesman in this period). According to Jensen, McKinley deliberately set about forming a new Republican coalition, one in which the pietists would have no part since he disagreed with their stringent views and also thought they threatened the electoral success of the party. With McKinley pushing the pietists out in his move to establish a pluralistic base, the only remaining home for them came under the banner of Bryan, the populist evangelist. The new Republican coalition would be a combination of liturgicals, workers, ethnic groups, and capitalists, with a distinct liturgical-pietist division still remaining visible in 1896.[43]

This theory departs from Burnham's in several ways. First, Jensen agrees with Kleppner that religion was still an important variable in 1896, although diminished in importance from previous years. Second, Jensen believes that the other fundamental causes of the 1896 election were basically not economic but political. McKinley's move to a pluralist-based party was crucial. Economic conditions helped such a new strategy, but without this strategy, the electoral decision might well have been different from what history records. Third, Jensen sees the consequences of the 1896 election as good, not bad; this is probably his most important disagreement with Burnham.[44] Jensen views McKinley as making one of the most positive contributions in the history of American politics—dampening and eventually stopping the religious wars by ousting the pietists from the party and starting a new pluralist politics in which groups learned to bargain, compromise, and get along with one another. Even the switch to an advertising campaign style had the positive benefits of getting away from mass drilling of people to vote their party identification to a new era in which politicians attempted to educate the people on the issues. As Jensen says, the era "was not a backward-looking emphasis on party loyalty, but McKinley's new spirit of pluralism."[45] And in another vein, Jensen lends his highest compliment to McKinley when he says that his hero, Franklin Roosevelt, incorporated the McKinley pluralism in a later age:

[42] While the two parties generally conveyed the opposite images prior to 1896, they were not above shifting positions on certain key issues, according to Kleppner, to gain temporary vote advantages from religious-ethnic groups which normally supported the other party.

[43] Jensen, *The Winning of the Midwest*, p. 296; also see pp. 291, 308, and chapter 10 in general.

[44] Jensen, in the next-to-last page in his book (p. 307), attempts to reconcile his interpretation of the 1896 election with Burnham's, but this limited effort is unconvincing. On the last page of his book (p. 308), Jensen summarizes his theory, vividly pointing out his positive feelings about the 1896 election and the subsequent period.

[45] *Ibid.*, p. 308.

Franklin Roosevelt perfected McKinley's strategy of inclusive pluralism by giving practically every major economic, ethnic, and regional interest group in the country the recognition and legislation it wanted; in the process, the Democrats recaptured the support of the liturgical and metropolitan electorate that had formed the key to McKinley's coalition.[46]

Jensen's theory also has implications for both lower turnout and higher split-ticket voting after the 1896 election. McKinley's themes of prosperity and pluralism would be conducive to a Eulau-type "politics of happiness," which would manifest itself in a reduced need or motivation to participate in politics.[47] When people are content with their lives, politics seems even more peripheral to their life spaces. Higher split-ticket voting could result from less emphasis on militaristic campaign techniques and greater emphasis on advertising techniques. The older mode of party presses stirring up people's emotions and party loyalties would be supplanted by a different style in which candidates informed the people about the issues in much the same way as McKinley discussed the tariff in the 1896 election as a means to economic recovery and prosperity. With these themes in mind, Jensen is little persuaded by Burnham's idea of an inevitable confrontation between corporate interests and pluralist elements which led to the downfall of those pluralist elements and to the subsequent deterioration of the system. In Jensen's version, pluralist politics won —not lost—in the 1896 election, vanquishing the previous system which was anything but enlightened. Rather than declining, democracy had actually come of age.

Anomalies such as these tend to weaken the Burnham theory. But one key element of the theory has more than a slightly plausible ring to it—the effects of the depression of 1893.[48] This economic force could easily be important in shaping the realignment of the 1896 election. Our knowledge of the effects of the 1929 depression on another realigning era testifies to this point. The difficulty arises when Burnham maintains that capitalist elites took advantage of such a depression to steal the electoral system from the people and consolidate their rule. One can buy one factor of the theory—the effects of the 1893 depression—without buying the other—the capitalist conspiracy. Burnham, now somewhat sensitive to his earlier implications of conspiracy, downplays this notion in his present article—calling it more a matter of circumstance than conspiracy that capitalist and party elites were able to control the system. The outcome, however, is the same—a system which Burnham still calls "elitist," "capitalist-dominated," and "retrograde" in character. In order to maintain that the system was declining from the enlightened peaks of earlier decades, Burnham must hold that the capitalist elites obtained power, regardless of how they did it.

Unfortunately, we may never know exactly what happened in the election of 1896. Ever since Key's article on "critical elections," this election has been assumed to be vitally important for any theory of electoral politics.[49] Precisely what its significance is remains a matter of debate. Jensen, Kleppner, and Burnham all have different interpretations of this election. The problem comes in deciding if these three theories can be integrated in any way in order to build a better theory of the 1896 contest. The task would be difficult because anomalies abound, but some consistent elements may also be present. One element, however, which is not consistent is Burnham's assumption of the retrograde effects of the system of 1896, a topic to which we now turn.

The Deteriorating Electorate

Burnham's assumption of a deteriorating electorate is based on two things—the statistical changes noted in voting variables and his particular interpretation of the 1896 election. To these he adds, in the present paper, a vague reference to the cause or causes which generated Populism, the landslide victories of the early 1890s, and the eventual critical realignment of 1896. Obviously, several key inferences are being made. With respect to the statistical information, Burnham is implying that lower turnout and higher split-ticket voting are signs of a less involved, less issue-conscious, and more alienated electorate. This view reads with considerable latitude the statistical evidence at hand. Why would lower turnout necessarily convey less involvement and issue awareness among the public, especially since the higher previous turnout, in Burnham's own words, depended on a militaristic drilling of party loyalties and what can be considered to be a pseudo-type issue based on moralism? By implication, Jensen, in his theory, believes that the new advertising style of campaigning would create a greater involvement and issue awareness among the public. Such a shift in campaign style should also cause more split-ticket voting. As contemporary survey research shows, split-ticket voting is a reaction to such short-term forces as issue appeal, allowing a breaking away from slavish loyalty to one's party identification.

[46] Jensen, *The Winning of the Midwest*, p. 308.

[47] Heinz Eulau, "The Politics of Happiness," *Antioch Review*, 16 (1956), 259–264.

[48] Burnham. *Critical Elections*, pp. 38–39.

[49] V. O. Key, "A Theory of Critical Elections," *Journal of Politics*, 17 (February, 1955), 3–18.

A more crucial belief of Burnham's is that lower turnout suggested an alienation of the electorate.[50] Alienation, however, implies a conscious withdrawal from the system because of intense dislike or disgust with it. A better theory would be that politics is peripheral to most people, and in the absence of intense machine efforts to mobilize the vote, the public would become more lax in their civic duties. Those who stayed in the electorate would then be assumed to be more susceptible to short-term forces such as issues than earlier electorates. If it seems anomalous that I can maintain that both lower turnout and greater issue awareness probably occurred at the same time, it will be shown below just how such a situation could, and probably did, happen once the crucial institutional reforms of ballot and registration were implemented. For the moment, however, all I wish to point out is that no hard evidence exists to link lower turnout to a conscious alienation from the system.

Burnham's inferences with regard to his conceptual arguments also merit close attention. His interpretation of the 1896 election as a capitalist capturing of the electoral system has already been discussed and found wanting. In no way does this interpretation constitute hard evidence for any notions of voter alienation. The vague reference to earlier causes before 1896[51] presents a perplexing situation—the very vagueness with which they are discussed precludes much intelligent debate about them. The only handle on them seems to be allusions to some of the same Industrial Revolution tensions which were supposed to have influenced the 1896 election. If one does not accept Burnham's theory of the 1896 election, then one presumably would not agree about the influence of these earlier causes. The question that naturally arises is why Burnham introduced these early causes in the first place. Two reasons come to mind. First, the initial changes in the voting indices came before 1896, weakening Burnham's original argument about the importance of this election. The causes of 1896, therefore, had to be pushed back in time in an attempt to explain the initial changes in these voting curves. Second, the other plausible explanation for these changes—the introduction of the new ballot and registration systems—also took place in the early 1890s. Pushing back the causes which led to 1896 might also conceivably be used to explain the rise of these institutional reforms.

Without these conceptual arguments, Burnham's claims of a deteriorating electorate rest only on the changing patterns of voting variables. The key question must be: Can factors other than the ones Burnham suggests explain these changing vote patterns, and if so, does this invalidate Burnham's notion of a retrograde age? I maintain that my legal-institutional theory goes to the heart of the matter, explaining many of the anomalies which have confronted us in this paper. The legal reforms of registration and the ballot, in particular, are important for explaining the changing vote patterns in the 1890–1920 period. After that, further legal variables are found to be important—most notably, the women's suffrage amendment.

The effects of these legal variables fall into two classes—intended and unintended. It is only when one considers both kinds of effects that a full understanding of how voting patterns changed is possible. For example, the intended effect of the new registration system was to end vote corruption, which it succeeded in doing. But its unintended effect was to put up an additional barrier to the vote, eliminating many honest votes on the part of marginally involved citizens. Turnout thus declined on two scores, as both the corrupt voters and the marginally involved were removed from the polls. The intended effect of ending the corrupt vote was accomplished in several ways. First, the new law required a frequent and supervised purging of the election rolls to validate them and keep them up to date. Second, it stipulated that each person wishing to vote must present himself before the registrar to justify his qualifications, providing a signature which could be used for verification on election day. Such requirements ended the practice of unqualified people voting themselves or for others who would not vote, were deceased, or had moved away. "Repeaters" became uncommon. The new system promoted professional bipartisan honesty on the election boards. At the same time, the law, in its attempt to end corruption by requiring the voter to take the initiative to prove his voting qualifications and become registered, also had the unintended consequence of pricing out of the system those who simply did not have the motivation to take such action. Before this time the nonpersonal registration laws had put the burden of registration on the election officials rather than the voters—the election officials having to take the initiative through canvassing, directory searches, or other means to establish who was qualified to vote.

With such facts in mind, the anomalies regarding turnout no longer seem so strange. A sizable portion of the enlightened electorate's turnout is reduced by the elimination of its most unenlightened element—the corrupt voter. Another group in this electorate—the deferential voters drilled by the party organization to participate—falls by the wayside, not having sufficient motivation to vote once an additional barrier has been placed in its path. These two elements comment

[50] Burnham, "The Changing Shape," p. 27.
[51] Burnham, "Theory and Voting Research," p. 1016.

on the Burnham theory in a most meaningful way —it is not the enlightened who are dropping out of the electorate because of alienation but rather those citizens who have always been characterized by their marginal involvement. Converse notes that the turnout drop in presidential elections in the North between 1896 and 1912 is slightly above 10 per cent.[52] This figure, it would seem, can be easily accounted for by the twin movements out of the electorate of the corrupt and peripheral elements. With these elements removed, the pre-1896 electorate no longer has any superiority in turnout to bolster the claim of an enlightened electorate and a golden age.

In actuality, the logic presented here would view the post-1896 electorate as more enlightened than the earlier Burnham electorate. Once the corrupt and marginal voters dropped out of the electorate as a result of the new registration barrier, those remaining would constitute the more issue-conscious core who had sufficient interest in politics to stay in the electorate despite the legal obstacles involved.[53] It is because of this that we can say that a decline in voting turnout does not necessarily mean a decline in issue consciousness, but could mean just the reverse. Those dropping out of the electorate were deadweight—voters who seemingly were motivated more by the militaristic drilling of parties than by the issues. If issues had been more important to them, they would have stayed in the electorate after the personal registration system was adopted. But these were people who were not much interested in politics and therefore needed the persuading of the party organization just to stay in the electorate in pre-registration reform days. Even the religious-ethnic variables of Jensen and Kleppner did not seem sufficiently important to keep many of these voters in the electorate once a deterrent had been placed in their way. Burnham's analogy likening the early American electorate to the highly mobilized European electorate—in which religious feelings were supposed to be the common cause of high turnout behavior—might better be replaced by an analogy which attributes the two electorates' high turnout behavior to possession of a common registration system—the nonpersonal registration system. The later American change to personal registration is a more likely explanation of our declines in turnout vis-à-vis the European electorate than is any disappearance of confessionalism from the political scene.

Assuming that registration is an important variable, we then move to the other reform in this period—the Australian secret ballot. Like registration, its intended effect was to stop vote fraud but its unintended effect was to permit and even encourage split-ticket voting (and roll-off) on the part of the electorate. The new law attempted to end corruption in several ways. It took the function of ballot preparation and distribution out of the hands of the parties and put it under the regulation of the state. No longer could party hawkers persuade, cajole, intimidate, and bribe voters to take their ballots as the voters walked to the polling place. Under the new system, the ballot could only be obtained from state election officials inside the polling station. The ballot law also provided for a secret vote, ridding the system of the bribery and intimidation of the earlier public voting arrangement. The ballots were, in addition, identified by numbers, stubs, initials of election judges, stamped endorsements, and the like in an attempt to stop ballot stealing and the "endless ballot chain" (or "Tasmanian dodge"). Finally, in an attempt to allow a free and equal choice between the parties, unhindered by party pressure, the states consolidated the ballot—placing the two major parties on the same ballot rather than continuing to allow the earlier system of separate party ballots. While this, too, was an aid to stopping party intimidation and vote fraud, it—combined with the secret vote feature—was to have the unintended consequence of changing vote choice patterns by permitting split-ticket voting. The voter, safe from party pressures, could view the candidates of both parties on the same ballot in the privacy of the polling booth, certainly enhancing the potential for split-ticket voting relative to the earlier party ballots system. The new ballot could also cause increases in roll-off, the tendency for the electorate to vote in lesser numbers for lower-placed offices on the ballot. Having to consider all offices on the new consolidated ballot and to vote on them was definitely a more tedious and difficult decision-making task than simply depositing the earlier system's party ballot in the ballot box, either marked or unmarked. No decision making was required in the latter situation, other than which party's ballot to pick up in the first place.

The impact the Australian ballot would have in stopping corruption should be quite apparent. Often, however, scholars do not seem to see how the ballot could affect the corrupt element of turnout; even Converse's treatment follows the standard pattern of attributing most of the decline in the corrupt element of turnout to the personal registration system. The fact remains that the per-

[52] Converse, "Change in the American Electorate," p. 290. The actual turnout difference figure between 1896 and 1912 is unknown; various estimates place it in the 10–17 per cent range.

[53] Those marginal voters dropping out of the electorate because of the registration obstacle would have an easier system to face a few decades later when the personal system changed from a "periodic" to a "permanent" basis of registering voters in many states.

sonal registration system would have been much less effective in stopping corruption if the Australian ballot had not been enacted. As Wigmore aptly notes, "by compelling the dishonest man to mark his vote in secrecy, it renders it impossible for him to prove his dishonesty, and thus deprives him of the market for it."[54] Or, as he says in another place, "... take away all interest in committing an offence, and the offence will soon disappear."[55] In short, there was no incentive for the vote buyer to pay for votes if the secret ballot prevented him from knowing whether the goods were delivered. As a result, the corrupt vote would seriously decline unless some way could be found to circumvent the new ballot system. The only way attempted was the "Tasmanian dodge" or "endless chain" which, according to Harris, was relatively unsuccessful.[56] The practice consisted of stealing an official ballot, marking it, giving it to a bribed voter to vote, and using the unmarked ballot he received in the next round of the chain. In this way, the vote buyer could guarantee, even with a secret ballot, that his money produced the desired results. States, however, countered such activity by incorporating in their ballot laws various provisions to ascertain that the ballot given to the voter at the polls was, indeed, the one he marked. According to Albright, twenty-five states had the most effective provision—the numbered stub.[57] This stub, detached from the ballot before it was deposited in the ballot box, was checked against the number of the ballot recorded in the poll book by the voter's name. If the two agreed, the vote was allowed.

The Australian ballot not only reduced turnout (in conjunction with the personal registration system) but also changed vote choice patterns. In this latter context, ballot was not only a "contributory" or "permissive" variable, as Burnham claims, but also probably had some direct causal effects as well. Empirically, the evidence shows different adjustment patterns to the party-column and office-bloc versions of the Australian ballot, and even today Campbell and Miller find independent effects of ballot on voting behavior.[58] The ballot system, like registration, was introduced in the early 1890s, antedating Burnham's crucial 1896 election. From the first, it was correlated with rises in split-ticket voting, although more major effects came later in some states, particularly party-column ballot states. Considerable split-ticket voting, however, occurred before 1904—for instance, 1896 was a high split-ticket year.

Probably the effect of ballot on voting is best combined with a theory of short-term forces. The Australian system had loosened up party voting enough to allow short-term forces such as candidate and issue appeal to play a role in determining the vote. Such forces would often be translated into split-ticket votes much as they are today. The fact that large spurts of ticket-splitting did not occur continuously after the 1896 election calls into question the existence and widespread impact of any Burnham business conspiracy or anti-party forces in this period. The variable nature of split-ticket voting would better support a theory which states that a high or low split-ticket election is dictated by the salience and intensity of the short-term forces in that election year. Of course, the theme of ballot is central throughout—the new ballot had to be introduced in order for people to split their vote, and even a low split-ticket year would usually have a higher split-ticket value in the Australian ballot period than previously, simply because some people would be using the new ballot's split-ticket feature. When one looks back to the pre-Australian days, one would acknowledge that short-term forces existed then also (although in no degree comparable to Australian days because of the nature of the pre-Australian ballot and its corresponding partisan culture), but the crucial point is that it was extremely difficult to translate such forces into split-ticket votes using the party ballot system.

Two other movements probably contributed, by default, to the increased ticket-splitting in the new ballot period. The elimination from the electorate of two sets of straight-ticket voters would correspondingly cause split-ticket figures to rise. These two sets were obviously the corrupt voters who were paid by party hacks to vote straight tickets and the marginally involved who would only go to the polls in response to the militaristic drilling of the party, dutifully casting their straight-ticket votes. The voters who would remain in the electoral stream in the reform period would be more likely to consider short-term political forces in their vote decisions, instead of staying as enslaved to the party organization as they had in the past.

Basically, ballot and registration go a long way toward explaining the changing vote trends in the North for the 1890–1920 period. But what about the South? This region has always been an analyst's nightmare, and it is no wonder that Burnham's focus on changing vote patterns deals almost exclusively with the North. If legal-institu-

[54] John H. Wigmore, *The Australian Ballot System as Embodied in the Legislation of Various Countries*, 1st ed. (Boston: Charles C. Soule, 1889), p. 32.

[55] *Ibid.*, p. 31.

[56] Harris, *Election Administration*, p. 73.

[57] Spencer A. Albright, *The American Ballot* (Washington, D.C.: American Council on Public Affairs, 1942), pp. 43–44.

[58] Rusk, "The Effect of the Australian Ballot Reform" (1970), and Angus Campbell and Warren E. Miller, "The Motivational Basis of Straight and Split Ticket Voting," *American Political Science Review*, 51 (June, 1957), 293–317.

tional variables are critical to understanding this period as Converse and I maintain, it would bolster our cause if we could show that such variables also explained the changes in southern voting behavior. The bizarre behavior of vote choice variables leaves little hope for illuminating this aspect of voting behavior, but the declining curve of southern voting participation is another matter entirely. The registration variable seems of little use here, since the South generally adopted personal registration systems much later than the North did, but the South enacted other laws governing the vote around the turn of the century which could be useful as explanatory variables—the well-known "Southern System" of poll tax and literacy test legislation. Such restrictive legislation could dampen voting turnout in the South much as personal registration did in the North. As Stucker and I show in an extensive empirical analysis of this period, the Southern System had major effects in curtailing voter participation.[59] Two data presentations will make this clearer. In Figure 1, one can see the widespread difference in turnout patterns between poll tax and non-poll tax states in the South and the similar declining patterns of the "poll tax state" and "all southern state" curves. The latter suggests the dampening effect which poll tax states had on the general southern turnout curve, pulling it down over the years. In contrast, the non-poll tax curve seems to have suffered no corresponding declines, but rather maintained a steady rate over time. Obviously, such states were not hampered by any legal effects which would discourage voting turnout. Table 1 gives average estimates of vote turnout behavior for various types of Southern System states in the 1892–1916 period. The message of the table is straightforward—both the poll tax and the literacy test laws had independent effects in decreasing voter participation, and, when combined, they had a strong cumulative effect, reducing turnout by 47.9 per cent from states not having such restrictive legislation. Although not shown here, another mode of analysis reveals that such legal variables explained much more of the vote decline in the South than did other factors, such as Key's notion of the development of the "one-party South" and the subsequent apathy it presumably produced.[60]

With the 1890–1920 period largely accounted for, in both the North and the South, by legal-institutional variables, the focus shifts to explaining the further changes in voting behavior in the 1920s—the continued decrease in turnout and rise in cross-party voting. Consonant with the Rusk-Converse theory, the variable which immediately comes to mind is women's suffrage. Opening of the system to a relatively politically unsocialized contingent could provide the further flux in electoral figures which Burnham and others

[59] Jerrold G. Rusk and John J. Stucker, "The Effect of the Southern System of Election Laws on Voting Participation: A Reply to V. O. Key," a paper delivered at the Mathematical Social Science Board's Conference on the History of Popular Voting Behavior, Cornell University, June 11–13, 1973. A revised version of this paper will appear in a forthcoming volume on the historical dimension of popular voting behavior, edited by Joel H. Silbey, Allan G. Bogue, and William H. Flanigan and published by Princeton Press.

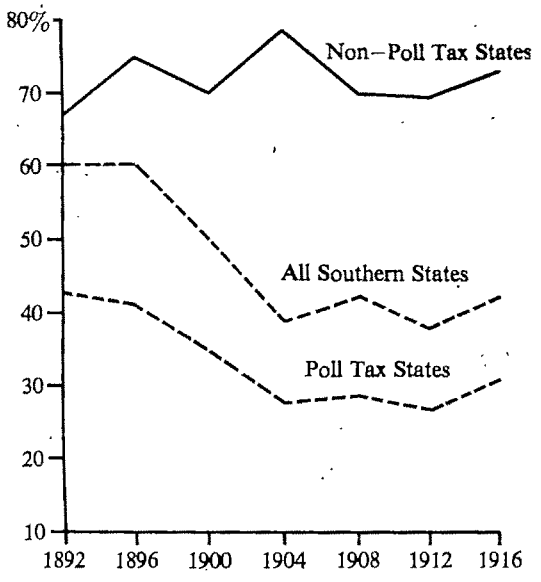


Figure 1. Presidential Turnout Curves for All Southern States, Poll Tax States, and Non–Poll Tax States, 1892–1916

Table 1. Relation of Poll Tax and Literacy Test Laws to Presidential Turnout in Southern States, 1892–1916[a]

	Absence of Literacy Test	Presence of Literacy Test	Marginals
Absence of Poll Tax	72.1 (38)	57.2 (2)	71.3 (40)
Presence of Poll Tax	40.2 (28)	24.2 (33)	31.5 (61)
Marginals	58.4 (66)	26.1 (35)	47.3 (101)

[a] Cell entries are presidential turnout means computed over all election years, 1892–1916, for states having the legal combinations listed in the table.

[60] V. O. Key, *Southern Politics* (New York: Alfred Knopf, 1949).

Table 2. Mean Difference Scores Between Two Elections Before and Two Elections After Adoption of Woman Suffrage, for Two Indices of Voter Turnout, Averaged Across States

	$\frac{(\bar{E}_{+1,+2})-(\bar{E}_{-1,-2})}{N}$
Presidential Turnout	−.097
Off-Year Congressional Turnout	−.104

have noticed. According to an empirical study by Stucker and myself, the evidence seems to support this notion, at least with regard to turnout.[61] Using a variant of the immediate before-after analysis technique, Stucker and I find, on the average, a 10 per cent drop in both presidential and off-year congressional turnout immediately after women's suffrage was adopted in the several states. (See Table 2.) The decline is undoubtedly due to the fact that women were unaccustomed to political participation and probably heedful of the long-standing social norm against it.

The other aspect of the argument would be that women, initially without a strong party loyalty or, perhaps, any party loyalty at all, would engage in considerable split-ticket voting. We have no empirical evidence on this score, but Burnham shows evidence from Chicago and suburban Cook County which supports a mild version of this contention. While his evidence of only a mild relationship between sex and voting volatility may be accurate for a larger universe than Cook County, it would be best to postpone judgment on the size of the effects until a more sweeping analysis has been made. Burnham also states that he would expect ticket-splitting to increase among women over time. On the contrary, given the tenets of political socialization theory, it would be hypothesized to decline—as women engage in more and more election trials and become more socialized into the political system, they should gain a greater psychological attachment to party which would manifest itself in reduced split-ticket voting in succeeding elections. As for turnout behavior over time, Burnham's assertion is probably correct: turnout among women would increase over several election trials. Such turnout might be modest at best, however, compared to new cohorts of women entering the electorate in later periods when the social norms were more favorable for political participation.

Perhaps the entry of women into the electorate could be viewed by Burnham as a deteriorating element since they engaged in politics in a less stable fashion than men. However, any new group entering the electorate—the immigrant, the black, the young, etc.—must be expected to show some volatility as in any learning or socialization process. That these groups eventually integrate themselves for the most part into the system is a plus for democracy. With regard to the wider scenario for the period, 1890–1930, little evidence of electoral deterioration is observed; instead the patterns of political behavior for this period are best understood in terms of the legislation enacted to govern them.

The Legal-Institutional Theory

Throughout this paper, I have proposed the legal-institutional theory as one perspective by which to view the political behavior of our electoral past. I initially formulated this theory in 1970, elaborated it in 1971, and Converse extensively elaborated it in 1972.[62] Basically, the theory postulates that legal-institutional properties of the electoral system—ballot and registration systems, voting systems (e.g., plurality, proportional representation), suffrage requirements, and the like—have important effects in influencing and shaping voting behavior. In essence, they define the conditions and boundaries of decision making at the polls. Often, though, they are taken for granted as "givens" instead of being probed for their effects on voting behavior. A better viewpoint would be to look upon institutional variables as providing the causal framework—the initial effects—within which the effects of other independent variables must be judged. By first ascertaining the effects of institutional variables, the researcher provides a reliable and much-needed baseline against which to measure the effects of other variables. Stanley Kelley and cohorts probably stated it best: it is a matter of "putting first things first."[63]

Thus far, we have seen empirical analysis conducted on the following legal variables: ballot (Rusk), poll tax and literacy tests (Rusk and Stucker), women's suffrage (Rusk and Stucker), and registration (Burnham). The first and third analyses involved the universe of all states; the second, the South; and, the fourth, four states. While the first three have completely covered their relevant universes, more work needs to be done in the fourth area, eventually extending the analysis to all states. From the evidence collected from these studies, the legal-institutional theory can be

[61] Jerrold G. Rusk and John J. Stucker, "The Effect of Legal-Institutional Factors on Voting Participation: An Historical Analysis," a paper delivered at the 1973 Convention of the Midwest Political Science Association.

[62] Rusk, "The Effect of the Australian Ballot Reform" (1970); Rusk, "Communications"; Converse, "Change in the American Electorate."

[63] Stanley Kelley, Jr., Richard E. Ayres, and William G. Bowen, "Registration and Voting: Putting First Things First," *American Political Science Review*, 61 (June, 1967), 359–379.

seen to have a strong empirical foundation—at least with regard to ballot, the Southern System, and women's suffrage. Indeed, it is the only theory mentioned in this paper which is established on such a strong empirical footing. The Jensen-Kleppner theory also has some empirical standing, but it is weakened by some of the assumptions it makes. The Burnham theory seems to have the least empirical foundation of all.

Confronted with this evidence, Burnham still does not view the legal-institutional theory as an alternative to his theory, but as a subunit within it. By and large, such an assumption is based on the notion of "legislative intent." To Burnham, the businessmen and Republicans who formed the capitalist conspiracy deliberately enacted such laws as ballot and registration as part of their plan to consolidate the stealing of the electoral system. At other times, Burnham states that they enacted the laws as part of a move toward antipartyism, presumably this being consistent with their plans to consolidate their power. Two initial points need to be made. First, the legal reforms generally antedated the 1896 "capture," forcing one to use Burnham's amended version of his theory which stipulates that the causes of 1896 had their roots in the political system since at least 1890. Second, if one does not accept Burnham's notion of a capitalist capture of the system, the question of legislative intent becomes immaterial. For the moment, we will set these objections aside, concentrating only on the issue of legislative intent.

The paramount question then becomes—*who* urged the passage of these laws and *why?* Earlier I stated that such laws were most likely passed to end vote corruption. Most historians would agree with this position. The forces most instrumental in pushing for these laws were probably reformers and good government people and not business-Republican elites intent on seizing control of the electoral system. Various histories of the Australian ballot law give this impression. According to Fredman, Henry George, the reformer, was as much the father of the Australian ballot in the United States as Dutton and Nicholson were in Australia.[64] Fredman also cites civic groups, Mugwumps (a reform wing of the Republican party), the Populist party, and trade unions as supporting the ballot movement. Jensen and Kleppner believe pietist elements lent strong support to the cause as well. What is most revealing is Fredman's claim that the voting on the ballot issue in state legislatures was relatively impartial. He states:

> If the party machines were prepared to support or to mangle the laws, they did so impartially. . . . Wigmore tabulated the legislative votes for 1888–1889. He found that the vote followed party lines in six states and ran across them in eighteen. If each party received one point for providing a majority for the bill, or for being the minority when it was defeated, the score was Democrats, fourteen and Republicans, ten.[65]

Such data fail to confirm the Burnham theory that the Republican party was the sole or main supporter of the ballot reform. If this party was the main beneficiary of such a law, the voting statistics do not seem to indicate much awareness of this fact.

Even if the Republicans were the main element behind these laws, which hardly seems the case, one must still explain how these laws could benefit them. To even a casual observer, the laws would seem to loosen rather than tighten control over the system—pushing people away from their partisan anchors to more of a split-ticket and floating voter status. This move to antipartyism (or whatever one wishes to call it) would seem to hurt the Republican as well as the Democratic party. If the urban workers were in the Republican coalition as a result of the 1893 depression, stringent registration laws would prevent many of the marginally involved among them from voting and the new ballot would tempt others to vote split-tickets in the presence of strong short-term forces. In general, one can envision little help that such laws would give the Republican party in maintaining or consolidating the power of any capitalist conspiracy.

If Burnham wished to find a conspiracy which used legal means to control the system, he should have looked at the Democratic party in the South instead of the Republican party in the North. According to Woodward, the Southern System of legislation was used by the Democrats deliberately to eliminate the Negro from the political system. According to Key, it could also have been intended by the Bourbon Democrats to rid the system of the poor white element.[66] Here was a conspiracy for Burnham but it had nothing to do with the exploitation of the Industrial Revolution or any capitalist capture of the electoral system. Its roots were in a different socioeconomic and cultural framework, one which would seem quite unfamiliar to the Burnham theory. It is indeed ironic that the intent factor which could not be found in Burnham's industrial North with its Republican dominancy was found in the agricultural South, a Democratic stronghold. Burnham was correct in

[64] L. E. Fredman, *The Australian Ballot: The Story of an American Reform* (East Lansing, Michigan: Michigan State University Press, 1968), p. 32.

[65] *Ibid.*, p. 50. Also see Table 6 in John H. Wigmore, *The Australian Ballot System as Embodied in the Legislation of Various Countries,* 2nd ed. (Boston: Boston Book Co., 1889), p. 205.

[66] C. Vann Woodward, *The Strange Career of Jim Crow,* 2nd ed. (New York: Oxford University Press, 1966); Key, *Southern Politics,* especially Part 5.

stating that the laws had effects on voting behavior, but the effects Burnham was alluding to, except in the South, were unintended in every sense of the word. Few people anticipated they would occur, and no capitalist elite deliberately tried to promote their occurrence.

The Modern Period

No discussion of the Burnham theory would be complete without tracing and critically analyzing its implications for the modern period—the 1930–1970 time span. Burnham basically continues to hold that throughout this period politics was retrograde and deteriorating. Much of this assertion, of course, is the consequence of Burnham's image of what the capitalists did to the political system in the aftermath of the 1896 election—putting politics on a deteriorating path which could not be reversed. But other aspects derive from Burnham's broader theory of politics: the continual tensions between the economic and political systems throughout history make their own contribution to the deterioration of the electorate. All realignments are the product of such tensions: the economic system, being out of step with the political system, causes severe dislocations and injuries which can only be solved, albeit temporarily, by party realignments. Such tensions did not stop with the 1896 realignment, but have continued to the present time. They forged the realignment of the 1930s, and the cyclical nature of these economic-political tensions calls for a new realignment in the present period. If such a realignment is not occurring now, it is only because, to Burnham, the electorate has deteriorated so thoroughly since 1896 that no viable bond remains between the voter and his party system. If this deterioration has reached the point of no return, we will be faced not with a party realignment but instead with an era characterized by the end of parties. We will examine each of these aspects of the modern period in turn—the nature of the 1930s realignment, the possibility of a new realignment, and the prediction of an end of parties.

The 1930s realignment marked the beginning of the modern period of politics. Undoubtedly, Burnham is correct in saying that it was, at least partially, the product of economic tensions and forces—this realignment was heavily affected by the 1929 depression just as the 1896 realignment was influenced by the 1893 depression. But, according to Burnham, this realignment did not have the bad consequences that the 1896 realignment did—instead it temporarily revitalized the electorate as the capitalist elites were replaced by what Burnham calls the "pluralistic, welfare-oriented elites and veto groups." This revitalization of the voters, to Burnham, is evidenced in the higher turnout and lower ticket-splitting rates of the 1930s—just the opposite of the patterns characterizing the earlier 1896 realignment. The irony of this is that Burnham contends that both realignments were the result of common explanations—economic tensions and forces culminating in depressions—yet one leads to good consequences and the other to bad consequences, one leads to one type of voting pattern and the other the opposite pattern. In my view, the 1930s realignment was behaving more as one would expect a realignment to behave—economic forces causing increased interest among the voters, especially among the marginally involved who came off the sidelines to vote in large numbers against the party of depression, the Republican party. The 1896 realignment either basically had just the opposite effect of not generating much interest, a point I find difficult to believe, or else the newly enacted institutional factors placed an effective damper on much of the interest that it did create.[67] The use of the legal-institutional theory could provide much of the answer to why the 1896 realignment was atypical. It would find unnecessary Burnham's assumption of a capitalist takeover of the system in 1896 with its attendant bad consequences.

The revitalization of the electorate was, however, only temporary in Burnham's view. The 1930s realignment could not satisfactorily resolve the economic tensions of this period any more than earlier realignments were able to do for their periods. Economic tensions and dislocations continued, bringing us either to a new realignment crisis in the present period or else to an end of parties. Such predictions place Burnham at odds with the current paradigm of the American voter laid out by the Michigan team of researchers.[68] In a key footnote, footnote 4, Burnham explicitly indicates his skepticism of the Michigan work, calling for a retesting or replication of *The American Voter*.

Burnham's evidence for either a new realignment or the end of parties rests on the same empirical base—the increasing number of Independent identifiers, split-ticket voters, and partisan swing voters in the current period. The most crucial datum for the end of parties argument would seem to be the increase in Independents or, to reverse the terms, the decrease in party identifiers.

[67] The 1896 election actually showed a small increase in turnout over the previous presidential election, 1892, testifying that some citizens were sufficiently motivated to overcome the legal obstacles to turnout. However, the legal obstacles discouraged any major increase in turnout, and succeeding elections continued to show this effect.

[68] Campbell, Converse, Miller, and Stokes, *The American Voter*.

But this decrease has been mild at best; still 70 to 85 per cent of the electorate identifies with a party, figures which probably rival those of Burnham's enlightened electorate.[69] While defections in voting may be increasing because of the greater import of problems in the late 1960s compared to the complacent 1950s, party identification has only narrowly declined at the same time. As Converse notes, " . . . even with the loosened levels of partisanship in 1968, party allegiance remained far and away the strongest determinant of most political cognitions and behavioral choices in the electorate. It could wane many times as much again without losing its dominant role."[70] Or, as Silbey and McSeveney comment, " . . . partisan identification remains a powerful political influence, particularly in elections other than those for the presidency. Certainly the success of the Democrats in the congressional elections of 1968 and the midterm elections of 1970 suggests that neither party identification as a determinant of popular voting behavior nor the continued strength of the Democratic coalition should be discounted as the nation moves into the 1970s."[71] Thus, a key Michigan concept—party identification—which Burnham so enthusiastically used to describe the enlightened electorate, still shows its dominance in the present period, making questionable any argument concerning an end of parties.

The possibility of a new realignment presents a more complex question. To have a realignment, important issues and forces must be present in the political environment to change party loyalties. But as Weisberg and I point out, there was very little concern in either 1968 or 1970 with economic issues—the key variables in Burnham's theory.[72] Also, Converse, Clausen, and Miller are unable to find any major economic forces in the 1964 election, the election Burnham believes started this new realignment period.[73] Other issues, however, were important in the late 1960s. As Converse, Miller, Wolfe, and I point out, the 1968 election showed considerable public concern over Vietnam, civil rights, and law and order.[74] But even here the vote for Humphrey and Nixon was based much more on party identification than on the issues. In a more extended analysis, Weisberg and I probed the public's perceptions of all possible presidential hopefuls, instead of just the nominees alone, and found that such perceptions were structured along issue as well as partisan lines.[75] The issue dimension that was identified (the "new issue factor") was relatively independent of the partisan factor, giving credence to the belief that this variable could cause a realignment. But longitudinal analysis of such perceptions, using the 1970 election data as a contrast to the 1968 data, revealed that the two dimensions shaping candidate perceptions—party and issues—were no longer independent of one another, but rather had come close to a convergence in the later election.[76] Subsequent results from the 1972 election study showed that this relative convergence had almost been completed.[77] The "new issue factor"—a forerunner of Scammon and Wattenberg's more publicized "social issue"—consisted of the elements which basically had bothered the 1968 electorate—Vietnam, civil rights, the urban plight, and law and order. Its effects, while it was a relatively independent factor in 1968, were to cause increased volatility in the electorate. The absorption by the parties of these issues in 1970—often by taking different stands on them—produced further volatility and possibly some realignment as the convergence became completed. But the primary effect of such an issue factor was to promote volatility rather than realignment. The new issue factor was basically defused by the 1970–1972 period.

Thus, while issues were more important in the 1960s than previously, they probably should be viewed as intense short-term forces causing voter volatility—increased ticket-splitting and partisan swing—instead of as crucial factors shaping a major new realignment. Burnham agrees that Vietnam and the urban plight were important issues, but the differences between us reside in that I neither perceive such issues as economic in nature nor do I regard them (or others) as successfully effecting a realignment. While there may be

[69] The exact figures depend on how one defines a "party identifier." The Michigan question taps a particular category of citizen—the "party leaner"—which some analysts classify as an "Independent" and others, as a "party identifier."

[70] Converse, "Change in the American Electorate," p. 322.

[71] Joel H. Silbey and Samuel T. McSeveney, eds., *Voters, Parties, and Elections* (Lexington: Xerox College Publishing, 1972), p. 306.

[72] Jerrold G. Rusk and Herbert F. Weisberg, "Perceptions of Presidential Candidates: Implications for Electoral Change," *Midwest Journal of Political Science*, 16 (August, 1972), 388–411, especially 399.

[73] Philip E. Converse, Aage R. Clausen, and Warren E. Miller, "Electoral Myth and Reality: The 1964 Election," *American Political Science Review*, 59 (June, 1965), 321–336.

[74] Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 63 (December, 1969), 1083–1105.

[75] Herbert F. Weisberg and Jerrold G. Rusk, "Dimensions of Candidate Evaluation," *American Political Science Review*, 64 (December, 1970), 1167–1185.

[76] Rusk and Weisberg, "Perceptions of Presidential Candidates."

[77] Personal communication with Professor Arthur H. Miller, Study Director of the 1972 Michigan Center for Political Studies National Election Study.

increasing vote defections which tend to paint the Democratic party as the standard bearer of the technologically competent and superfluous strata and the Republican party as the keeper of white Middle America, the vote defections have not led to basic changes in party loyalties. The relative increase in Independents may have some bearing on this as a "half-way house" theory would imply, but until actual conversions take place, there is little evidence for inferring that a major new realignment—economically or socially motivated—is currently happening. And until declines in party identification occur at a much faster rate than is presently the case, and the reasons for them are known, an end of parties argument could not be viewed as a plausible substitute for the empirically-weak new realignment thesis.

In a yet broader perspective, perhaps Burnham's theory of realignment needs to be substantially revised. According to Burnham, our rudimentary party structures have never been able to adjust to or to control an autonomous economic system. Realignments result because they have been "the chief means through which an underdeveloped political system can be recurrently brought once again into some balanced relationship with the changing socioeconomic system."[78] But as Boyd notes, if the inability of our rudimentary party system to control the accelerating pace of the economic system with its myriad dislocations is the cause of realignments, why have realignments also occurred in countries such as England whose party governments exercise a tighter control over economic changes than do American governments?[79] Boyd finds McClure's explanation for realignments more satisfying—McClure emphasizing the difficulty of any majority party (rudimentary or not) to respond quickly to changing issues, even issues of major import and consequence.[80]

Summary and Conclusions

In this paper, the Burnham theory has been investigated using both logical and empirical modes of analysis. Through such analyses, I have drawn the following conclusions. Basically, I do not believe that Burnham's enlightened electorate was all that enlightened, that the 1896 election resulted in a capitalist take-over of the system, that the consequences of the 1896 election for the subsequent period were retrograde and deteriorating for the electorate, or that further manifestations of this deterioration in the present day have resulted in either a major new realignment or an end of parties. I do feel that Burnham's assertion of economic forces influencing political behavior is valid, in the limited sense that the depressions of 1893 and 1929 helped to shape the 1896 and 1930 realignments, respectively, and that economic forces must always be considered as one variable in any multivariate analysis aimed at unlocking the mysteries of our electoral past. I also agree with the Jensen-Kleppner component of the Burnham theory that religion is another variable that should be added to this multivariate equation, but that it probably was not the major factor causing intense polarizations and strong partisan loyalties that Burnham believed it to be. Rather, religion seemed to be a local factor of some limited importance, coming to the fore only when major campaign issues pointed to the salience of religious feelings and paved the way for their easy translation into political terms. Essentially, the portrait of the electorate that does emerge from this paper is one which is a product of institutional and party forces in the pre-1890 period (especially in the 1865–1890 period) and a consequence of the interaction of institutional, party, and short-term forces in the post-1890 period.

The pre-1890 electorate lived in a highly partisan and corrupt atmosphere. The parties in this period attempted to control the vote by implementing registration and ballot systems which were conducive to this goal. They militaristically drilled voters to the polls, relying on their pressures on the voters, coupling this with the buying of votes and the padding of election rolls to assure the desired outcome. Obviously the most important single cue the electorate received for voting was a partisan cue. Whatever short-term forces existed favoring a split-ticket vote would be difficult to implement in a system with such a strong party aura and on a ballot which did not easily allow a split-ticket vote to be cast. The extent of such short-term forces in this heavily partisan era will never be known since they could rarely be translated into split-ticket votes, but the lack of a mass media system to communicate such forces on a wide scale probably comments on their relatively modest occurrence in this period. The absence of significant educational training among a public characterized by a pervasive sense of party identification also means that there would be little involvement with the political issues of the day.[81]

[78] Burnham, *Critical Elections*, p. 181.

[79] Boyd, "A Review of Walter Dean Burnham."

[80] Robert McClure, "The Flow of American Politics and Party Systems: Cycles of Stability and Change," a paper delivered at the 1970 Convention of the Midwest Political Science Association.

[81] The political issues of the time (1865–1890) were, in any event, not "life-and-death" issues, but rather ones of less pith and moment. Parties were often drilling the voters to the polls with loyalist and patriotic appeals, such as the Republicans "waving the bloody red shirt" as a constant reminder that they were the party which preserved the Union. Even later national issues—such as the tariff—would only take on an importance, however limited, among the

The institutional reforms of the 1890s changed the nature of the electoral universe. Strong registration systems and the new secret ballot combined to eliminate the corrupt vote. Such registration systems by themselves eliminated much of the marginally involved vote which the parties had been able to drill to the polls in the earlier period. With the loss of these two elements, the electoral universe also lost a considerable portion of its earlier straight-ticket voting component. The new ballot offered to those remaining in the electorate the first clearcut opportunity they had ever experienced to vote a split-ticket. The troubled times of the 1890s would provide sufficient short-term forces to guarantee that the ballot would be used in this way. In addition, frustrated voters of the earlier period who had wanted to vote split-tickets could now do so. The Australian ballot had eliminated the aura of the "party polling place," freeing the voter from party, employer, or other pressure in the casting of his vote. In essence, the public clamor for reform ended the earlier party practice of using the institutional framework for its own benefit. Instead of a party conspiracy to pass the new reforms, it was the reformers who initiated such action to end the earlier party conspiracy of party ballots and weak registration systems.

The electorate subsequently produced by these reforms would seem more enlightened than deteriorating. The most questionable elements of the earlier electorate had been eliminated, leaving an electorate which was more educated, more informed about the issues, and less likely to always cast a straight-ticket vote. This was an electorate which was sufficiently motivated to overcome the legal obstacles to voting and which, with the new ballot system, was no longer a slave to party but free to react to short-term forces as it wished. With the advent of the advertising style of campaigning, rising levels of education, and the development of the mass media, short-term forces would become more and more a prevalent factor of the electoral scene. The 1896 election was not an isolated case with its strong short-term forces of issues—the depression and the tariff—and candidates—the attraction of McKinley and the fear of Bryan. Other important issues and candidates would rise to the fore in the next seventy years, prompting voters to defect from their party identification as a result of these short-term forces. In actuality, the institutional requirements of the 1890s allowed a new theory of voting behavior to come into play—the Michigan theory of party identification and short-term forces. The ballot had permitted short-term forces to enter as a major new variable, yet one which was an adjunct to party identification rather than a replacement for it; it provided the opportunity for people to defect in their voting for certain candidates, not being a slave to party as they had been in the past but still retaining that important psychological attachment to it which would affect much of their behavior. The interaction of these two forces—party identification and short-term forces—still characterizes the electorate today, testifying that we are neither in the midst of a new realignment or an end of parties. The nature of party identification has undoubtedly changed somewhat as a result of the institutional reforms of the 1890s, possibly manifesting itself in more people claiming weak rather than strong loyalties. But the reforms in no way destroyed its meaning to people, only their exclusive reliance on it. With 70 to 85 per cent of the electorate claiming such an identification, one wonders if we will ever see an end of parties.

In the final analysis, the Burnham theory should probably be turned around—Burnham's enlightened electorate should be a deteriorating electorate and his deteriorating electorate more an enlightened one. If we accept this reversal, then the conclusions emerging from it are optimistic—that politics advanced after the 1896 election to a period of better times, that pluralist democracy was not defeated in 1896 but, as Jensen notes, received a new impetus for the modern period. The basis for such conclusions is largely the fact that undramatic variables—institutional factors—were the causal variables of note in this early period, rather than Burnham's more dramatic ones. Burnham has contributed immensely to political science with his unique perspective, promoting important discussion and research on our electoral past. But, all things considered, I believe my perspective offers a closer match to reality than Burnham's—one which explains more consistently, with the data at hand, the past and provides a better continuity to what we know about the present. It is this theory I offer to V. O. Key as the perspective by which to understand the nature of our historical past and its linkage to our present system of electoral politics.

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public when the troubled times of the 1890s had arrived. As for the pervasive sense of party identification, it did not seem to be forged out of whatever religious conflict existed in the late 1800s, but instead was most likely a product of the sectional-economic realignment of the 1850s. People after the War would often be voting the same loyalties that had existed prior to the War, the behavior becoming so routinized that frequently the original motive for such loyalties was later forgotten or considered of little import.

Rejoinder to "Comments" by Philip Converse and Jerrold Rusk

WALTER DEAN BURNHAM
Massachusetts Institute of Technology

Professors Converse and Rusk have replied to my present paper in two different ways: the former by a short, sharp rejoinder and the latter by a longer and more diffuse article. For space reasons alone, my commentary on their remarks must be concise; and while there is much which I should like to discuss further, there comes a time when the case must be sent to the jury.

Professor Converse's Note

It is best, I think, to confine most of my comments on Philip Converse's Note to the last half of it. We begin with *The American Voter* and the SRC "paradigm" as I see them. Having also reread this book, I will go even further in my praise of it than Converse has. It is not only a meticulous piece of scholarship; it is also truly a classic, a seminal work which justly gives permanent professional honor to those who wrote it. A seminal work is one which not only contributes to knowledge but permanently reorganizes the shape of a scholarly field. This can be done only by providing a conceptual framework for the myriad nonseminal but essential scholarly activities which take place in that field thereafter. As a meticulous piece of work, *The American Voter* was indeed qualified and judicious in its statements and generalizations. It also had at least as much sensitivity to the time dimension as could be expected for 1960. In case the point has been lost to view in the clash of arms, I will simply reiterate the obvious: every one of us in this field owes a permanent intellectual debt to *The American Voter* and its authors, and it is only common decency cheerfully to acknowledge that debt.

Associated with Thomas R. Kuhn's analysis of "scientific revolutions" are a few elementary observations concerning the sociology of organized scientific research communities. First, the members of such communities tend to have their own interests and value positions, which in the case of the social sciences usually involve ideologies either explicitly or implicitly. Such communities tend not only to organize subjects into research projects, but to organize them out as well. Second, there is the law of the twenty-four-hour day. Graduate students and young professionals who are involved in learning the methodologies of something as complex as survey research are unlikely to have been educated as thoroughly in historical context, nonstatistical contingency in political affairs, and issues of political and social theory. Third, in this particular case it remains a fair statement, I believe, that the conceptual core of *The American Voter* itself is derived from social psychology rather than from, say, concepts to be found in political sociology. Such focus naturally strengthens the work's powerful organizing character, but at the same time necessarily circumscribes it.

In the context of the organized research community's own interaction patterns, one must remember that the seminal work is not all there is to the story. Assuming that *The American Voter* is all that Converse says it is, this study has also developed a very rich gloss over the past dozen years or so. The development of gloss is a process very similar to that we find in religions, both theistic and secular. It is no news that Christianity has developed in ways which could not have been predicted from analysis of the Gospels, or that Marxism is a phenomenon which has passed far beyond its founder's vision. So too here: the paradigm discussed here starts from *The American Voter* but incorporates the gloss as well.

Of all the enormous corpus of work done within the paradigmatic boundaries set up by the authors of *The American Voter*, it is singularly hard to point to those which have—at least until extremely recently—displayed awareness that there was a relevant time (and therefore contingency) element to the subjects they were investigating. I think it a fair statement that this voluminous gloss has left a vivid impression among students of the field which is quite close to my own description in this article. If I discussed *The American Voter* in a way which falsifies the intent and text of the authors, I recant herewith. But—as is usual in such cases—*The American Voter* is not only a book; as the *Urtext* of a thriving scientific community, it is also a symbol. Any such references to it as I may have made here should be understood accordingly.

I will conclude this discussion by asking Converse two questions. First, does a defense of the text of *The American Voter* really rebut the proposition that, as a whole, this scientific community has worked within the intellectual perspectives discussed in my paper? Second, I appreciate Con-

verse's kind references to Gerald Pomper and myself. But if so, why should we and possibly others make so crass an error of interpretation? Are you sure that the man we have attacked—the *whole* man—is really composed entirely of straw?

The original 1965 article, with all of its deficiencies, developed from an informed hunch that scholarship had not come to terms with important and disturbing aspects of American political reality, past and present. It has, at last count, stimulated at least the production of a doctoral dissertation, an article and exchanges in the *Review*, and the first half of Converse's "Change in the American Electorate." Naturally, this is gratifying; but the author could perhaps be pardoned for reacting as any discoverer of anomaly could be expected to react when subjected to so systematic a barrage from such eminently reputable sources. The original questions which informed the inquiry were simple ones: Has it always been so? Must it be so always? I submit, with all respect, that in the 1965 state of the profession, the asking of such questions was both appropriate and necessary. The obvious response to attack, this pointing out of alternative "possibilities with some vigor," to use Converse's phrase, was to re-establish the credibility of the argument by showing that none of these alternative explanations—nor indeed all of them taken together—can come close to explaining all the change which occurred. That is what I have tried to do.

I leave it to our readers to judge for themselves whether or not Converse's hypothesis of general but marginal rural corruption in the nineteenth century holds water. Similarly, they may judge for themselves whether his references to contemporary examples of high rural participation in politically underdeveloped countries have any probable relevance to rural society and politics in the non-Southern states during the last century. Naturally the evidence must be circumstantial at the individual level. But if we have evidence which so widely represents inversions of the contemporary pattern of *aggregate* data, is it reasonable to assume that the *individual-level* patterns have been largely the same across the past century?

The circumstantial evidence suggests very strongly, to my view, that elections meant something very different to 19th-century Americans than they mean to their descendants today; that parties also meant something very different. While we cannot know everything about that past, we can and will get much closer to it than we are now. It would not surprise me at all if we finally learned that Converse was broadly right for most voters at most times in the nineteenth century ("The Nature of Belief Systems in Mass Publics"), but also that the linkage structures then were fundamentally different from what they are now—and different in ways which are of the utmost importance in mapping the evolution of this political system.

It is of course gratifying for Converse to inform us that work is going on which clearly reveals significant changes in the post-1960 survey profile of American voters. Perhaps it is a little ungracious of me to point out that the country has been in an overt, steadily worsening political crisis ever since the assassination of John F. Kennedy and the beginning of the Vietnam war. If a crisis of such magnitude did *not* produce significant changes in survey profiles of voters and their relationships to issues, candidates and parties, that would truly be a wonder. At the same time, there is strong reason to suppose that these reactions, in the aggregate, have produced the analytical opposite to the classic nineteenth-century critical-realignment sequence. One major reason for this, I suspect, is that both the public and the situational contexts which surround it, from imperial foreign policy to media penetrations, have profoundly changed since the time of the Lincoln-Douglas debates or in the great Free Silver controversy.

There are deep problems involving the use of the time dimension. A study may very well be explicitly "longitudinal" without contributing very much to our knowledge of historical process or the evolving political sociology of a nation; and a great many of such studies fall in that category. Much more is required for effective work in this field than time-series analysis—so much more that one may doubt that any one man can hope to master all important parts of his task with adequacy. Still, teams of scholars with complementary skills and experiences may be able to unravel these difficulties. It is precisely here, indeed, that contemporary survey-research hypotheses and inferences must play a crucial role in the exploration of "past public opinion." Cooperation rather than antagonism between scholars of differing but complementary backgrounds is clearly essential to the scholarly enterprise in this field. The bridges must be built. But they are not to be constructed by explaining away anomalies which are inconvenient to the perpetuation of intellectual constructs. Instead, they can only be dealt with successfully by incorporating such anomalies into a more complete explanatory theory of electoral politics than anyone now possesses. It may be said that the dialectics of this kind of controversy distract from cumulativeness of knowledge. I cannot agree; for any supposed cumulative theory-construction which fails to find adequate explanation for these anomalies involves, explicitly or implicitly, premature certitudes. Whatever infelicities of expression or harshness of comment about the dominant perspectives of an organized re-

search community may have appeared here, that is just what this symposium is all about.

Professor Rusk's Paper

Commentary on this elaborate critique will be confined to a very few points which seem of particular substantive importance. I think it true, as Professor Rusk argues, that the whole setting of the controversy before us requires a reference back to the original 1965 article in this *Review*. There I made a first approximation at identifying and explicating a major reality of American political history which had not only somehow escaped most historians, but which seemed to cast very peculiar light upon the American electorate. At its simplest level, the nineteenth-century American electorate and its behavior patterns make altogether too little sense when viewed through the lenses which the Survey Research Center has provided us. For instance, we usually find that the inverse of Angus Campbell's "surge and decline" model explains short-term landslides in the last century: they were produced by one-sided abstentions of one party's supporters, not by heavy ingestions of marginal and weakly socialized elements in the electorate. There is also genuine shock, a shock which I myself felt, when it is learned that mobilization and other indices of party activity increase backwards across time, as the level of formal education declines.[1] One could go on, but the examples simply abound even to the casual observer.

Even more striking, and to me much more so now than in 1965, is the extent to which the demobilization then described stands in stark contrast to longitudinal developments at the same chronological time in the history of European electoral politics. It was only natural to assume that profound system-wide influences were at work, influences which in their scope and thrust were virtually or wholly unique, among the cosmos of Western nations, to the United States. The description of these interlocked system changes, and of the universe of politics was alone enough to be a stumbling block to those—far more numerous in 1965 than they are today—who would apply the findings of contemporary survey research to all times and places. The explanatory scheme which was developed to deal with these phenomena was even more of a stumbling block, for it argued that the United States evolved after 1900 in such a way that political stability and elite insulation were bought at the price of partially liquidating political democracy.

Professor Rusk calls me to task for a failure to "operationalize" that explanatory scheme, and for modifying some of my hypotheses concerning the variables at work, particularly in the 1890's. I plead guilty to such charges—or rather, might strive for a *nolle prosequi* in the court of academic opinion: for no one knows better than the author of "The Changing Shape" how extremely complex the phenomena he sought to explain are in fact. Nevertheless, it is surely not out of place, contemplating such profound changes, to move from analytic decomposition of phenomena toward their synthetic integration. Such integration is very likely to go beyond data. Granted the current state of integrative efforts in political science, I can only add that happy is he who has as much solid data at the base of his inferences as are available to us in this subfield of study.

At the same time, the conflict which Professor Rusk purports to see between the original, economic-conflict hypothesis of the 1965 piece and the ethnocultural-conflict argument of 1970 and 1973 is one which I fail to perceive. Surely no one can doubt that causal explanations of complex change sequences in a political system must themselves, when fully elaborated, have a certain complexity. Let us review the matter this way. If there is any one thing which the Survey Research Center's work has taught us thoroughly, it is that voters mediate the world of political objects in terms of their immediate life experiences and group memberships. Such work has been invaluable in casting appropriate doubt on the tendency of earlier political historians to assume a public which is in the main *constantly* preoccupied with the issues of the day, which *constantly* pays attention to abstract doctrines and arguments which are quite remote from everyday life. Who today

[1] For a variety of reasons, one should beware of making too much of this decline. Certainly from a comparative point of view, the United States a century ago was conspicuous for its general literacy rate, and also for the extent and depth of interest among all classes of the (white) population in written communications. See Edgar W. Martin, *The Standard of Living in 1860* (Chicago: University of Chicago Press, 1942), pp. 295–342, 393–404. Moreover, one should take Professor Rusk's comment about a 20 per cent illiteracy rate in 1870 with a grain of salt. In the ex-Confederate states, the rate of those males 21 and over who could not write to the whole age cohort was indeed 46.5 per cent. But then we have two intervening legal-structural variables which explain most of this: the institution of black slavery and—subordinate to this—the prohibition which existed in many Southern states against teaching slaves to read or write. For the whole of the Free States, the proportion of adult males not able to write was 7.1 per cent, falling as low as 3.7 per cent in New Hampshire and 3.9 per cent in Maine. Literacy was extremely widely spread in the nineteenth-century white electorate, and seems to have been actively pursued, and noted by European visitors to these shores. *For the time*, this social indicator reveals, as do many others, an extremely high level of socioeconomic development.

would seriously suppose a high level of mass voter consciousness about the intricacies of the tariff arguments made by political elites in the 1880's?

So let us assume that, at the grass roots, acute group conflicts existed in the nineteenth century. Some—not all—of these were directly linked to the construction of major-party coalitions, compiled upwards from the congeries of local group conflicts toward a national summit. The evidence is very strong that many of the most politically salient of these conflicts turned upon subcultural antagonisms among well-defined categoric groups and their members, and that explicitly religious (or religio-political) perspectives formed the cognitive frame of reference for expressing these antagonisms. If we assume this, then it is hardly surprising that so little of the ethnocultural conflict at state and local levels should be manifested in national party platforms. Indeed, it may well be that issues such as the tariff were not only chosen by elites because they were interested in the outcome, but because they formed what we call today a safe set of "codewords." Being in favor of a protective tariff meant being, broadly, in favor of positive intervention by public authority across a wide range of "unsafe" issues which could be explicated more fully at the local level—for instance, in the conflicts between pietists and liturgicals over enactment of sumptuary, "legislating-morality" enactments. Finally, to the extent that tariffs were perceived at all at the mass level, it would be a safe bet that they were perceived as coalition-integrating "code words" covering many issues which were better left without discussion by national party leaders. So Professor Rusk's point that these local conflicts were curiously left out of national-level platforms becomes quite other than the curiosity which he thinks it is.

At the same time, acceptance of the proposition that such conflicts fueled much of nineteenth-century American electoral politics and mass voting behavior does not in any way deny that other issues grossly affected voters, especially during critical-realignment sequences.[2] Particularly in the crisis of 1854–60, it seems nearly certain that there was a notable *convergence* between local ethnocultural conflict issues and the national issues of slavery expansion and sectionalism: the one seems not merely to reinforce, but partially to explain the significance of the other. In the 1890's, the situation was probably more complex. But the basic point would surely be that *both* the shattering force of economic blows *and* changing ethnocultural party images were of immense significance in freeing a critically large minority of American voters for a permanent realignment in their voting behavior.

What might tie these strands together? First, the basic coalitional patterns of the 1894–96 realignment grew out of an economic conflict which was arrayed in geographical terms, i.e., between "colonial" and "metropole" regions of the vast American republic. Second, one among many reasons for Republican success nationwide, and in the vital "metropole" region, was the success of the McKinley-Hanna strategy of *secularizing* the Republican party, making it cosmopolitan and detaching it from identification with semi-agrarian religious perspectives which had become *passé* by the mid-1890's. The larger the "emerging Republican majority" became in North and West, the more attractive and successful this secularization would prove in augmenting it still further. Third, the Democratic party's base—ethnocultural as well as regional—significantly narrowed and, forced back upon a "colonial" clientele, became under Bryan's leadership markedly more Fundamentalist and ex-Confederate than it had been.

One of the most consistent charges leveled against that party by Republicans for the next forty years was its alleged lack of competence to govern; and this incompetence was linked by Herbert Croly[3] with Democratic incapacity to come to terms with the imperatives of high industrial capitalism. The point has merely been restated, with considerable supporting evidence, in David Burner's recent book, *The Politics of Provincialism* (New York: Knopf, 1967). Very broadly, some such composite explanation must come fairly close to the mark; it is very hard to see how, or in what sense, one of its aspects contradicts the other. And suppose it happens that there are a substantial number of people who live in industrial settings, or who are becoming linked to the elaborate division-of-labor world of industrialization. Suppose further that such people are not particularly involved in conflicts between "colonial" and "metropole" regions, or between two broad sets of religious perspectives which no longer relate to their life experiences. In such a case, they might well have two basic alternatives: to vote Socialist if a Socialist party exists, or to abstain. Both were manifest in the 1908–1914 period. By the time "normalcy" was achieved, abstention became paramount. The "party of nonvoters" was larger than all active voters put together in every election from 1918 through 1926;

[2] The reader is referred to a review of several recent quantitative-history studies by me, "Quantitative History: Beyond the Correlation Coefficient," *Historical Methods Newsletter*, Vol. 4, pp. 62–66 (1971), for a more detailed discussion.

[3] *The Promise of American Life* (New York: Macmillan, 1909).

and not since 1910 has a majority of the potential electorate cast ballots in off-year congressional and state elections.

All of this is by now more than a twice-told tale. But it seems to relate directly to another of Professor Rusk's arguments: the contention that the legal-structural change theory he and Professor Converse have elaborated explains most of what there is to explain. If there is anything which my article here attempts to demonstrate as conclusively as possible, it is that such a theory does *not* explain most of what there is to explain. This is so, indeed, even by a very narrow construction of the argument—one which simply looks at the data with the intervening change variables taken as themselves not requiring explanation. Nor can one escape from this conclusion by referring to the "limited" nature of the data analysis. Of course, we all await the multivariate analysis based upon total universe recovery to which Professor Rusk alludes. Those of us who work in this area know only too well the immensity of preliminary data recovery and combination which this will require—to say nothing of the very real problem not merely of identifying the date and nominal scope of a legal change, but of measuring its effectiveness as concretely applied at varying points in time. As to this matter, I will make only two further points. First, the areas selected for analysis contained in the period studied far more than one-third of the total population of the United States. Second, V. I. Lenin is not the only psephologist with an intellectual grounding in political sociology who stressed the point that votes must at times not only be *counted* but *weighed*, that some kinds of political activity are simply more strategically important to the future evolution of a political system than others.[4] Considering not only the size but the strategic political and economic positions occupied by the four states analyzed here, a comparative study of legal and turnout changes within and among them can be amply and independently justified on such grounds. So, for that matter, can the study of critical realignments as decisively important phenomena be justified: for such realignments always involve the movements not of whole electorates, but of decisively large and strategically concentrated minorities.

But the most compelling reason for rejecting the legal-structural change theory—or more precisely, for subordinating it to its proper role in a more comprehensive set of propositions concerning the etiology of major electoral change—is that, from beginning to end, Professors Rusk and Converse have treated this intervention as though it were an uncaused cause, a kind of external intervention into the history of men. This is closely linked, one suspects, to their extreme aversion to a view of elite action, expressed in 1965, which could be read as verging on "elite conspiracy." In point of fact, the *deus ex machina* as an explanation should be left to phenomena which it does explain, such as the 1906 San Francisco earthquake. Otherwise, we are surely on much safer ground if we conclude that such changes are created by men who are more or less consciously pursuing goals through manipulation of the legal parameters of politics.[5] The case in the post-1890 South is too patent for argument, and has a richly documented literature. But the South is properly viewed as an extreme rather than a wholly deviant example of processes more generally and diffusely at work. Proximate motivations behind such actions may well have been couched in such nonconspiratorial terms as "purifying politics," "eliminating corruption" and the like. But a review of the record of the 1894 New York constitutional convention—not to mention developments in Alabama and North Carolina in the same period—yields clear evidence that a larger and very determined "search for order" dominated the work of the revisionists.

The basic model of this order, one to which politics was to be assimilated, was that of the *business corporation*, the most important and successful form of social organization ever developed in this country. Reception of this image by elites, and their transformation of the political institutions of the country in conformity to it, forms the shank of change during this period. Not only does it underlie, in my view, the legal-structural changes described by Professors Rusk and Converse, it also links such changes with the Supreme Court's 1890–1905 revolution in constitutional interpretation and with the drive toward nonpartisan local governments based on at-large elections. Above all, to the extent that this image of the business corporation came to dominate the intellectual world of the Metropole's middle and upper classes, it became very largely a consensual reception, one which found no organized ideological challenge among the overwhelming majority of politically active Americans. Thus it was that the uniquely American personal-registration statute was not only adopted all across the coun-

[4] V. I. Lenin, "Vybory v Uchredital'noe sobranie i diktatura proletariata," ("Elections to the Constituent Assembly and the Dictatorship of the Proletariat"), Dec. 16, 1919. Fifth Russian edition of *Collected Works (Sochinenlye)*, Vol. 40, 1–24 (Moscow, 1958–1965).

[5] For an extremely suggestive, probably parallel case, see J. H. Plumb, *The Origins of Political Stability in England, 1675–1725* (Boston: Houghton Mifflin, 1967); and cf. Robert H. Wiebe, *The Search for Order, 1877–1920* (New York: Hill and Wang, 1967).

try, but remained very largely immune from serious academic or other criticism until very recently.

As I have said before, this antipartisan, corporate Utopia was nearly achieved in the 1920s, after some decades of development after the legal-structural changes which are supposed to explain so much. By that time, its near-achievement was the fruit of behavioral development in the electorate. Not until the great crash of 1929 was the corporate idol partially dethroned. Calvin Coolidge spoke not just for himself when he commented that the business of America was business; that a factory was a temple, and that he who worked there worshipped there. The point which my supposedly "conspiratorial" view of elite behavior between the 1890's and World War I sought to convey is that, under the given historical and sociological conditions of time and place, no viable alternative to this idolization of the corporation existed. I suppose that it is a truism of history that any search for political order which has achieved long-run success in its objectives has succeeded because it better corresponds in some way to the realities of social and economic power differentials in a given society than either the preceding disorders on one hand, or all imaginable alternative orders on the other. No doubt it is time to cease vain nostalgic heartburnings over what might have been after 1900 in American politics, for what might have been never happened after all. But it was certainly time in 1965 to point out a truth to my colleagues and others that a very high human price was paid for the deformation of democracy which dominated that era. An astonishingly large part of the profound political and constitutional crisis through which we are now living can be traced directly back to this deformation and its subsequent evolution; nowhere more so than in the internal structures of Congress and its linkages to the public through the electoral process. Let him who chooses to be "optimistic" about all this be so. My own effort, such as it has been, has not been to establish grounds for optimism or pessimism but to sense out the workings of a complex political system across time, to count costs, and to call things by what appear to be their right names.

One small analogic point about current electoral behavior needs to be made before I conclude. A basic thesis which underlay the 1965 article and all else which has followed is that a system tends to act as a system across time: changes in one measurable component of voting behavior can be by this time axiomatically postulated to be closely associated with changes in another. They are mutually interdependent. It is rather curious, in all this debate, that no one yet has dealt with post-1960 developments in our political system, or the arguments which have been made elsewhere about their implications. For reasons of space, I shall not attempt to present a full-scale documentation of the points I am about to make; but anyone who does the analysis on the file I will discuss will see for himself.

V. O. Key's classic *Southern Politics*[6] provides us with a simple profile of interrelated behavioral phenomena pertaining to that region, which shows the following: In the absence of party competition, turnout tends to be low. In the absence of stable party referents, electoral coalitions tend to be *ad hoc*, i.e., highly fluid from election to election and, within the same election, from office to office. When such a pattern exists, the probabilities are very high that "friends-and-neighbors" effects will become visible in election results: major candidates coming from a specified geographical area will enjoy disproportionate support from the voters of that area. One could go on, but this will suffice. We turn now to quite recent developments in the state of West Virginia—again, no doubt, a very limited selection, but one whose major features are visible very widely around the country. One reason for choosing this state is its historical record both of turnout and of very high levels of party-oriented voting behavior; and also that all of its major statewide offices since 1872 have been chosen in presidential years.

Analysis of these files leads to some unambiguous conclusions:

(1) No significant change has occurred in the distribution of party registration in West Virginia since 1956. Such change as there is has been in a glacially pro-Democratic direction.

(2) Since 1952 partisan decomposition has geometrically increased, as measured cross-sectionally across the array of offices for which the state at large is the constituency. Even leaving aside the increasingly discrete coalitional structures found in elections for federal offices, these differentials in outcomes have strongly increased (e.g., from a standard deviation of 0.98 in 1952 to one of 4.19 in 1972, with all contests in both years being two-party contests).

(3) While in 1940 a partisan intercorrelation of major-office voting reveals virtual identity in geographically partitioned coalitional structure, by 1972 severe decomposition has occurred. (Thus, county-level Presidential-Congressional correlation of per cent Democratic of the two-party [and total] vote in 1940 yields an r of $+.988$, and a gubernatorial-congressional r of $+.994$. By 1972 these figures had fallen to $+.536$ and $+.503$, respectively.)

(4) Increasing insulation of congressional incumbents has been detected by a number of

[6] (New York: Knopf, 1949).

scholars as a major national characteristic of recent voting behavior. This phenomenon is also conspicuous in West Virginia. The last time any of the state's incumbent congressmen was defeated in a general election was in 1958. Examination of congressional elections in the First ("Panhandle") congressional district reveals that the breakaway toward dissociation of the congressional vote from voting for other offices can be traced to the 1960 election: Nixon received 46.8 per cent of the vote in the district while the second-term incumbent, Arch Moore (R), received 60.3 per cent. It should be noted, first, that this distribution occurred within district boundaries which had not been touched since 1916; and second, that—until 1960—this district had been one of the most durably competitive of any in the state or the country. By 1972, in an expanded district, we find McGovern winning 35.8 per cent of the district's vote and Robert Mollohan (D), the incumbent congressman, winning 69.4 per cent, a gap of 33.6 percentage points.

(5) With all this decomposition of the partisan intermediate between the voter and the candidates /offices on the ballot, one might well expect that sooner or later "friends-and-neighbors" effects might emerge. In 1972 they became generally visible, even for such relatively minor statewide offices as Attorney General. We may also examine one specific case, Mineral County. This county is the home of Harley Staggers (D), who has represented the Second Congressional District uninterruptedly since 1948. Here again, 1960 must be viewed as a "breakaway" year. While the mean party vote for all offices in this county reveals extremely close and long-lasting party competition (the range being between 42.1 per cent [1956] and 58.4 per cent [1964] Democratic of the two-party [and total] vote), Staggers moved into the noncompetitive range with 64.9 per cent in 1960, a lead of 16.8 per cent above the mean. By 1972, the mean for all offices in this county was 50.4 per cent Democratic; Staggers, receiving 79.1 per cent of the vote cast, led this mean by 28.7 per cent.

(6) One might, then, anticipate that whatever factors have been at work to reduce the intermediate influence of party on voting decisions might be closely linked in time with a decay in voting participation. Again, this is what we find, for both presidential and off-year elections. In 1972 the turnout rate for president fell to 64.9 per cent in West Virginia. One must go back prior to the re-admission of ex-Confederates to the state's electorate in 1870 to find a lower rate of participation. In the off-year contests, the decline in participation has been even more dramatic, reaching an all-time low of 40.3 per cent of the potential electorate in 1970. This, it should be noted, was an election with an important office (U.S. Senator) on the ballot; yet not even in the 1920's, or in the wartime election of 1942, were these depths reached. They contrast markedly with turnout rates such as 84.8 per cent in the 1890 off-year election, 84.0 per cent in 1894, or even the 65.7 per cent level reached in 1934. Very probably, the fact that the incumbent Senator, Robert C. Byrd (D) won with 77.6 per cent of the vote had some relationship to this massive voter apathy: unless one includes the uncontested 1863 and 1864 state elections, no precedent can be found for so sweeping a majority in a nominally partisan contest in this state.[7]

Now, all of this emergent behavioral pattern is obviously and closely linked in time with a major political crisis in the affairs of the American people. One of the most conspicuous aspects of this crisis has been a steep upward movement in anxiety and alienative responses to questions such as "People who are in charge of things don't care what I think." Obviously, a series of sample surveys of West Virginia's voters would be required to ascertain whether such an increase in alienation exists there, and if so whether and how it is linked to the size and behavior of the state's active electorate. But one can reach a less assertive position. First, the American electoral system is extraordinarily complex, reflecting as it does a complicated constitutional scheme involving federalism and the separation of powers. Second, as Key has pointed out, "party is the solvent of federalism," i.e., it permits integration of coalitions and mass demands (if any) across office-specific and geographical boundaries. Third, a decomposition of the relevance of party, for whatever reason, will be closely associated with the voter's quest for other cues to his voting decision. One of these cues may be the visibility of legislative incumbents; another, the chance that a major-party candidate comes from his home town. Fourth, the net effect of these developments will be a *radical reorganization* of American electoral politics along office-specific (from the voter's perspective: candidate-specific)

[7] The decay in presidential participation from 1960 through 1972 is one of the most significant aspects of recent American electoral politics. It cannot in any way be viewed as primarily due to the addition of the 18–20 age cohort to the electorate. It is easy enough to demonstrate that if as few as 35 per cent of this cohort voted in 1972, turnout in previously enfranchised age cohorts could not have been higher than 57.4 per cent. The range of turnout in 1972 among previously enfranchised age cohorts lay almost certainly between 56.5 per cent and 57.4 per cent. Moreover, if one excludes the eleven ex-Confederate states from analysis, turnout plunged in 1972 to levels very little above their all-time lows of 1920 and 1924. After more than fifty years of women's suffrage, it is highly unlikely that women can be blamed very much for this result.

lines. Fifth, this will immensely increase the conservative policy effects of the constitutional structure, very much along the lines hoped for by Madison in the Tenth Federalist. Key's solvent is itself dissolved. Sixth, while those voters who are still active will seek heterogeneous nonparty cues for their voting decisions, this reorganization will also serve to baffle and confuse a large number of citizens. The constitutional structure is immensely involuted, as every "a-bill-becomes-a-law" study duly reveals. When the simplifications provided by party evaporate, the voter is faced with a naked complexity which is sometimes unfathomable even to holders of Ph.D.'s in political science. Under such conditions, it would be only to be expected that participation rates would decline *pari passu* with increases in decompositional indices. On a smaller scale, this trend seems remarkably similar to those of the early twentieth century's "changing political universe."

It is somewhat refreshing, after all, to conclude that a system is behaving like a system. In its development there is a proper, and a properly subordinate, role for legal-structural change as an intervening variable. That proper role has yet to be worked out satisfactorily; certainly it has not been so worked out by the authors under review here. From another perspective, things are not at all so refreshing. As we are in the process of learning, the liquidation of party-in-the-electorate is associated with the disappearance of democracy, and *therefore* with the suppression of a vital element in the maintenance of political stability and legitimacy in the United States. In this connection, I cannot forbear to mention Gordon Strachan's Watergate testimony of July 23, 1973. He claims that a policy decision was made in the White House to use funds so as to avoid serious Republican contests to two classes of legislators: Democrats who supported the president's Vietnam war position, and another group of Democrats backed by organized labor, whose support Nixon sought for himself in 1972. The inferences which Senator Weicker made from this disclosure were precise. First, this ensured from the start that the Republican party could have no chance to win control of either house of Congress. Second, this reflected an aggrandizement of the president's personal power at the direct expense of the continuing electoral viability of his own party.

So it is that deliberate elite strategies may reinforce or accelerate trends toward the liquidation of party-in-the-electorate. Whatever the situation around 1900, the Watergate affair *in toto* and this particular episode in particular are useful reminders that conspiracies sometimes do exist. Sometimes, in fact, a theory of specific conspiracy best fits the ascertainable facts. Nor is there any very good reason to doubt that the decay of party-related voting behavior in our time has an intimate relationship—of proximity and interaction, if not of causality—with the crisis of constitution and legitimacy of which Watergate is currently the leading symptom. Who gains if party disappears and Madison's dream is finally realized, even in a shape he would never recognize? Is it really necessary, at long last, to ask?

By all means, therefore, let us have a complete replication of *The American Voter*, based both upon current surveys and upon careful longitudinal analysis of attitudinal and voting changes and consistency from the Eisenhower to the Nixon eras. This replication will serve the important scholarly purpose of bringing together in a single place the findings of many recent, scattered articles, and of reconciling, modifying or rejecting their arguments. It will not, alas, tell us anything conclusive about the nineteenth-century American electorate: the sociological structures of that era, and the political democracy which they nourished, have vanished. But it will tell us a lot about the intrusion of stress upon American citizens in today's world. One can confidently predict that the exercise will be worth doing both for what is discovered and for what needs to be recombined and rethought. We can continue at least to hope and to suppose that, by the time they appear, the findings of such a replication will be of more than historical interest for scholars and practitioners of American politics.

Social Change and Political Mobilization in the United States: 1870–1960*

JESSE F. MARQUETTE
The University of Akron

In the literature on political change, the achievement of mass participation is viewed as a crisis which must be passed if the process of political development is to continue.[1] Mass participation is seen as highly significant even when such participation is nothing more than a public ritual. Since citizen participation in the political process has been shown to correlate with macro-social characteristics in a variety of cultural contexts,[2] accurately specifying these social antecedents of the diffusion of mass political participation is a major concern of students of political change.[3] The widespread evidence of this correlation has provided a large measure of the empirical justification for viewing mass participation as a co-equal factor in the development syndrome. Neither this shared theoretical concern, nor the associated empirical research, have as yet produced any reliable contributions to the general theory of political development or a more limited theory of political mobilization.[4]

Using data drawn from the preceding century of American history, this paper will attempt to resolve the problems which have plagued previous research efforts and to develop a reliable causal model of the social bases of political mobilization in one society, the United States. Two factors motivated the choice of the United States as the object of research. First, the United States has undergone a very rapid process of economic, social, and political change during the past century. Second, the period under consideration may be reliably examined with data available in a number of published sources. It therefore presents the possibility of conducting, with sound data, a longitudinal examination of a society undergoing fundamental social and political redistributions.[5] The data points of the research are the states of the United States at each decennial census from 1870 to 1960.

A Critique of Previous Research

Previous research on the process of political mobilization and social change has been possible only by virtue of a fundamental, and erroneous, assumption. That assumption is quite simply that the process of change is itself unchanging. We have absolutely no empirical basis for such an assumption, and evidence will be shortly presented to show that the assumption is, in fact, incorrect.

It is impossible to utilize cross-national data to test hypotheses about development sequences without assuming an invariant process. It is only by making this assumption that an argument may be developed in which one society, exhibiting lower orders of magnitude on a variable, may be treated as a temporally posterior representative of a society exhibiting higher orders of magnitude on the same variable. We cannot use data from one point in time to make inferences about change through time without assuming that the form of change is a constant.[6]

* The author would like to thank Professor Alfred B. Clubok of the University of Florida for reading and commenting on earlier versions of this research.

[1] For a review of approaches and research in the area see Myron Weiner, "Political Participation: Crisis of the Political Process" in *Crises and Sequences in Political Development*, ed. Leonard Binder (Princeton: Princeton University Press, 1971), pp. 159–204.

[2] Seymour Martin Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review*, 53 (March, 1959), 69–105; also Philips Cutright, "National Political Development: Measurement and Analysis" in *Politics and Social Life: An Introduction to Political Behavior*, ed. Nelson W. Polsby, Robert A. Dentler, and Paul A. Smith (Boston: Houghton Mifflin, 1963), pp. 771–780.

[3] For a general theoretical review of these linkages see Ioan Davies, *Social Mobility and Political Change* (New York: Praeger Publishers, 1970), pp. 11–46; Daniel Lerner, *The Passing of Traditional Society: Modernizing the Middle East* (Glencoe, Illinois: The Free Press, 1958), pp. 43–75; Donald J. McCrone and Charles F. Cnudde, "Toward a Communications Theory of Democratic Political Development: A Causal Model," *American Political Science Review*, 61 (March, 1967), 72–79.

[4] Samuel P. Huntington, "The Change to Change: Modernization, Development and Politics," *Comparative Politics*, 3 (April, 1971), 283–322.

[5] The data for this research were derived from the various reports of the U.S. Bureau of the Census, *U.S. Census of Population* (Washington, D.C.: G.P.O., 1872, 1883, 1895, 1901, 1913, 1922, 1933, 1943, 1952, 1962).

[6] For a similar critique see Lee Sigelman, "Modernization and the Political System: A Critique and Preliminary Empirical Analysis," *Sage Professional Papers in Comparative Politics*, 2 (1971), 12–13.

One author has recognized this difficulty and attempted to deal with it by treating longitudinal data from only one society.[7] This solution is only partially valid, for the author then proceeds to enter a series of data points, drawn from different points in time, into the same correlation analysis. To treat data drawn from 1790 in the same linear analysis as data drawn from 1960, again requires the assumption of an invariant process. Using longitudinal data we are faced not with the problem of establishing one society as a temporally posterior representative of another, but with the high probability that the correlations generated by the analysis are mere statistical artifacts. In essence, a causal model is a statement of the relationships obtaining among a set of variables derived from their covariation. As the magnitude and rate of particular changes increase or decrease, we would expect an alteration in the observed relations of variables, and thus, in the inferred pattern of the process itself. The pattern of relationship which obtains for an extended intertemporal analysis may, in fact, be nothing more than a bastardized combination of several patterns which prevailed during subsets of the time span under consideration. Assuming an invariant process may allow the researcher to ignore this possibility, but it does not eliminate it, and the question remains whether the assumption or the research results are correct.

In part, the assumption by researchers of an invariant process may be ascribed to the inclusion of that assumption in the argument by Lerner.[8] Most of the causal analysis of the mobilization process has referred to Lerner's work to justify the hypothesized pattern of relations.[9] Although Lerner treats the process as essentially circular, he nevertheless fails to deal with the problem of an alteration in the nature of the process. Yet, this alteration is implicit in the work of those authors concerned with "stages" or "crises" of development.[10] Lerner's failure to deal with the possibility of different patterns of change meant a theoretical position that invited researchers to employ methodology not capable of discerning sub-patterns of the process. The most appropriate methodology for a causal model of the development process is a design providing longitudinal aggregation of cross-sectional analyses derived from only one society.

In very general terms there is agreement on the major factors comprising the modernization process. Social mobilization is seen as one significant characteristic and is understood to be dependent upon major changes in the amount and form of wealth production.[11] Similarly, modernization is expected to entail increases in the level of individual adaptability, whether that adaptability is seen as "empathy,"[12] "innovation,"[13] "achievement orientation,"[14] or simply an acceptance of "modern"[15] values.

The process of modernization is often referred to as a "syndrome,"[16] indicating a general agreement on the high level of interdependence of factors entailed by the modernization process. Beyond this superficial agreement lie major differences in the expected time-order and significance of the elements of modernization. A number of alternative models are therefore possible as we pick and choose among contending theories. Empirical validation of the synthesized model however, is the only method of ultimately choosing the most acceptable

A Tentative Model

The argument upon which the following model will be based proceeds as follows: Increasing wealth leads to increasing social mobilization. Social mobilization can be broken into two major, interrelated processes—urbanization and mass communication—each more specific and more easily operationalized. In the following discussion, urbanization will be considered only in terms of intense demographic concentration. Industrialization, as it occurred under past levels of technology, required extreme demographic concentration. Throughout the history of industrialism, the growth of the cities has been primarily the result of stripping the hinterland of agrarian population. Urbanization did not result merely from natural population increase in pre-existing cities.

[7] Gilbert Winham, "Political Development and Lerner's Theory: Further Test of a Causal Model," *American Political Science Review*, 64 (September, 1970), 810–818.

[8] Lerner, *The Passing of Traditional Society.*

[9] Including McCrone and Cnudde; Winham; and Arthur K. Smith, "Socioeconomic Development and Political Democracy: A Causal Analysis," *Midwest Journal of Political Science*, 13 (February, 1969), 95–125.

[10] Lucian W. Pye, *Aspects of Political Development* (New York: Little, Brown, 1966).

[11] Karl W. Deutsch, "Social Mobilization and Political Development," *American Political Science Review*, 55 (September, 1961), 493–514.

[12] Lerner, *The Passing of Traditional Society.*

[13] Everett E. Hagen, *On the Theory of Social Change: How Economic Growth Begins* (Homewood, Illinois: The Dorsey Press, 1962), pp. 12–16.

[14] David C. McClelland, *The Achieving Society* (Princeton: D. Van Nostrand, 1961) also David C. McClelland and David G. Winter, *Motivating Economic Achievement* (New York: The Free Press, 1969).

[15] Alex Inkeles, "Participant Citizenship in Six Developing Countries," *American Political Science Review*, 63 (December, 1969), 1120–1141.

[16] Pye, pp. 45–48.

Indeed, as Deutsch has noted, in industrializing societies, new cities sprang up as new industries were formed.[17] In a cyclical process, industrialization makes large scale urban populations possible, and increasing urban populations are necessary for further industrial expansion. The byproduct of this process is an ever-increasing number of mobilized individuals who, by their activities, will tend to reinforce both industrialization and urbanization. Urbanization necessitates the second process of social mobilization—mass communication. To create an urban *society*, mass communication is absolutely essential.

In earlier epochs, communication required literacy since the media were print. This has led some theorists to presume that mass literacy is necessary for mass communication.[18] There is, in reality, no reason for mass literacy to precede mass communication. Prior to the development of electronic media, as long as the nodal personality in a sociometric network was literate, the other persons in the net could effectively be considered part of the mass communications system.

A society cannot support urbanization without the large-scale community integration made possible by the process of mass communication. Demographic concentration can continue for some time, but the process of modernization will break down under the pressure and confusion of a nonintegrated environment. An extension of the cycle is apparent; industrialization leads to urbanization, which in turn is necessary for further industrialization. Yet, continued urbanization is not possible without the community integrating functions of mass communication.

Mass communication has a significant impact on the further development of mobilized individuals. Where urbanization acts as the first stage of social mobilization, mass communication acts as a second stage by extending the range of increased individual aspirations, making more individuals aware of currently available alternative life styles.

As the range of alternative roles and life styles is increased through the introduction of new forms of wealth production, the old social order is subjected to extreme stress and individuals are forced into new patterns of activity.[19] Increasing social mobilization will lead to pressure for higher individual adaptability. At this juncture, social stress becomes reflected in the governmental sphere. Individuals are faced with the unpleasant realization that their abilities are not commensurate with their aspirations. It has been argued that social mobilization destroys the individual's links with his previous social position. In order to exist in his new situation, the individual must find new forms of behavior in which to secure himself. He must, in short, take on new roles. Yet his ability to fill new roles is contingent upon his level of general adaptability. If the rate of mobilization is slow, learning from experience will be sufficient to perpetuate modernization for a time. Repeated exposure to modern life styles may simply inure the individual to the harassments of modernity. The more rapid the original rate of mobilization, however, the more necessary an efficient form of education will be.[20] Insecurity and maladjustment are common products of repeated failures to cope in the face of an incomprehensible social order. As the number of maladjusted individuals increases, the ability of the remaining mobilized individuals to sustain the process of modernization decreases. Hence, the process of social mobilization creates a situation in which education becomes increasingly necessary. In terms of an entire society, mass formal schooling becomes necessary if the process of modernization is to continue.

Formal schooling provides the only method of increasing individual adaptability in the mass of the mobilized populace. As the rate of social change increases, governmental activity becomes unavoidable. Were it possible to generate mass education without state intervention, then the process of modernization would continue without the state. Mass education is a social service with no immediate return, and government activity is thus an intervening variable between the pressures resulting from mass mobilization and the increased need for education. Government activity is obviously necessary to the process of social modernization, yet it is not a logical link in the chain of causality. It is the *rate* of modernization which renders non-governmental solutions to the problem impossible.

This increase in the adaptability of the mobilized members of the society afforded by mass education insures the continuation of the process.

To develop a logically satisfying end to this chain, our comprehension of politics must compel our perception of political mobilization as a necessary response to the social changes described in the model. Such a perception may be assured only by employment of a delimitation of the concept "politics" which underscores its relation to social

[17] Karl W. Deutsch, *Nationalism and Social Communication: An Inquiry Into the Foundations of Nationality* (New York: Wiley, 1953), pp. 97–126.

[18] See Lerner and also McCrone and Cnudde.

[19] Gerhard E. Lenski, *Power and Privilege: A Theory of Social Stratification* (New York: McGraw-Hill, 1966), pp. 31–32.

[20] In fact, much modernization research used the formal educational attainment of the populace as a measure of modernization, thus framing the argument in terms of education as an index of modernity instead of as a means to modernity. See Lerner and also Deutsch, "Social Mobilization and Political Development."

tension. The chain of events described in the model is tension producing; but in what sense does it necessitate political mobilization?

This research employed the following working definition of politics: *The attempted or actual use of the authority of the state to redistribute the life-chance matrix of the society to a configuration which is not legitimizable by traditional social norms.* This definition emphasizes the nonsubstitutable nature of political activity, and also meets the criticism that the definitions of Lasswell and Easton are also descriptions of the stratification system.[21]

Social processes of economic change and social mobilization disorient the norm structure of the previous stratification system, introducing new kinds of roles and valued goods. In large measure, the pre-existing norm structure contains referents neither for the appropriate *form* of a new distribution of valued goods, nor an appropriate *means* of agreeing upon one.[22] Therefore, political activity —recourse to the generalized decision processes of the state—becomes a necessary substitute for customary agreement. As the rate of change increases, there is correspondingly greater pressure to generalize and externalize decision processes.

Over an extended span of time the modernization process may continue only with the assumption by the state of responsibility for mass education. This assumption of responsibility is, in itself, a major political decision, although in terms of the mass the decision is undertaken by a small minority of the total society. Forces generated by the processes of social change may produce pressure for widespread education, but the political involvement of the mass of the populace is contingent upon the increasing adaptability fostered by education. Mass political mobilization serves to legitimize the new distribution of life chances, and also to provide an outlet for frustrations regarding perceived imbalances in the current distribution.[23]

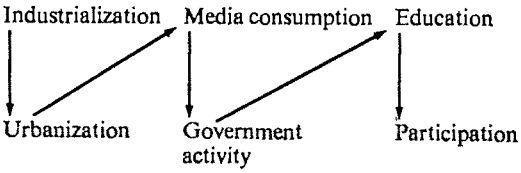


Figure 1. Proposed Sequence of Relationships

This hypothetical sequence completely reverses the literacy/media aspect of Lerner's argument.[24] Lerner's presentation is derived from the perspective of the individual, and is thus bound to the sequence by which an individual moves from lesser to greater empathic ability. From a more general perspective of social change and adaptation, however, it is realistic to argue that desire precedes ability, and that the wish generated by exposure to modern activity leads to pressure for a means to fulfill that wish.[25] As a first approximation, this research will test the sequence of change depicted in Figure 1.

It must be kept in mind that this pattern is not expected to hold for the entire century under consideration. In 1870, at the beginning of the period, the United States was basically an agrarian society, despite earlier changes prerequisite to rapid industrialization.[26] As a pattern of change, the model is expected to be empirically valid only for an early portion of the century, and subsequent alterations in the relationships of the factors will be identified as they occur.

Methodology

As noted earlier, the major objection to the methods of previous researchers was directed not at the process by which they identified their models, but with the methods they used to generate their base correlations. Either the correlations were drawn from a single cross-section, or they dealt with longitudinal data by ignoring possible changes in the process under study.

[21] Lenski, p. 3ff.

[22] There is an interesting parallel here between Lenski's discussion of institutional evolution of stratification systems, and Thorstein Veblen's *The Theory of the Leisure Class* (New York: Viking Press, 1912), pp. 60–70. At both ends of Lenski's spectrum, hunting and gathering societies and mature industrial societies, we find relatively loose stratification systems. Veblen's discussion of conspicuous consumption suggests that in primitive societies and industrial societies, conspicuous consumption is a right, whereas in traditional societies (Lenski's agrarian societies) it is a privilege. In both forms of society where conspicuous consumption is a right of the mass, we find loose stratification systems and no perception of limited good. Given the absence of limited good, and the flexibility of the stratification systems, conspicuous consumption is an obvious and probably necessary means of identify definition. Veblen argues that conspicuous consumption is a means of maintaining reputation, a primary component of individual identity. David Riesman's discussion of the "inner-directed" and "other-directed" individual follows similar logic; see *The Lonely Crowd: A Study of the Changing American Character* (New Haven: Yale University Press, 1964). For a recent analysis of this interaction see Orrin Klapp, *Collective Search for Identity* (New York: Holt, Rinehart and Winston, 1969).

[23] Weiner, "Political Participation," and Inkeles, "Participant Citizenship," pp. 1123–1124.

[24] Lerner, *The Passing of Traditional Society.*

[25] Jesse F. Marquette, "Social Mobilization and the Philippine Political System," *Comparative Political Studies,* 4 (October, 1971), 339–347.

[26] Seymour Martin Lipset, *The First New Nation: The United States in Historical and Comparative Perspective* (New York: Basic Books, 1963); Walter W. Rostow, *The Stages of Economic Growth: A Non-Communist Manifesto* (Cambridge: Cambridge University Press, 1960).

To correct these deficiencies the following procedure was employed:

For each decennial census period a cross-sectional correlation matrix was generated using the states of the United States as data points. The data points used were those states which were so recognized at the time of the respective census; hence, certain southern states were not included during the period 1870 to 1880. Territories were added to the data set as they were admitted as states, beginning with the next following census period. For each pair of variables, the decennial correlations were then averaged over the total time span. Averaging was carried out by the use of an adjustment formula which controlled for the relative size of the correlations and the original number of data points on which they were based.[27] This method of averaging gives the final average correlation a reliability equal to the total number of data points rather than to the decennial base. Simon-Blalock causal analysis was then conducted on the aggregate correlation matrix for the entire time span under consideration.[28]

As expected, the model was not found appropriate for the entire century. The aggregate matrix for the entire period was then disaggregated into two periods by reference to the intertemporal alterations of relation between the base correlations. This dis-aggregation resulted in the identification of two models. One, corresponding to the proposed model, was identified for the period 1870 to 1910. The second, covering the period 1920 to 1960, was largely similar to the first, but some modifications appeared in the last stages.

After the two successive phases of the model were identified by cross-sectional analysis, a new set of correlation matrices was generated. These matrices were based on time lagged correlations; data from 1870 were correlated with data from 1880 and so on, through the entire period. The procedure outlined above was then repeated on this new set of matrices, and the existence of the two successive models previously identified was reconfirmed.

The operational indicators of the six concepts, and their component variables are reported in Table 1. Where relevant, the method of combining variables into indices was simple summing, all component variables having been computed on the same base.

Election data were averaged to a decennial base to give an aggregate participation index, Elections for 1868 and 1872 were averaged for the participation index for 1870, the procedure being repeated for the entire period.

Both procedures of testing the model were repeated substituting off-year Congressional election turnout for presidential turnout. The results are not reported in detail because of their redundancy; the analysis of these data confirmed the models identified with the presidential data.

This study uses voting in presidential elections as a proportion of the total population. This usage is dictated by the nature of the model under study. The model is designed to depict *political mobilization*, not simply *political participation*, and the measure of political mobilization is affected not only by whether an individual chooses to participate in the system, but also by whether the system will allow him to participate. Since at any point in time, legal modifications could alter the composition of a state's eligible electorate, measuring the percentage of eligible voters would not take into account the system's control over individual mobilization. In other words, an index based on the eligible electorate, by states, might reflect a state with highly restrictive eligibility (but high turnout of eligible voters) as highly mobilized, while another state with a broad base of eligibility (and the same number of voters) would appear far less mobilized. Such usage would not reflect both aspects of mobilization—the individual and the societal. Concerned with political mobilization, rather than simply political participation, it was necessary to use an index which tapped the totality of the society's involvement in political activity.[29]

Analysis

A correlation matrix composed of the averaged decennial correlations from 1870 to 1960 is presented in Table 2. The model and prediction equations derived from this matrix are presented in Table 3.

As might be expected, the model postulated earlier is clearly not identifiable for the entire period under study. The prediction equations indicate that the first four variables are related in the sequence predicted from the theoretical presentation, but the chain does not follow the expected sequence from media consumption to political mobilization.

To retest the model with subsets of the total

[27] Truman L. Kelley, *Fundamentals of Statistics* (Cambridge: Harvard University Press, 1947), pp. 363–365.

[28] Hubert M. Blalock, Jr., *Causal Inferences in Non-Experimental Research* (Chapel Hill: University of North Carolina Press, 1964).

[29] For an excellent analysis of the misleading effect of changes in electoral laws and practices on the conclusions of historical patterns, see Philip E. Converse's critique of Walter Dean Burnham's "The Changing Shape of the American Political Universe," *American Political Science Review*, 54 (March, 1965), 7–28; Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change*, ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), pp. 263–337.

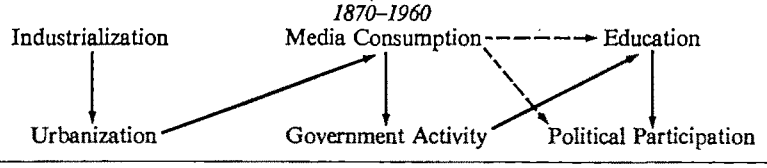
Table 1. Variables Included in Each Final Index with Years of Inclusion

Variable	Index Components	Years
INDUSTRIALIZATION	Per capita value added in manufacturing	1870–1960
URBANIZATION	Percentage of population in cities larger than 2,500	1870–1960
	Percentage of population in cities larger than 25,000	1870–1960
MEDIA CONSUMPTION	Average daily newspaper circulation per 1,000 population	1870–1960
	Postal receipts, per capita	1870–1960
	Telephones per 1,000 population	1880–1960
	Radios per 1,000 population	1930–1960
GOVERNMENT ACTIVITY	Per capita state and local government expenditures	1870–1960
EDUCATION	Percentage of population which is literate	1870–1960
	Percentage of the school age population attending high school	1870–1960
	Percentage of the 19 to 24 age group enrolled in college	1910–1960
POLITICAL PARTICIPATION	Voter turnout in presidential elections as a percentage of total population; or	1870–1960
	Voter turnout in off year congressional elections as a percentage of total population	1870–1960

Table 2. Aggregate Correlation Matrix of the Six Indices for the Period 1870 to 1960

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.8044	0.5861	0.4453	0.3218	0.2405
U		1.0000	0.7705	0.5709	0.3831	0.2661
M			1.0000	0.7319	0.5946	0.5140
G				1.0000	0.6554	0.5392
E					1.0000	0.7213
P						1.0000

Table 3. Model and Analysis of the Cross-Sectional Correlations: 1870–1960



Analysis 1870–1960

		Actual − Expected = Difference
IM.U	$IU \times UM$ $.8044 \times .7705$	$.5861 - .6197 = -.03$
UG.M	$UM \times MG$ $.7705 \times .7319$	$.5709 - .5639 = .01$
IG.UM	$IU \times UM \times MG$ $.8044 \times .7705 \times .7319$	$.4453 - .4563 = -.01$
ME.G	$MG \times GE$ $.7319 \times .6554$	$.5946 - .4797 = .11$
GP.E	$GE \times EP$ $.6554 \times .7213$	$.5392 - .4727 = .06$
MP.EG	$MG \times GE \times EP$ $.7319 \times .6554 \times .7213$	$.5140 - .3460 = .17$

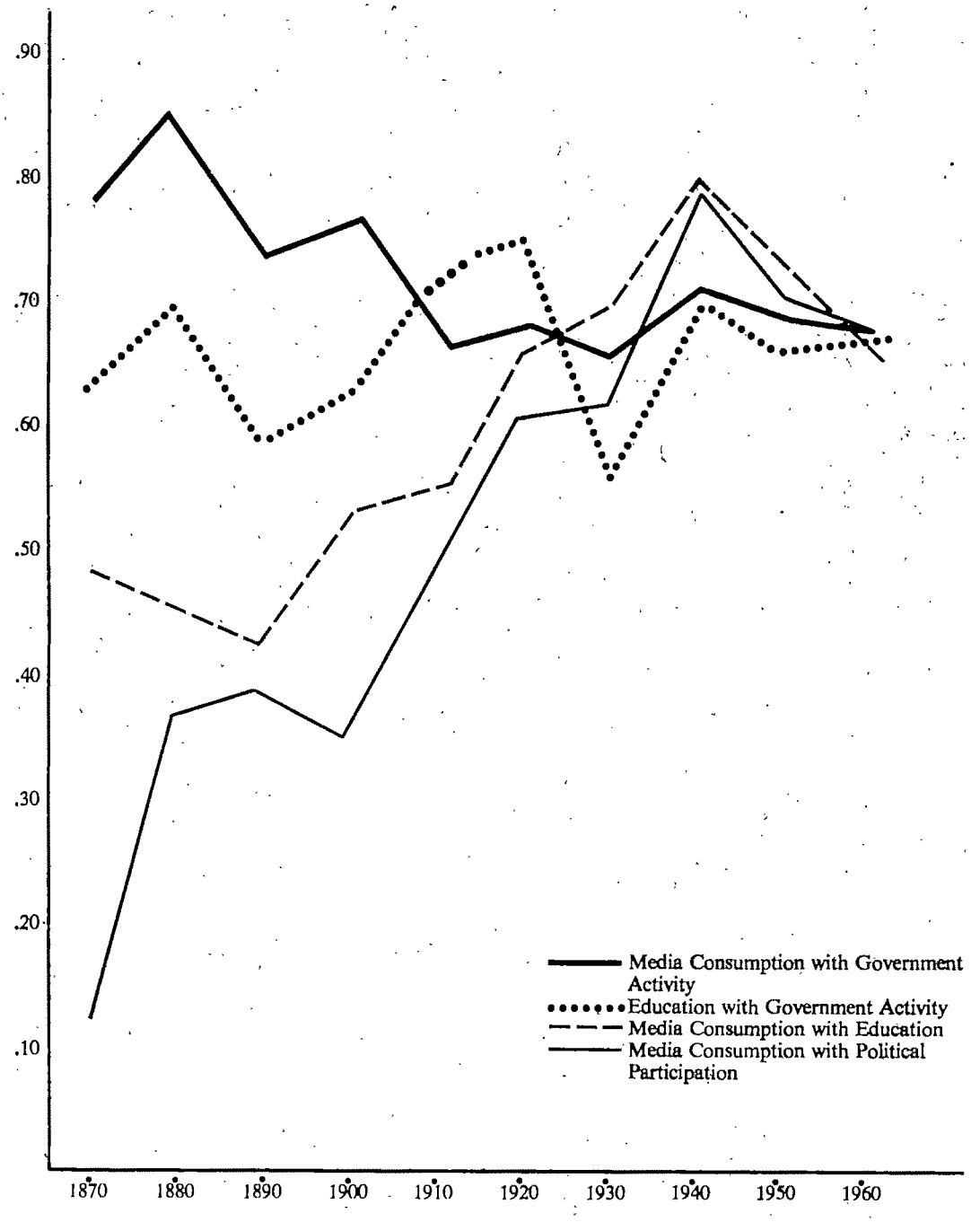


Figure 2. Plot of Correlation Coefficients of Media Consumption, Government Activity, Education, and Political Participation Versus Time: 1870 through 1960

period under study it is necessary to decompose the matrix in Table 2 into two or more submatrices. The significant question is the identification of the appropriate time periods on which to base the new correlations. It was decided that the best procedure was to resort to an examination of the intertemporal behavior of the relevant correlation coefficients.

Figure 2 is a plot of the correlations among media consumption, education, government activity and political mobilization by census years. The plot of these correlations indicates that at some time during the period 1915 to 1925, the magnitude of the correlations became very similar, and began to vary in much the same fashion. In particular, the track of the correlation between

Table 4. Aggregate Correlation Matrix of Six Indices for the Period, 1870 to 1910

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.8875	0.6230	0.5635	0.4542	0.1204
U		1.0000	0.7670	0.6280	0.4349	0.1665
M			1.0000	0.7578	0.4986	0.3621
G				1.0000	0.6545	0.5075
E					1.0000	0.7160
P						1.0000

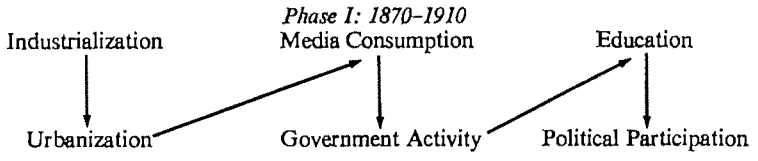
media consumption and political mobilization suggests an increasing direct relationship between these two variables. As illustrated in Table 3, the inability to partial out mobilization was one of the major difficulties with the test of the model over the entire century. On this basis it was decided to decompose the matrix for the entire century into two matrices covering the periods 1870 to 1910 and 1920 to 1960.[30]

The aggregate correlation matrix for the period 1870 to 1910 is presented in Table 4. Table 5 presents a model and set of prediction equations for the period much more gratifying to the original expectations. All prediction equations in Table 6 are well within the range of the minimum .10 cutoff suggested by Blalock.[31] The analysis in Table 5 provides support for the argument advanced earlier. Tables 6 and 7 present the correlation matrix, model and prediction equations for the time period 1920 to 1960. The model in Table 7 is a replication of the relationships found for the period 1870 to 1960. However, the size of the differences of the expected and actual correlations for the paths *Media Consumption—Government Activity—Education* and *Media Consumption—Government Activity—Education—Political Participation*, are much larger than the respective differences for the entire time-period test. This increase indicates that the model for the entire time period was unclear because of the altered nature of the relationships which prevailed during the second half of the period under study.

[30] Interestingly enough this empirical cutpoint corresponds closely to the transition from the "maturity" period to the "high mass consumption" period suggested by Rostow for the U.S., Rostow, pp. 75–76.

[31] Blalock, *Causal Inferences in Non-Experimental Research*.

Table 5. Model and Analysis of the Cross-Sectional Correlations: 1870–1910



Analysis
Phase I: 1870–1910

		Actual − Expected = Difference
IM.U =	IU × UM .8875 × .7670	.6230 − .6807 = −.05
UG.M =	UM × MG .7670 × .7578	.6280 − .5812 = .04
IG.UM =	IU × UM × MG .8875 × .7670 × .7578	.5635 − .5158 = .05
ME.G =	MG × GE .7578 × .6545	.4986 − .4959 = .002
GP.E =	GE × EP .6545 × .7160	.5075 − .4686 = .04
MP.EG =	MG × GE × EP .7578 × .6545 × .7160	.3621 − .3551 = .007

Table 6. Aggregate Correlation Matrix of Cross-Sectional Correlations: 1920–1960

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.7675	0.5626	0.3346	0.1850	0.3690
U		1.0000	0.7833	0.5140	0.3304	0.3772
M			1.0000	0.6882	0.6992	0.6876
G				1.0000	0.6636	0.5946
E					1.0000	0.7614
P						1.0000

Before attempting to deal with the lack of clarity in the second phase of the process, a re-examination is conducted by developing inter-temporal correlation matrices for the two phases of the model. These time-lagged correlations enable an examination of the behavior of the model through time, and eliminate the possibility of mutual causation.

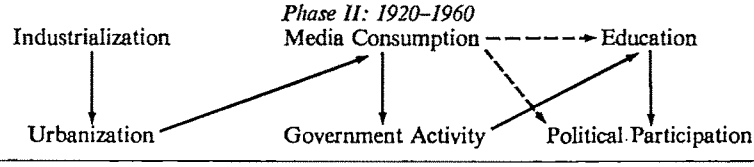
Tables 8 and 9 present the correlation matrix, model, and analysis for the period 1870–1910. The analysis in Table 10 replicates the conclusions which were derived from the cross-sectional analysis in Table 5. During the early period of major industrial and social change the model proceeds as expected.

The data and analyses in Tables 10 and 11 also replicate the finding reached for the second period through the cross-sectional analysis. Again, unexpected direct relationships are observed among media development, education and participation Clarification of the causal priorities obtained during the period 1920–1960, is possible only by reference to the conceptual basis of the proposed model.

The original argument postulated a relationship between increasing available wealth and social mobilization. It was argued that industrial development necessitated media development. Urbanization was understood to be an intervening variable, primarily because of the characteristics of the productive process. Similarly, media development was expected to necessitate increased government activity which, in turn, fostered educational development. Government activity was understood to be a necessary intervening variable because of the extreme cost of developing an educational establishment.

Both urbanization and government activity, as

Table 7. Model and Analysis of the Cross-Sectional Correlations: 1920–1960



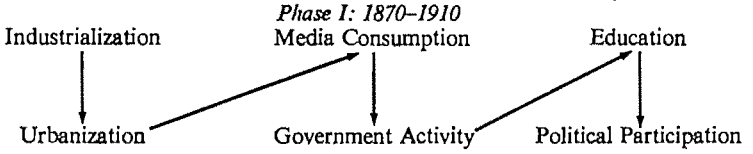
Analysis
Phase II: 1920–1960

		Actual − Expected = Difference
IM.U	= IU × UM .7675 × .7833	.5626 − .6000 = −.04
UG.M	= UM × MG .7833 × .6882	.5140 − .5286 = −.01
IG.UM	= IU × UM × MG .7675 × .7833 × .6876	.3346 − .4100 = −.07
ME.G	= MG × GE .6882 × .6636	.6992 − .4455 = .25
GP.E	= GE × EP .6636 × .7614	.5946 − .5033 = .09
MP.EG	= MG × GE × EP .6882 × .6636 × .7614	.6876 − .3391 = .32

Table 8. Aggregate Correlation Matrix of the Intertemporal Correlations: 1870 to 1910

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.8805	0.6176	0.4935	0.3937	0.1677
U		1.0000	0.7414	0.5383	0.3855	0.1815
M			1.0000	0.6718	0.4715	0.3636
G				1.0000	0.6669	0.4926
E					1.0000	0.7613
P						1.0000

Table 9. Model and Analysis of Intertemporal Correlations: 1870–1910



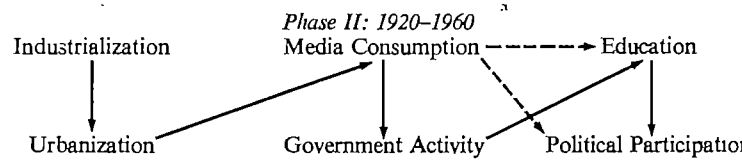
Analysis
Phase I: 1870–1910

		Actual − Expected = Difference
IM.U =	IU × UM .8805 × .7414	.6176 − .6528 = −.03
UG.M =	UM × MG .7414 × .6718	.5383 − .4981 = .04
IG.UM =	IU × UM × MG .8805 × .7414 × .6718	.4935 − .4386 = .05
MG.E =	MG × GE .6718 × .6669	.4715 − .4480 = .02
GP.E =	GE × EP .6669 × .7613	.4926 − .5077 = −.01
MP.EG =	MG × GE × EP .6718 × .6669 × .7613	.3636 − .3406 = .02

Table 10. Aggregate Correlation Matrix of the Intertemporal Correlations: 1920 to 1960

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.7323	0.5941	0.3950	0.2084	0.4396
U		1.0000	0.7974	0.4710	0.3513	0.4482
M			1.0000	0.6223	0.6969	0.7060
G				1.0000	0.6637	0.6937
E					1.0000	0.7454
P						1.0000

Table 11. Model and Analysis of Intertemporal Correlations: 1920–1960



Analysis
Phase II: 1920–1960

		Actual − Expected = Difference
IM.U =	IU × UM .7323 × .7974	.5941 − .5839 = .01
UG.M =	UM × MG .7974 × .6223	.4710 − .4962 = −.02
IG.UM =	IU × UM × MG .7323 × .7974 × .6223	.3950 − .3634 = .03
ME.G =	MG × GE .6223 × .6637	.6969 − .4130 = .28
GP.E =	GP × EP .6637 × .7454	.6937 − .4947 = .20
MP.EG =	MG × GE × EP .6223 × .6637 × .7454	.7060 − .3079 = .39

they relate to political mobilization, may be understood as infrastructures. In the early phases of the development process, the social costs of urbanization, and the development of an educational plant must be taken into consideration. It is however possible that, as suggested by Deutsch,[32] there exists a threshold effect in terms of the establishment of basic infrastructures. While it is obvious that urbanization and governmental expenditures for education continue, in time the threshold effect may render them *causally* insignificant.

In this view the general argument for the process of political mobilization remains the same. Increasing wealth production causes social mobilization, which necessitates increasing individual adaptability if the process of mobilization and wealth production are to continue. Increasing individual adaptability makes political mobilization possible as a generalized response to the social dislocations inherent in the earlier stages of the process. Given the expected threshold for the urban and educational infrastructures, we are now discussing a four-variable not a six-variable model. The expected sequence of the four variable model is *Industrialization—Media Consumption—Education—Political Participation.*[33]

Since the division of the process into two phases has twice been confirmed, the final portion of the research employs somewhat different methodology, focusing on the examination of the process of political mobilization as an example of change through time. In this section all states will be entered into one analysis for each of the periods 1870–1910 and 1920–1960.[34] This results in two correlation matrices based on 212 and 240 cases, respectively.

Tables 12 and 13 deal with the new correlations for the period 1870 to 1910. The prediction equations in Table 13 again indicate a satisfactory solution for the model in six variables. The model identified through this technique is a replication of the model generated through the use of cross-sectional and intertemporal correlation bases.

Finally, we may now examine the contention that urbanization and government activity are functioning as infrastructures by examining the data and model presented in Tables 14 and 15. Clearly the expectations were correct, the model is now reduced to the proposed four-variable case. This four-variable model is much closer to a direct representation of the processes of interest than the six variable model, which, necessarily, contained indices of developing infrastructures.

[32] Deutsch, "Social Mobilization and Political Development," pp. 496–497.

[33] For a similar model see Marquette, "Social Mobilization and the Philippine Political System," p. 340.

[34] Reported correlations are based on untransformed data, although all correlations were checked by logarithmic transformations since some of the growth rates were slightly curvilinear.

Table 12. Total Data Point Correlation Matrix: 1870–1910

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.8871	0.6053	0.5777	0.4861	0.1201
U		1.0000	0.6873	0.6060	0.4581	0.1767
M			1.0000	0.8192	0.6881	0.3378
G				1.0000	0.7542	0.4841
E					1.0000	0.5315
P						1.0000

Discussion

At the outset of this research it was suggested that the identification of a longitudinal model of the process of political mobilization would be a useful contribution to the study of political change. A model of this process for a one hundred year period of American history has been identified. Yet that model is merely a description of a set of relationships between variables and does not, by itself, constitute an explanation.

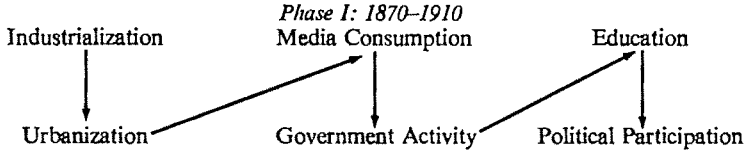
The original depiction of the proposed model was motivated by an implicit theory of change. In essence, that theory is based on a reformulation and synthesis of the social process and identity formation approaches to change.[35] To provide an explanation for the process represented by the model it is necessary to make that theory explicit.

Some of the most imaginative treatments of change have proceeded from the concept of social tension. Basically tension has been understood as a result of an imbalanced relationship between certain culturally established desires and the available means of satisfying them. In its original formulation this treatment was confined to a discussion of different alienated responses to such imbalance. In *Social Theory and Social Structure*, Merton advanced a paradigm of alienation based on a comparison of the two variables, cultural goals and institutionalized means.[36] Although not so treated by Merton, to maintain consistency

[35] Huntington, "The Change to Change."

[36] Robert K. Merton, *Social Theory and Social Structure* (Glencoe: The Free Press, 1949), pp. 125–149.

Table 13. Model and Analysis of Total Data Point Correlations: 1870–1910



Analysis
Phase I: 1870–1910

		Actual − Expected = Difference
IM.U =	IU × UM .8871 × .6873	.6053 − .6097 = −.004
UG.M =	UM × MG .6873 × .8192	.6060 − .5630 = .04
IG.UM =	IU × UM × MG .8871 × .6873 × .8192	.5777 − .4995 = .08
ME.G =	MG × GE .8192 × .7542	.6881 − .6178 = .07
GP.E =	GP × EP .7542 × .5315	.4841 − .4009 = .08
MP.EG =	MG × GE × EP .8192 × .7542 × .5315	.3378 − .3284 = .009

Table 14. Total Data Point Correlation Matrix: 1920–1960

	Industrialization	Media	Education	Participation
I	1.0000	0.7355	0.5701	0.3384
M		1.0000	0.8330	0.6141
E			1.0000	0.6729
P				1.0000

with later authors, this relationship may be expressed as a simple ratio:

$$\text{Dissatisfaction} = \frac{\text{Cultural Goals}}{\text{Institutionalized Means}}$$

Merton's discussion of this relationship is oriented toward the identification of modes of adaptation, and he consequently pays little attention to the development of imbalances throughout society.

In later research, Lerner expanded this earlier conception and defined the phenomenon as one which occurred " . . . when many people in a society want far more than they can hope to get."[37] Lerner's representation of this ratio is:

$$\text{Satisfaction} = \frac{\text{Achievement}}{\text{Aspiration}}$$

Lerner and Deutsch have attempted to explain the ramification of this response pattern throughout a society by relying on the destructive impact of social processes on pre-existing social structures.[38]

In *Political Order in Changing Societies*, Huntington[39] extrapolated this ratio into a three-tiered representation of political change:

a) Social Frustration

$$= \frac{\text{Social Mobilization}}{\text{Economic Development}}$$

b) Political Participation

$$= \frac{\text{Social Frustration}}{\text{Mobility Opportunities}}$$

c) Political Instability

$$= \frac{\text{Political Participation}}{\text{Political Institutionalization}}$$

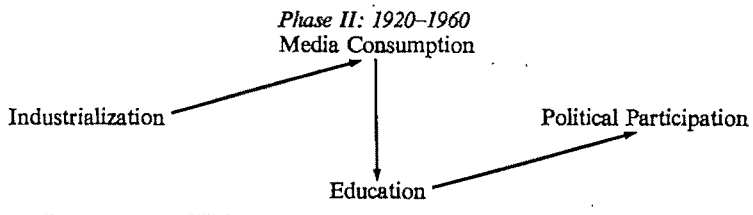
This presentation is an appealing description of what seems to occur within each of the three realms. The first two ratios are clearly an adequate alternative representation of the model identified in this research. As an explanation, however, this system fails on two counts: (1) it is merely asserted that individuals will be frustrated without any attempt to account for the mechanism by which they are actually frustrated; and (2) no ex-

[37] Daniel Lerner, "Toward a Communication Theory of Modernization: A Set of Considerations," in *Communications and Political Development*, ed. Lucian W. Pye (Princeton: Princeton University Press, 1963), p. 333.

[38] Lerner, "The Passing of Traditional Society," pp. 43–75; "Communications Systems and Social Systems: A Statistical Exploration in History and Policy," *Behavioral Science*, 2 (October, 1957), 266–75; and Deutsch, "Social Mobilization and Political Development."

[39] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), p. 55.

Table 15. Model and Analysis of Total Data Point Correlations: 1920–1960



Analysis
Phase II: 1920–1960

		Actual − Expected = Difference
IE.M	= IM × ME .7355 × .8330	.5701 − .6126 = −.04
MP.E	= ME × EP .8330 × .6729	.6141 − .5605 = .05
IP.ME	= IM × ME × EP .7355 × .8330 × .6729	.3384 − .4122 = −.07

planation is given for the transformation of change from one level to the next. Both Lerner and Huntington are attempting reductive explanations of change, even though their primary attention is directed toward social processes. Explanation of social phenomena by reference to psychological characteristics requires a link between the two levels of explanation. Lerner's and Huntington's explanations are not compelling because such a link is never established. In short, the criticism that political theories of change lack composition laws still stands.[40] An adequate reductive explanation of political change can be advanced only if the original conception of the psychological attributes of the individual contains the seeds of the explanation of the extension into social process.

Both Pye [41] and Hagen[42] have attempted to deal with change and responses to change as they originate in a particular psychosocial process—identity definition. Neither has produced theories which are reliable descriptions of the process of social change. In general, any psychological explanation which predicts a particular behavior pattern or a particular personality type is open to question.[43] Similar personality types may manifest different behaviors in response to the same situation, and as a developmental process not all individuals subjected to the same environment develop the same personality type. For instance, Holt and Turner point out that Pye's analysis, as applied to Germany, results in diametrically opposite conclusions to those derived by Pye in the Burmese case.[44]

Hagen bases his theory of change on the concept of status deprivation, and predicts that over time this will result in the development of an "innovative" personality.[45] In essence, Hagen's argument reduces to one category of Merton's paradigm of alienation. Status deprivation produces an imbalance in the ratio of goals/means, but in Merton's paradigm, innovation is only one possible response.[46]

As an explanation of social change, the most serious failure of Hagen's argument lies in its reliance on the occurrence of a particular event, status deprivation, as the causal basis for his explanation. Although he treats the development of an innovative personality as a *process*, his explanation in its entirety is not process oriented. This lack of process orientation forces Hagen to argue that economic modernization results from the existence of a particular group of individuals, whose forefathers, as a group, were subjected to severe status deprivation. He is not able to provide any explanation of the manner in which the existence of this group ramifies through the entire society to produce a self-sustaining process of change. These two examples indicate that an explanation of social change which relies on the development of particular personality types is likely to be extremely limited.

Emphasis on a personality type undoubtedly flows from the same orientation which produced the "great dichotomy."[47] If tradition and modernity are distinctly different, then it is not unreasonable to expect basic differences in "traditional" and "modern" men. Recent criticism of this position has, in fact, cast doubt on the argument that the nature of change is subject to some fundamental dichotomization.[48] It is equally plausible to argue that there is no difference, and that the process of social change is inextricably linked to the *process* of individual identity definition. Process is emphasized to reiterate the concern with the manner in which individuals develop personal identities, not with the particular identities which develop.[49] What follows is an attempt at a partial explanation of that linkage, using a very limited number of factors.

We may begin by examining Erikson's definition of the process of identity formation.

> . . . in psychological terms, identity formation employs a process of simultaneous reflection and observation, a process taking place on all levels of mental functioning, by which the individual judges himself in the light of what he perceives to be the way in which others judge him in comparison to themselves and to a typology significant to them; while he judges their way of judging him in the light of how he perceives himself

[40] Robert T. Holt and John E. Turner, *The Political Basis of Economic Development: An Exploration in Comparative Political Analysis* (Princeton: D. Van Nostrand Company, 1966).

[41] Lucian W. Pye, *Politics, Personality and Nation-Building: Burma's Search for Identity* (New Haven: Yale University Press, 1962).

[42] Hagen, *On the Theory of Social Change*, pp. 123–160.

[43] Holt and Turner, p. 31.

[44] *Ibid.*, p. 30.

[45] Hagen, *On the Theory of Social Change*, pp. 141–143.

[46] Merton, pp. 134–140.

[47] Huntington. "The Change to Change," pp. 287–298.

[48] Reinhard Bendix, "Tradition and Modernity Reconsidered" *Comparative Studies in Society and History*, 9 (April, 1967), 313–14. See also Lloyd I. Rudolph and Susanne Hoeber Rudolph, *The Modernity of Tradition: Political Development in India* (Chicago: The University of Chicago Press, 1967), pp. 3–14.

[49] It may be useful to make a distinction between rational anxiety, uncertainty and concern produced by a rapidly changing social situation, and irrational anxiety, uncertainty and concern produced by individual misperception of cues understood by the majority of individuals. Our concern focuses on the effect of rational anxiety on the identity definition process, as this anxiety results from and is reflected in rapid social change.

in comparison to them and to the types that have become relevant to him.[50]

There are two significant aspects of Erikson's definition; the interactive process of mutual judgment, and the typology which establishes the context of that judgment. Mutual judgment implies a mutual conception of the relevant typologies employed, and at least to some extent, mutuality of the typologies themselves. The more alien the typology employed by a "significant other," the more likely judgment is to proceed in terms of our own typology.

In Erikson's argument, this typology includes a number of attributes, perhaps the most significant being the "cultural consolidation" developed in response to the dominant techniques of the society.

> In every technology and every historical period there are types of individuals who (properly brought up) can combine the dominant techniques with their identity development, and *become* what they *do*. Independently of minor superiorities or inferiorities, they can settle on that *cultural consolidation* which secures them what joint verification and what transitory salvation lies in doing things together and in doing them right . . . . The point is that only such consolidation offers the coordinates for the range of a period's identity formations . . .[51]

The psychoanalytic concern with the development of the self, the individual's sense of "who he is," is inextricably linked to a broader conception of "what he is." For the individual, who he is, and what he is, are in the eyes of his judges, largely inseparable.[52] Identity formation proceeds as an internalization of the judgment of "what he is" in the eyes of others. To combine psychoanalytic and sociological terminology, the individual's conception of self is bounded by the range of life-chances available to him.

The process of social mobilization is often described in terms of the roles available to the individual, and the destruction of old roles and their normative supports.[53] A general theoretical difficulty with social mobilization as a concept is the consistent implication that there is a movement "from" traditional "to" modern behavior patterns. In terms of our concern, the implication is that a more "modern," and relatively stable, identity is waiting for the mobilized individual.[54] It is also agreed, however, that the possibilities for individual identity definition are alterable by virtue of changes in the range of life chances available to him. A not so obvious corollary is that an increase in the rate of change in the spectrum of life chances available will result in a change in the very nature of the process of identity definition.[55] What I am suggesting is that the possibilities for a "cultural consolidation" decrease as the rate of change in the spectrum of life-chances accelerates.

The existence of a typology, or cultural consolidation, would give the individual a certain independence in the process of identity formation. Since the individual is not only judging himself as others judge him, but also judging their judgment in terms of the typology, he must accept or reject their view of him depending upon their comparative standing in terms of the typology.[56]

In a rapidly industrializing society, however, economic development results in a hasty multiplication and differentiation of available roles. As the importance of new roles increases, the utility and meaning of older roles disappears. With the erosion of these older roles, the individual loses any temporally extended anchor for personal identity. In the absence of past referents, identity definition more and more entails comparison of present-oriented symbols and signs, without the filtering effect provided by reference to a previously internalized cultural consolidation. As the rate of change in the number and range of life-chances increases, the ability of the individual to discriminate among the judgments of others decreases, because of a lack of standards by which to judge the judges.[57]

In short, rather than presaging the existence of a "modern" cultural consolidation, social mobilization actually renders the possibility of such a consolidation nil. Instead, the value of external symbols increases. Unable to judge in terms of an agreed typology, individuals are compelled more and more to judge in terms of more easily visible signs of status, such as place of residence, group affiliations and forms of leisure. As the signifi-

[50] Erik H. Erikson, *Identity: Youth and Crisis* (New York: Norton, 1968), p. 22.

[51] *Ibid.*, pp. 31–32.

[52] Talcott Parsons has suggested that Freud's later work also demonstrated increasing concern with social stress on the development of the ego. See Parsons, "Social Structure and the Development of Personality: Freud's Contribution to the Integration of Psychology and Sociology," *Psychiatry*, 21 (1958), 321–340.

[53] Deutsch, "Social Mobilization and Political Development," p. 496.

[54] *Ibid.*, pp. 496–498; Lerner, *The Passing of Traditional Society;* Inkeles, "Participant Citizenship."

[55] Dahrendorf has argued that the changing nature of identity referents suggested by Riesman was actually argued much earlier by Tocqueville; see "Democracy without Liberty: An Essay on Other-Directed Man," in *Culture and Social Character: The Work of David Riesman Reviewed,* ed. Seymour Martin Lipset and Leo Lowenthal (New York: Free Press, 1961), pp. 175–206.

[56] Erikson, pp. 22–24.

[57] Klapp (*Collective Search for Identity*, pp. 73–116) pursues this analysis in terms of not only physical symbols but also patterns of leisure, movement membership, etc.

cance of the earlier cultural consolidation recedes, the identity definition process is increasingly externalized, precipitating the individual into increasing competition. He is compelled to compete for the external symbols because the external symbols now constitute the basis for discriminating the judgments of others, and the basis for comparison of self to others.[58]

What occurs then may be understood as a transformation of meaning. Under a pre-existing cultural consolidation, certain symbols were understood to attach to particular roles. As the previous cultural consolidation melts under the pressure of accelerating economic change, the meaning system relating status and symbols is reversed. Instead of status implying symbols, symbols come to imply status. Possession of the appropriate symbols becomes the most significant aspect of the quest for identity.[59]

Obviously, the meaning system of any society may be drastically altered by many forms of social disruption such as war or trade expansion. As sources of social change, however, war, conquest, or trade expansion must at some point cease, and at that time a new cultural consolidation is possible.[60] Industrialization, on the other hand, is not only a source of social change, it is also a continuing productive process which reinforces itself. The changes produced by industrialization render the old cultural consolidation obsolete, and identity formation becomes externalized. Individual identity definition by reference to external symbols increases, and possession of the symbols is most easily achieved by involvement in the productive process. Involvement of greater numbers of individuals in the process further decreases the utility of the previous cultural consolidation for those individuals not yet involved, and the cycle continues. Not only does the dominant mode of identity definition become externalized, it also becomes constantly shifting, as the basis of comparison, the agreed valuation of symbols, constantly shifts. As a technological-economic process, industrialism produces a steady flow of new roles and new symbols, increasing the tentativeness of the individual's identity.[61]

Since the meaning imputed to a particular set of symbols is no longer derived from an accepted cultural consolidation, the shared meaning must be based on current agreement, and is open to change simply because some portion of the populace chooses to disagree. Social sanctions which would have been available to penalize a deviant individual or group under the old cultural consolidation are no longer useful in an effort to compel agreement. This uncertainty of valuation also increases the uncertainty of the individual's identity definition, since he is confronted not only with a flow of new roles and symbols, but also with unreliable valuations of older roles and symbols.[62] This tentative quality of individual identity has, in turn, both personal and social consequences.

Our earlier discussion of the various ratios detailing social tension may be reviewed with a different emphasis. In effect, the ratios in the form:

$$\text{Alienation} = \frac{\text{Institutionalized means}}{\text{Cultural Goals}}$$

may be comprehended as partial descriptions of the stratification system. For our purpose, the most significant aspects of a stratification system are the distribution of valued goods, the "proper" form of social mobility, and the normative justification for that definition of propriety. In a strictly economic sense, the process of industrialization operates directly on two elements of the stratification system: the extant distribution of life chances, and the form of social mobility. Increasing economic productivity offers alternatives to each of these elements. As the number of alternatives increases, changes in the normative supports are necessary. An increasing rate of change in the number of alternative life chances however, results in an increasing lag in the relationship between the extant distribution and its normative justification.

A norm is a synthetic link between act and consequence.[63] Formation of a synthesis by custom becomes more difficult as the rate of expansion of available acts increases. Custom implies shared experience, sentiment, and meaning, but the rate of change in the number and kind of available roles makes such sharing impossible. In this context the significance of media development appears more clearly. Media growth is significant not only for its functional utility in the

[58] Riesman pursues a similar argument in *The Lonely Crowd,* pp. 11–36.

[59] The changing use of symbols had been paralleled by changes in the myth system underlying the symbols; see Richard Weiss, *The American Myth of Success: From Horatio Alger to Norman Vincent Peale* (New York: Basic Books, 1969).

[60] The ultimate cessation of other sources of change was the explicit reason behind Hagen's postulation of a personality type—the innovative individual. Hagen, *Theory of Social Change,* pp. 12–16.

[61] In our literature the problem is perhaps best illustrated by Babbitt's encounter with a cigar-lighter as a new means of self-definition. Sinclair Lewis, *Babbitt* (New York: Harcourt, Brace, 1922), pp. 52–57.

[62] Such uncertainty is one of the major results of competitive consumption. Veblen, *The Theory of the Leisure Class;* and Lenski, *Power and Privilege.*

[63] Emile Durkheim's formulation of a norm most clearly illustrates the process of translation from social to political legitimation. See *Sociology and Philosophy,* D. F. Pocock, trans. (Glencoe, Illinois: The Free Press, 1953), pp. 40–46.

transmission of "facts," but for the generalization of meaning. Mass media enable widespread sharing of the meaning to be attached to new symbols and roles.

The identity definition process creates pressure for some justification for the distribution of newly available symbols, but the process of norm formation is not able to respond with the necessary rapidity. Hence there is an externalization of the process of sanctioning the newly developing distribution from a customary synthesis to a legal synthesis.[64] In short, the legitimation of a distribution of newly available life chances is impossible by customary means, and the process of legitimation is politicized.

In this environment education serves two distinct functions; it enables the individual to compete for possession of significant symbols within the new productive process, and it predisposes the individual to a belief that he is competent—at least to a minimal degree—to influence the political system. The motive force for participation comes from a need to protect new distributions of valued goods, or to acquire access and protect new modes of acquiring valued goods.

I am, of course, not arguing that individuals perceive the political process as a means of protecting their modes of identity definition. Anxiety over identity as it inheres in the possession of status symbols and mobility opportunities however, is the primary motivation for intense politicization of conflict over possession of those symbols and opportunities, and this anxiety persists long after there need be any objective concern for the means of subsistence. It therefore makes sense to assert, as does Inkeles, that participant citizenship—political mobilization—is not just a characteristic of democracy, but is, in fact, compelled by the process of "modernization" itself.[65]

[64] Max Weber, *From Max Weber: Essays in Sociology*, ed. H. H. Gerth and C. W. Mills (New York: Oxford University Press, 1946), pp. 224–229.

Conclusion

In his massive review of the state of political change theory, Huntington suggested that a major gap in that effort was the lack of any model for the process of change in an industrialized, western nation. This research has provided a first attempt to fill that gap.

A model of the process of mobilization in the United States is perhaps most significant because it is, in a sense, unique.[66] In a society lacking any traditionally legitimate stratification system, we may observe the effect of the process of economic change in its rawest form. It is therefore unlikely that many other societies will follow the same path as the United States, but therein lies the utility of using the United States as a model. If this is, in fact, the impact of economic development in a society lacking strong traditional structures, we may attempt to develop adequate models of change in other societies by understanding them as a conjunction of a general process and particular structures. This research has, in a limited way, attempted to clarify the nature of that general process.

[65] Inkeles, "Participant Citizenship," pp. 1122–1123.

[66] Louis Hartz, *The Liberal Tradition in America: An Interpretation of American Political Thought Since the Revolution* (New York: Harcourt, Brace, 1955); Alexis De Tocqueville, *Democracy in America* (New York: Edward Walker, 1847).

Institutionalization, the Ombudsman, and Bureaucracy*

LARRY B. HILL
University of Oklahoma

An Authority-Based Conception of Institutionalization

This article focuses upon one principal aspect of the process of institutionalization, upon the political structure's relationship with its environment. The other major thrust of the institutionalization literature is upon internal structural integration. Nelson Polsby exemplifies this thrust in his study of the institutionalization of the House of Representatives in which he states: "Over the life span of this institution, it has become perceptibly more bounded, more complex, and more universalistic and automatic in its internal decision making."[1] Additionally, of Samuel Huntington's four criteria of institutionalization, two are internal: complexity and coherence.[2] Such measures of organizations' internal structures and capabilities clearly are relevant, particularly since large, intrinsically important structures including legislatures, political parties, bureaucracies, and armies are the usual bodies that are analyzed. These two criteria, however, constitute only a limited perspective for the analysis of the process of institutionalization. Organization-environmental relationships are the focus of Huntington's two other criteria: adaptability (a function of chronological age, generational leadership succession, and ability to take on new functions while discarding obsolete ones) and autonomy ("the development of political organizations and procedures that are not simply expressions of the interests of particular social groups"[3]). If when in addition to internal complexity and coherence, organizations attain high degrees of adaptability and autonomy in the face of environmental challenges, then they are said to be institutionalized.

Implicit in these criteria of institutionalization are various definitions of the phenomenon. The most influential and common intradisciplinary definition is sociologist Philip Selznick's statement that to institutionalize is "to *infuse with value* beyond the technical requirements of the task at hand."[4] Among those studying institutionalization, political scientists are most likely to know Huntington's definition: "Institutionalization is the process by which organizations acquire value and stability."[5] Notions of institutionalization based upon stability and value obviously are useful for many purposes. They are, however, insufficiently political in conception for this paper.

I view the organization (the incipient institution) as a behaving entity or actor in an environment populated with other potentially competitive actors.[6] Not only must an organization *defend* itself against its environment (Huntington's adaptability and autonomy), but also it must have an *offensive* capability. It must carry out its mission, a programmatic goal more demanding than mere survival, as well as having an impact upon that environment.[7] *Institutionalization is a process*

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[1] Nelson W. Polsby, "The Institutionalization of the U. S. House of Representatives," *American Political Science Review*, 62 (March, 1968), 145.

[2] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1969), p. 12.

[3] *Ibid.*, p. 20. It should be noted that Polsby's first criterion, well-boundedness or environmental differentiation, is not strictly internal; it is related to Huntington's environmental concept of autonomy.

[4] Philip Selznick, *Leadership in Administration: A Sociological Interpretation* (New York: Harper and Row, 1957), p. 17; Selznick's italics. On page 40 Selznick elaborates and indicates that organizations which are infused with value become "prized not as tools alone but as sources of direct personal gratification and vehicles of group integrity."

[5] Huntington, *Political Order*, p. 12; he cites Selznick at p. 15.

[6] As S. N. Eisenstadt has observed (*Essays on Comparative Institutions*, New York: John Wiley, 1965, p. 187): "From the very beginning, any bureaucratic organization is put in what may be called a power situation in relation to its environment and has to generate processes of power on its own behalf."

[7] This perspective is generally consistent with the "institution building" strand of literature, whose major theoretician has been Milton Esman. *Influence*, the extent to which the organization affects decisions in its functional area and its ability to enlarge its sphere of action, and *impact* or spread-effect, meaning the degree to which its values and actions be-

that occurs over time in which the organization creates authority relationships vis-à-vis the environmental actors. That is, the organization attains a level of internal structural integration sufficient for the performance of its other functions; it manages to cope with any depredations from the environment; and through the exercise of whatever resources of power and influence are available, it succeeds in establishing itself as an authority figure.[8]

This is not to imply that the successful organization, the institution, exercises anything like sovereignty over its environment. Minimally, it does mean that the legitimacy of its existence is acknowledged. Furthermore, the institution has created the expectation among the environment's members that its actions and demands will fall within the boundaries of a shared normative framework, so that predispositions toward at least negotiated compliance are established. Institutionalization, thus defined, can be measured in various ways. The approach taken here is to examine the behavioral interactions, linkages, or exchanges,[9] between an organization—in this case the New Zealand ombudsman—and the actors in its environment—primarily the bureaucratic actors. From those indicators, inferences are drawn concerning the strength of authority relationships and, hence, of political institutionalization.

The Ombudsman: Candidate for Institutionalization

Both internationally and in the United States, attention increasingly focuses upon the originally Swedish institution of ombudsman as a bureaucratic control mechanism.[10] There is even a worldwide ombudsman movement[11] which has reached such proportions that it often has been labeled as ombudsmania. A key to understanding the ombudsman's popularity is its relative simplicity; "grievance-man," "mediator," and "citizens' defender" are commonly used and not inaccurate synonyms. The 1972 edition of the *Encyclopaedia Britannica*, which offers a more structural definition, states that an ombudsman "is a legislative commissioner for the investigation of citizens'

come normative within the environment, are the labels given the two major criteria. Milton J. Esman and Hans C. Blaise, *Institution Building Research: The Guiding Concepts* (Pittsburgh: University of Pittsburgh, Graduate School of Public and International Affairs, 1966), pp. 6–7.

That paper was a product of the Inter-University Research Program in Institution Building (Indiana, Michigan State, Pittsburgh, and Syracuse Universities are the program's members). Most of its papers have been distributed in mimeographed form, but two collections have been published: Joseph W. Eaton, ed., *Institution Building and Development: From Concepts to Application* (Beverly Hills: Sage Publications, 1972); and D. Woods Thomas, Harry R. Potter, William L. Miller, and Adrian F. Aveni, eds., *Institution Building: A Model for Applied Social Change* (Cambridge, Mass.: Schenkman Publishing Co., 1972). Esman's indebtedness to Selznick's conception of institutionalization is acknowledged in "The Elements of Institution Building," in Eaton, p. 28.

[8] The present focus upon the organization's offensive capability parallels Ralph Braibanti's discussion of "permeative dynamism" and the "propelled rediffusion of norms from one institution to others." "External Inducement of Political-Administrative Development: An Institutional Study," in Ralph Braibanti, ed., *Political and Administrative Development* (Durham, N.C.: Duke University Press, 1969), p. 55.

[9] Esman discusses linkages and transactions in "The Elements of Institution Building," pp. 28–34. For an interpretation of institutionalization as an exchange process see Eisenstadt, *Essays on Comparative Institutions*, chapter 1, part 1.

[10] On the national level the ombudsman has been transferred not only throughout Scandinavia and to New Zealand, but also to Great Britain, Guyana, Tanzania, Northern Ireland, Israel, Mauritius, France, and Fiji. New Constitutions in Bangladesh, Greece, Pakistan, and the Philippines provide for the office. Also, West Germany has a military ombudsman, and Canada has a "Commissioner of Official Languages." On the sub-national level, three American states (Hawaii, Nebraska, and Iowa), six Canadian provinces (Alberta, New Brunswick, Quebec, Manitoba, Nova Scotia, and Saskatchewan), and three Australian states (Western Australia, South Australia, and Victoria) have ombudsmen. Such cities as Zurich, Switzerland; Jerusalem and Tel Aviv, Israel; Dayton, Ohio; Seattle, Washington; San Jose, California; and Kansas City, Missouri have municipal ombudsmen. At least one hundred ombudsmen exist on the campuses of American colleges and universities.

Merely keeping track of the plethora of ombudsman proposals has become a major undertaking. The Ombudsman Committee of the American Bar Association, which has taken on that chore, reported in 1973 news about ombudsmen of various types in ninety different non-American jurisdictions, mainly national ones. In addition to several proposals at the American federal level, by 1973 ombudsman bills had been introduced into the legislatures of all but eight of the American states. Bernard Frank, "American Bar Association Section of Administrative Law, Ombudsman Committee Development Report, July 1, 1972–June 30, 1973," Annual Reports of Committees: Section of Administrative Law, American Bar Association Vol. 10 (1973).

[11] See Larry B. Hill, "International Transfer of the Ombudsman," in Richard L. Merritt, ed., *Communication in International Politics* (Urbana: University of Illinois Press, 1972), pp. 295–317. The volume edited by Donald C. Rowat, *The Ombudsman: Citizens' Defender* (Toronto: University of Toronto Press, and London: Allen and Unwin, 1965), has been an important document in the ombudsman movement. Two other such documents are the papers presented in 1967 at Columbia University's American Assembly, which are collected in *Ombudsmen for American Government?* (Englewood Cliffs, N.J.: Prentice-Hall, 1968), ed. Stanley V. Anderson; and the papers in "The Ombudsman or Citizens' Defender: A Modern Institution," ed. by Roy V. Peel, *The Annals*, 377 (May, 1968).

complaints of bureaucratic abuse."[12] In developing a more comprehensive definition, I have isolated several defining characteristics:[13] the classical ombudsman is (1) legally established, (2) functionally autonomous, (3) external to the administration, (4) operationally independent of both the legislature and the executive, (5) specialist, (6) expert, (7) nonpartisan, (8) normatively universalistic, (9) client-centered but not anti-administration, and (10) both popularly accessible and visible. The institution's mission is to generate complaints against government administration, to use its extensive powers of investigation in performing a postdecision administrative audit, to form judgments which criticize or vindicate administrators, and to report publicly its findings and recommendations but not to change administrative decisions. Indeed, one of the institution's most interesting puzzles is its apparent effectiveness despite minimal coercive capabilities.

The extensive popular and (mainly normative) academic interest[14] establishes the ombudsman as an appropriate subject of policy analysis, but little empirical research yet has been undertaken. Such research often has been discouraged by the new ombudsmen's legal secrecy requirements and by their sometimes excessively cautious estimations of the sensitivity of materials in their files. This article is the first empirically based, extended treatment of the inner workings of an ombudsman as political actor.[15] In 1962 the New Zealand official became the first extra-Scandinavian ombudsman, and his success provided the initial evidence that the institution was not dependent upon a uniquely Scandinavian constellation of cultural and political factors. Since then, the New Zealand institution has become the primary model for the world's newer ombudsmen. Because Sir Guy Powles has become the model ombudsman and because of his relative longevity in office, which provides necessary historical perspective, it is appropriate to select this first Anglo-Saxon ombudsman for detailed study. Concluding comparisons also are drawn with other national ombudsmen and with the Hawaiian official.

The New Zealand materials were collected during nineteen months of fieldwork in the ombudsman's office. This article's data were generated from a content analysis of a sample of his cases. The sample was chronologically based and divided into nine time periods. Between September, 1962, and March, 1969, 4,280 complaints were lodged. Eleven per cent of the cases, 450 complaints, constitutes the sample, which tests revealed was highly representative of the universe (see footnote 21).

New Zealand's Parliamentary Commissioner (Ombudsman) Act, which demarcates the organization's formal boundaries and specifically places the major government departments under its jurisdiction, provides a legal foundation for the office's authority.[16] Within these boundaries, the ombudsman is given extensive latitude to carry out investigations of any departmental decision, recommendation, act, or omission "relating to a matter of administration." Most important, he is

[12] Stanley V. Anderson, "Ombudsman," *Encyclopaedia Britannica*, 1972, XVI, 960.

[13] For an explication of the following conception of the ombudsman, see Larry B. Hill, *Ombudsmen, Bureaucracy, and Democracy* (New York: Oxford University Press, forthcoming 1975), chapter 1. See also Donald C. Rowat's rather legalistic listing of the ombudsman's "essential features." "Preface to Second Edition," in *The Ombudsman: Citizens' Defender*, rev. ed., ed. D. C. Rowat (Toronto: University of Toronto Press, 1968), p. xxiv; and Stanley V. Anderson's functionally based "essential characteristics" in *Ombudsman Papers: American Experience and Proposals* (Berkeley: University of California, Institute of Governmental Studies, 1969), pp. 3–4.

[14] A perusal of even the titles of the rapidly expanding ombudsman literature clearly reveals its normative thrust. Counting only the items from scholarly or semi-scholarly sources, Stanley Anderson's 1969 bibliography contains nearly 250 listings, and it now is quite incomplete. See *Ombudsman Papers*, pp. 381–407. The most influential early popularizing article was Henry J. Abraham, "A Peoples' Watchdog Against Abuse of Power," *Public Administration Review* 20 (Summer, 1960), 152–57.

[15] The inaccessibility of files for extended research does not, of course, mean that no valuable work can be done. The ombudsmen themselves provide useful data in their annual reports, and they often have cooperated with scholars by granting interviews and sometimes providing additional statistical data. The classic comparative study remains Walter Gellhorn's *Ombudsmen and Others: Citizens' Protectors in Nine Countries* (Cambridge, Mass.: Harvard University Press, 1966). Britain's Parliamentary Commissioner, who took office after Gellhorn's book was completed, is analyzed in William B. Gwyn, "The British PCA: 'Ombudsman or Ombudsmouse?'," *Journal of Politics* 35 (February, 1973), 45–69.

The University of California's Ombudsman Activities Project, sponsored by the Office of Economic Opportunity (Stanley V. Anderson Principal Investigator), which has provided technical assistance to the new American ombudsmen, is not hampered by such research limitations. See especially John E. Moore's forthcoming monograph on the Hawaiian ombudsman.

[16] Complaints relating to the armed services, those within the jurisdiction of a court or an administrative tribunal, and complaints against the Public Trustee and the Crown Law Office are specifically excluded. Implicitly excluded are complaints against Cabinet ministers' decisions (although departmental recommendations to them are included), complaints against government corporations, and most complaints against local authorities. Inmates of institutions may submit sealed complaints. For overviews of the New Zealand ombudsman see Gellhorn, Chapter 3; and Larry B. Hill, "The New Zealand Ombudsman's Authority System," *Political Science* 20 (September, 1968), 40–51.

guaranteed access to departmental files and records. The Act's specification of the ombudsman's domain appears hyperbolic: he may investigate where the action complained of appears to be illegal, mistaken, unreasonable, unjust, oppressive, discriminatory, or even "wrong." Although the ombudsman cannot change administrative decisions, the moral force behind his recommendations gives him sufficient authority so that potentially he could disrupt the administrative process. Alternatively, through timidity or being co-opted by the administration, the role of New Zealand's ombudsman could atrophy.

In pursuing the topic of the ombudsman's institutionalization through a sociometric analysis of his interaction with bureaucracy over time, this article investigates four questions: How extensive is the ombudsman's interaction with the bureaucracy? How consequential for the government department is the threat posed by complaints? What demands does the ombudsman make upon the agency? How cooperative is the department in responding? Their answers also will illuminate the public policy question of what kind of governmental device an ombudsman actually is.

Extensiveness of Ombudsman-Departmental Interaction

Because a common objection to the ombudsman institution is that it would create a large additional workload for already overburdened agencies, a preliminary step in interpreting the relationship between the ombudsman and the agencies was to measure the frequency and mode of their interaction. After receiving a complaint, the initiative lies with the ombudsman. He can have no impact upon the bureaucracy unless he decides to investigate; thereupon he must make important strategic decisions about how to conduct the investigation and about whom to involve.

Targets of Complaint. Table 1 indicates that more than three-quarters of the ombudsman's complaints processed through the 1973 reporting period were against fourteen major agencies of government. Only five departments (Treasury, External Affairs, Defence, Internal Affairs, and Agriculture) which traditionally are considered major are missing from that list. However important the functions performed by them are, they do not regularly come into contact with large numbers of individual citizens (New Zealand farmers are not subsidized). The major targets of complaint are the primary institutions of New Zealand's highly developed welfare state, the institutions which most often interact with citizens.[17] Although it is impossible to determine how many complaints "should" have been made against particular departments during a decade, the numbers actually lodged seem quite small. The multifarious activities of all of these departments require tens of thousands of yearly contacts with clients of one type or another; each transaction is a potential complaint to the ombudsman.

Incidence of Investigation. According to Table 1, the ombudsman has investigated about three-fifths of the complaints within his jurisdiction. One might speculate that as he gained experience he would investigate fewer and fewer complaints, but according to the data in his reports, this has not occurred. Although there have been some fluctuations in the annual proportions of cases investigated, the deviations are minor and there is no secular trend. When investigation is defined slightly differently as I do in the sample of cases, however, the incidence of investigation *has* fallen somewhat from 72 per cent in the first time period to 56, 50, 58, 32, 38, 44, 46, and for the last period 46. This definition is a behavioral one; if the Ombudsman queried the department about the case, then it was investigated. He sometimes tentatively began an investigation and then discontinued it for various reasons, but nevertheless labelled the case "not investigated." It is because my definition of investigation and the Ombudsman's are not exactly the same that comparisons of the actual complaints with the sample are not made in Table 1. The definitions, however, are quite similar, and 62.8 per cent ($n = 221$) of the sample cases were investigated compared with 60.1 per cent for the actual cases as of 1969. Since interaction with departments took place only on those cases which were investigated, 221 becomes the n for subsequent discussion of the findings from the sample.

As expected, cases against the major complaint departments constitute a predominant proportion of those which the ombudsman investigated. Social Welfare's incidence of complaints investigated is artificially high because it has no separate appeal agency, whereas Inland Revenue's is low due to the Tax Court's displacement of the ombudsman's jurisdiction. Otherwise there are no particular surprises in the departmental percentages of complaints which the ombudsman has deemed worthy of investigation. Certainly, the fact that—excepting Social Welfare's special circumstances—over a decade the ombudsman investigated a total of no more than 340 cases against any one department indicates in a gross

[17] New Zealand's percentage of working age population employed by its government agencies and enterprises is the world's highest. See Bruce Russett, ed., *World Handbook of Political and Social Indicators* (New Haven: Yale University Press, 1964), p. 70.

Table 1. Departmental Targets of Complaint and Investigation: 1 October 1962 to 31 March 1973

Department	Complaints Processed		Complaints Investigated		Target's Per Cent of Processed Complaints Investigated
	n	%[a]	*n*	%[b]	
Total Major Investigative Targets	5,360	78.4	3,234	81.3	60.3
Social Welfare	1,154	16.9	837	21.0	72.5
Education	581	8.5	340	8.5	58.5
Justice	543	7.9	286	7.2	52.7
Inland Revenue	432	6.3	168	4.2	38.9
State Services Commission	342	5.0	242	6.1	68.8
Post Office	313	4.6	188	4.7	60.1
Health	308	4.5	183	4.6	59.4
Police	290	4.2	147	3.7	50.7
Labour	286	4.2	158	4.0	55.2
Customs	279	4.1	174	4.4	62.4
State Advances	262	3.8	169	4.2	64.5
Works	240	3.5	123	3.1	51.3
Railways	171	2.5	107	2.7	62.6
Government Superannuation Board	159	2.3	112	2.8	70.4
Total Other Targets	1,476	21.6	745	18.7	50.5
Total	6,836	100	3,979	100	58.2

Compiled from *Report of the Ombudsman for the Year Ended 31 March 1973* (Wellington: Government Printer, 1973), Appendix B. One hundred sixty-three complaints still under action at the end of the year and 1,746 complaints against agencies outside of the ombudsman's jurisdiction are excluded.

[a] Means target's per cent of total complaints processed.

[b] Means target's per cent of total complaints investigated.

quantitative sense that the burden of his investigations has not been overpowering.

Interaction Through Correspondence. If the ombudsman's number of cases investigated does not appear to be very burdensome even for the major investigative targets, how extensive have the investigations of the individual cases been? Nearly all of his interactions with departments on particular cases have been through correspondence which he initiated. There was a long-range tendency for him to write more than a single letter, which was all he wrote in 21 per cent of the investigations; but the most common situation (36 per cent), one that remained relatively stable over time, was that in which he wrote two letters. The mean was 2.8 letters. In only 13 per cent of the cases were five or more letters sent, so the instances of a heavy input from the ombudsman were few. But such cases (nine or more ombudsman-letters were sent in 3 per cent of the investigations) could be expected to have a considerable impact upon the departments.

Although letters do vary greatly in length and in the effort that goes into them, the fact that in nearly half (48 per cent) of the cases the department wrote only a single letter in response to the ombudsman indicates that agencies may not regard responding to him as onerous. The mean number of departmental replies was 2.1 letters to the ombudsman. Truly extensive correspondence involving five or more letters from the departments was required in only 7 per cent of the investigations, and departmental responses per case exceeded nine only 1 per cent of the time. These facts confirm that departments have not been heavily burdened by the ombudsman's investigations. Also, they support an impression gained from reading the letters that departments have prepared carefully their initial response so as to preclude a request for fuller information. No secular shifts in the number of departmental responses have occurred.

Other Interaction. The ombudsman held personal interviews—all initiated by his office—with departmental officers in just 7 per cent of the cases. Most of them were with permanent heads, and all but one conference was held at the ombudsman's office. In only 13 per cent of the investigations did the ombudsman's office telephone the department. Most calls were by the ombudsman himself, primarily to department heads. There are notes of telephone conversations that were initiated by the department in 6 per cent of the cases; the real figure is probably somewhat higher.

Furthermore, despite the ombudsman's very extensive power to investigate departmental premises, in only 3 per cent of the cases did he exercise it. The circumstances then at issue usually were a construction site or disputed piece of property. Knowing of the ombudsman's legal powers, some administrators have held a fearful image of him as a crusading figure who barges into agencies looking for secret files; this image is incorrect. Although it is clear that the ombudsman's communications with the departments by means other than correspondence have been quantitatively infrequent, this is not to say that they are necessarily unimportant.

Not only was the interaction initiated by the department with the ombudsman limited, but also interaction with other actors in the course of dealing with a complaint was rare. Perhaps surprisingly, governmental officials other than the ombudsman and departments very seldom participated. Cabinet Ministers played some role in the investigation of only 5 per cent of the cases; Members of Parliament were implicated in 8 per cent.[18] Other government departments seldom participated in investigations, and there was no pattern of departments consistently being doubly involved in cases. In fact, four was the most times any given department interacted with any other; in just 4 per cent of the cases were three departments involved; and four departments were concerned in a single investigation only twice.

Pervasiveness of Complaint's Threat

Writers sometimes have provided examples of the kinds of cases which ombudsmen investigate, but these usually rather dramatic stories may be quite unrepresentative of the general run of cases.[19] Determining how great a threat those complaints which the ombudsman decides to investigate pose to agencies is central to an understanding of their experience with him.

Extent of Departmental Discretion. One matter vital to an assessment of the ombudsman-departmental relationship is whether or not the agency had been free to make a contrary decision. It emerged from the analysis that 4 per cent of the complaints were really not against a decision of a government department. An additional 1 per cent pertained to a decision made by a Minister rather than a department (New Zealand does not work very hard at maintaining the British convention that all decisions are made by Ministers). Eight per cent concerned the department's duty to enforce a law or regulation; many statutes allow no discretion—as in the computation of several taxes. Slightly more complaints really were against government policies which the departments were forced to implement. Except as they may have convinced Cabinet to adopt the policies, they cannot be held responsible. An additional 16 per cent fell into the less formal category of departmental procedures or policies, which normally were at least tacitly approved by government. The complaints arose over their content, their justice, etc.; whether or not the complainant met the established criteria was not at issue.

Only about one-third of the complaints investigated concerned matters on which the department had made discretionary decisions. The administration of any welfare state requires that huge numbers of such decisions be made—about matters such as whether Applicant *X* meets the qualification rules for Benefit *Y*—and it is not surprising that they should be the largest single occasion for complaint. Almost another third of the complaints were lumped into a miscellaneous category; often a departmental omission or failure to act was alleged. In perhaps a majority of that category of cases it was not within the province of the department to have acted differently. The finding which emerges from this analysis of the extent of the departments' discretion is that—either because they did not make the decision or for various legal or procedural reasons—in probably no more than *half* of the total complaints investigated could the department reasonably have acted to forestall the complaint. Knowing this, departments would not have been particularly concerned about the other half of the cases.

Depth of Threat. The complaints which the ombudsman forwarded did not often seriously threaten departments; in 74 per cent of the total sample only an individual case was involved. For example, questions about such matters as the interpretation of a citizen's qualification to receive a benefit would be at issue. Even these mundane matters, however, were potentially costly; 53 per cent of these cases made some financial claim upon the department. Broader matters were raised by the remaining 26 per cent; these included issues such as whether or not, according to a proper interpretation of the Act, a particular

[18] Larry B. Hill, *Parliament and the Ombudsman in New Zealand* (Norman: University of Oklahoma, Bureau of Government Research, 1974), pp. 90–103.

[19] For analyses of the complainants and their complaints see Larry B. Hill, "Complaining to the Ombudsman as an Urban Phenomenon: An Analysis of the New Zealand Ombudsman's Clients," *Urban Affairs Quarterly* 8 (September, 1972), 123–27; and Larry B. Hill, "Socio-Psychological Dimensions of Complaints to Ombudsmen: A New Zealand Analysis" (paper presented at the meeting of the American Political Science Association, Chicago, Illinois), September 7–11, 1971.

subclass of people should be entitled to a benefit.[20] Some of these cases raised for the department the specter of being forced to amend an Act or of reallocating resources. Indeed, 58 per cent of these cases involved financial claims. It was obvious from a reading of the departmental responses that such possibilities—especially prospective Treasury raids—evoked feelings of trepidation and even indignation. Although the ombudsman's complaints did not usually threaten departments beyond a single case, they have raised more far-reaching matters often enough to keep agencies acutely aware of his existence.

Locus of Decision Making. Fifty-three per cent of the complaints investigated concerned decisions originally made at the across-the-counter level. Since the ombudsman's complaint is routed to the department head, this meant that it often was necessary for a local branch office of the department to become involved in the internal investigation. Sometimes files were sent to the head office, sometimes reports were sent, and sometimes conferences were held. A dramatic increase has occurred in the complaints made against local office decisions, while those made by the head office have decreased. This has been due to complainants deciding to skip an internal appeal to the head office as the ombudsman's existence as an alternative appeals agent became more widely known.

Despite increased work in communication, the fact that complaints were increasingly against a local office rather than headquarters actually may have tended to reduce the level of ombudsman-departmental conflict, for less threat was involved. It is easier for an agency's chief policymakers to consider reversing a subordinate's decision than one of their own.

Focus of Responsibility. Although the ombudsman sometimes has been depicted as the scourge of civil servants, the complainants normally conceived of the department as a corporate body and focused their ire upon it. Individual employees were accused or challenged for mistakes, misbehavior, etc. in only 7 per cent of the investigations. In none of them was the matter ever carried far enough for the accused civil servant to ask to state his defense to the ombudsman as the Act provides (Section 15 [3]). What usually happened in those cases was that the department pointed out to both the complainant and the ombudsman that what had been interpreted as the capricious action of an individual civil servant was actually an action in pursuance of the law or governmental policy, etc., so that the focus of the complaint was shifted to the department as an impersonal entity. Since the ombudsman's complaints usually are potential threats to the agency as a whole rather than to individual civil servants, agencies must be quite sensitive to them.

[20] In about one-third of those cases the ombudsman added importance to the investigation to the extent of asking the department for an explanation of their policy.

Nature of the Ombudsman's Demands

Of course, it is the ombudsman's own actions which are crucial in this relationship. That he has little coercive ability is clearly established, but he does have discretionary power about how he shall allocate his energies and define his role. Is he, as some have supposed, a modern Don Quixote with his lance ever poised for tilts at bureaucratic windmills?

Amount of Information Provided. In nearly half of the cases the ombudsman began the investigation by writing to the department giving an extensive précis of the complainant's allegations, including long quotations from his letter, and the ombudsman's preliminary legal investigations. This practice has remained stable, but that of sending the department a copy of the client's letter, which was done in about one-third of the cases, has greatly increased. It may be that complainants have come to make fewer contentious—and potentially legally actionable—statements in their letters, so that the ombudsman has felt less hesitation in sending copies to departments; or it may be that he has decided more often than previously that it is sounder strategy or administration to send a copy. The office has no copying machine, so copies have to be individually typed. In the remaining cases the ombudsman forwarded only a bare sketch of the complaint; that procedure virtually has stopped. It was becoming usual for the department complained against to be given nearly as much information about the complaint as the ombudsman possessed. Under such circumstances, departments would have no fear that the ombudsman could be holding out information on the complaint in an attempt to trap them.

Action Demanded. Aside from the ultimate power of recommendations to Parliament, the ombudsman's most important potential threat over government departments is that he may examine any of their files. He exercised this option, however, in only one-fifth of the complaints. The ombudsman asked for the files *alone* in about half of those cases (but sometimes the files were accompanied with an unsolicited report). These were usually cases in which he was only interested

in determining a factual matter that could be found easily in the file, and the practice usually should be interpreted as an attempt to relieve the department of the burden of preparing a report on an obvious matter. In the other half of the cases in which the ombudsman demanded the department's files he also required them to submit a report. Only twice did he originally ask for a report and then subsequently also ask for the files.

In more than three-quarters of the cases investigated the ombudsman was satisfied merely to demand a report from the department concerned. (Even though they were not requested to do so, in 4 per cent of the cases the departments sent the relevant files in addition.) Thus, he generally has been satisfied with the quality of the reports received. It could have been predicted that in a first flush of crusading zeal the ombudsman might often have demanded to see the files, and that as he gained confidence in the departments' dependability, he would then tend to rely more heavily on their reports. This has not been the case, and the only trend discernible over time is that a miscellaneous category composed of situations in which interviews and telephone calls were requested, has vanished.

Ombudsman's Impact. At the conclusion of his investigation the ombudsman formally labels cases as indicated in Table 2.[21] Although three-quarters of the sample's complaints which were designated "not justified" and "discontinued" were investigated, the ombudsman's chief impact upon the departments was in those cases listed as "rectified" and "recommendation made." Adding those two categories together (10.8 per cent plus 5.0 per cent) and using the ombudsman's terminology, we find that almost 16 per cent of the sample cases investigated were "justified." Analysis revealed no important changes over time, aside from a trend for somewhat more complaints to be "withdrawn." Even among the complaints designated as "rectified," in only one-fourth of the cases did the department change a decision because of overt pressure from the ombudsman. In those cases the department bowed not to his moral suasion but to his political demands. The making of a recommendation is the ombudsman's most potent sanction. It is not surprising that, as indicated in the Table, it is seldom used and then only as a last resort. Thus, the ombudsman's quantitative coercive impact upon departments has been relatively small.

[21] Table 2, which deals with a most important variable, offers an opportunity for a statistical test of the sample (the sample figures are best compared with those for 1968 in parenthesis in the Table). When the frequencies for each classification which were observed in the sample are compared with the frequencies expected—because they represent the actual classifications—and the standard one-sample (or $1 \times n$) chi-square was applied, it was determined that the differences were not significant. According to the .05 criterion, the sample is representative of the universe. (one-sample $X^2 = 3.98$, d.f. $= 5$, $p. > .50$).

Departmental Reaction

The nature of the ombudsman institution makes it more probable that government departments will be acted upon than initiate action. This does not, however, mean that they are passive. The bureaucracy might use "sandbagging" or other obstructive strategies to increase the difficulty of the ombudsman's job; thus, an analysis of the department's reactions to the ombudsman's investigations is essential to an understanding of their relationship.

Speed of Departmental Reply. Departments generally have accorded the ombudsman's complaints high priority. *On almost one-third of them the ombudsman received a substantive reply within one week.* (Interim replies merely acknowledging receipt of the complaint were not counted.) Nearly as many more were received in the second week, and less than one-tenth took more than a month. Taking into account the large volume of paper work that government departments must process, the common occurrence of crises, the fact that reports from subsidiary offices were so often required, and the complex nature of many complaints, the replies seem quite prompt. Also, there is a trend for complaints to be dealt with more quickly over time. As further evidence of the alacrity of departmental response, in only 7 per cent of the complaints did the ombudsman send a prompter to the department; and in only one case was it necessary to send a second prompter.

Contested Jurisdiction. There is a high rate of agreement between the ombudsman and the departments upon his jurisdictional boundaries. In only three cases did the department contest his jurisdiction. In two of them the department convinced the ombudsman that he had no jurisdiction, and in the other he convinced them that he did. But in none of them did the department carry its disagreement to the Crown Law Office for a legal opinion to buttress its claim as the Act provides that it may. Measured by this important criterion, the relations between the ombudsman and departments generally have been amicable. Another obstacle that departments could raise would be an attempt to hide behind their Minister's authority. In only one case in the sample, however, did they involve their Minister in a case

Table 2. Classification of Ombudsman's Decisions: 1 October 1962 to 31 March 1973

Classification	Per Cent of Actual Complaints		Per Cent of Complaints in Sample	Per Cent of Sample Complaints Investigated
	$n=8,582$[a]	$(n=4,414)$[b]	$n=450$	$n=221$
Not Justified	36.6	(37.5)	38.7	61.1
Declined	32.9	(36.3)	34.4	5.6
Discontinued	12.9	(8.8)	8.4	13.1
Withdrawn	7.8	(8.5)	10.7	4.3
Rectified	7.6	(6.4)	5.3	10.8
Recommendation Made	1.5	(2.3)	2.4	5.0
Justified—No Recommendation[c]	.6	(—)	—	—
Total	100	(100)	100	100

Compiled from *Report of the Ombudsman For the Year Ended 31 March 1973* (Wellington: Government Printer, 1973), Appendices B and C.

[a] One hundred sixty-three complaints still under investigation at the end of the year and 1,746 complaints outside of the ombudsman's jurisdiction are excluded from this total.

[b] This column reports cumulative figures as of the 1969 *Report*, which is the reporting period closest to the one covered in the sample.

[c] Classification used for the first time in 1967.

when neither the ombudsman nor the complainant previously had done so. This is further evidence that the departments did not view the ombudsman as a dangerous enemy who should be opposed at every turn.

Disputed Facts. An important insight into the departments' perceptions of the ombudsman's complainants can be gained from another finding. In only 19 per cent of the investigations did they dispute a fact given to the ombudsman by the complainant. It is interesting that essential facts were at issue in half of the cases in the first time period, so that the initial conclusions of departments about the ombudsman's complainants were that they were often not truthful. Of course, it might be that the department was wrong and that it was defending its position by attacking the complainant's integrity. Its contentions usually were substantiated, however, with documentary evidence that convinced the ombudsman and the coder. Some of the facts in contention were easily verifiable, such as a date of application, but others were less so, such as a verbal promise from an officer. Thus, in 80 per cent of the cases, disputes between the department and the ombudsman were on grounds other than the facts of the complaint.

Rectification. As alluded to above, a common departmental reaction to the ombudsman's investigation was to rectify the complaint. The analysis revealed that in more than one-third of the cases which the ombudsman categorized as "discontinued" the department was already in the process of correcting the matter; the ombudsman felt his role was insufficient to warrant classifying the case as "rectified." Usually departments would have corrected a mistake, etc. had it been brought directly to their attention.

Turning to the 10.8 per cent of the sample's investigated cases categorized as "rectified," at the time of complaint the department itself was in various stages of rectifying more than one-fourth of them. In an additional half the department readily righted the matter when informed of it by the ombudsman. Thus, reversals stimulated by the ombudsman's insistence accounted for only about a quarter of the "rectified" cases.[22]

Evaluation and Comparisons

Although organizations that meet specified criteria are given the label of institution, I recommend with S. N. Eisenstadt that rather than conceiving of the phenomenon as an end point we should "talk about the *process of institutionalization* and look on it as a process of continuous crystallization of different types of norms and frameworks."[23] As Huntington has reminded us, political structures decay perhaps more commonly than they develop.[24] Because political organizations are vulnerable to so many environmental and internal vicissitudes, we cannot with confidence examine Organization X or Y at Time Z and

[22] An important recent development is that increasing numbers of complaints are "rectified." Whereas in earlier years only 60 to 80 per cent of the total "justified" complaints were "rectified," the figure has been over 90 per cent since 1971.

[23] Eisenstadt, *Essays on Comparative Institutions*, p. 32.

[24] Samuel P. Huntington, "Political Development and Political Decay," *World Politics* 17 (April, 1965), 386–430.

certify it as truly institutionalized in the sense that it is vulcanized against further change. Nevertheless, this review of the New Zealand ombudsman's interaction with the bureaucratic actors in his environment elicits the *provisional* judgment that according to the theoretical criteria specified above, the ombudsman has become an institution. That is, not only has the ombudsman developed levels of internal structural integrity and environmental coping mechanisms adequate to the challenge,[25] but also he performs his mission well and has established himself with the environmental actors as an authority figure. A review of our findings on the policy questions posed in the introduction will assist in evaluating the New Zealand ombudsman's institutionalization. Included with this review are some brief comparative observations on the Swedish, Finnish, Danish, Norwegian, British, and Hawaiian ombudsmen.[26]

First, the ombudsman is on target, but his interaction with the bureaucracy is not highly extensive. In keeping with the aims of many advocates, ombudsmen have become institutions for the resolution of citizens' quarrels with welfare state agencies. Even the original Swedish ombudsman (an institution that was in 1970 made tripartite) now has the resolution of such conflicts as his principal role, whereas formerly he was preoccupied with the judicial system. The volume of complaints which ombudsmen investigate does not create a consequential burden for administrative agencies; few departments would receive more than one complaint per month. The New Zealand finding that almost all interaction took place through correspondence is, however, not universal. A large proportion of the Hawaiian ombudsman's investigations utilize the telephone; feeling that letter inquiries generate "paperwork," some agencies have requested this form of interaction whenever possible. The Swedish and Norwegian ombudsmen also use the telephone much more often than their New Zealand counterpart does. Conversely, Britain's Parliamentary Commissioner very often sends a staff member to the accused agency for conferences and examination of documents. These differences are stylistic; in no case are departments inundated by work from the ombudsmen. Many bureaucratic officials may be aware of the ombudsman's legal authority, but he cannot build an actual authority relationship without interaction with them. On the other hand, the decision to involve a department in the investigation of a case must be considered carefully, for the ombudsman's credibility is at stake in each investigation.

Second, the finding that the New Zealand complaints seldom were fundamentally threatening to the department is common. Differences in legal systems and in the ombudsmen's jurisdictions make precise comparisons difficult, but it appears to be general that the bureaucracy would have had the option of deciding in favor of the citizen in only about half of the cases investigated. Also, complaints seldom have repercussions beyond a single, individual case; but the focus of complaints is almost always the department and not an individual civil servant. This is less so in Sweden where tightly drafted laws hold civil servants strictly accountable for administrative actions. The finding that complaints from the ombudsman are unlikely to pose a serious threat is positive for his authority, because a perception of threat would be likely to produce attempts at cover up rather than cooperation.

Third, ombudsmen invariably have been punctilious to avoid the disruption of the administrative process; they have treaded softly and usually have fully informed departments about details of the complaint. The Danish ombudsman normally sends the actual letter of complaint—without making a copy—and has experienced no adverse effects. The Hawaiian official even enjoys that ubiquitous American administrative accoutrement, the Xerox machine. It is not unusual for the Danish ombudsman to request the departmental files, and the other ombudsmen seem to do so more often than in New Zealand; especially in the beginning, the Hawaiian ombudsman frequently asked for the documents. But ombudsmen have typically found that they can depend upon the administrators to such an extent that their option of calling for documents seldom is exercised. It is especially difficult to compare the ombudsmen's disposition of cases, but their ability to obtain a favorable outcome for their client in about one-tenth of the cases (but in a somewhat higher per cent of the complaints investigated) is roughly comparable. Reflecting his traditional status and power to prosecute, the Swedish ombudsman is the most likely to use coercion; whereas the Finnish official—who is legally almost as powerful—seems least likely to do so. Coercion normally is avoided. The ombudsman has little real power, and his judicious use of the limited resource has been important to the office's institutionalization.

Fourth, bureaucrats have not obstructed the ombudsman. It is clear that the New Zealand official gets service that is usually twice as fast as

[25] These matters are treated in Hill, "The International Transfer of Political Institutions," chapter VII.

[26] The data on the European ombudsmen were gathered during the summer of 1971, and the Hawaiian materials were obtained the following summer. These impressionistic observations are elaborated upon in Hill, *Ombudsmen, Bureaucracy, and Democracy.*

his brethren (Finland's is the slowest; there it may take from two to three months for a reply), but cooperation is the norm. Even though an ombudsman's jurisdiction often is debatable, it is almost never contested. Denmark's is the clearest instance of bureaucratic tolerance, for the ombudsman has no legal jurisdiction unless internal appeals through the Minister have been exhausted; practically, the ombudsman simply forwards such complaints and they are almost always dealt with as if they were actionable. Willing rectification of clear mistakes as in New Zealand is also internationally normative; such cooperation is an important index of the ombudsman's institutionalization.

In comparing the various ombudsmen one is struck by how similar they are, but their environments are also similar.[27] Often the ironic comment is made that ombudsmen exist where they are least needed, and it is true that standards of public administration are quite high in all of these jurisdictions. One might even say that they enjoy an administrative civic culture. We understand the institutionalization phenomenon only imperfectly, but it might be hypothesized that the classical ombudsman could not succeed in an environment which lacked this infrastructure. Certainly, one might suspect that a corrupt bureaucracy would be unlikely to cooperate fully in its own arraignment.

Probably the congruence between the norms of the New Zealand political-administrative environment and the ombudsman's putative norms goes far to explain a salient finding: there were few changes over time in ombudsman-environmental relationships. Perhaps the most important authority enhancing development is the departments' increasing willingness to rectify complaints so that the ombudsman does not have to make formal recommendations. Although the relationships have not changed in critical ways, time may yet be an important variable. The sharing of stable interaction patterns, which seem to have been reasonably mutually satisfactory, probably has tended to create expectations of further similar experiences. The creation of such expectations enhances the ombudsman's authority.[28]

It may appear from the foregoing empirical analysis that the ombudsman accomplishes little. Certainly, he has not had a profound impact upon the workings of public administration. This may dishearten advocates, who have envisioned him as Don Quixote on a charging steed. But critics may be mollified; the ombudsman has not tilted at windmills, nor, to mix metaphors, has he thrown sand in the administrative gears. His accomplishments are not flashy, but in addition to his substantive and psychological public impact, he has affected administration in subtle ways. The ombudsman is not an administrative panacea; it is probable that those who will be most disappointed with ombudsman experiments are those who expect too much.[29]

[27] See Hill, "International Transfer of the Ombudsman," pp. 309–312.

[28] See Larry B. Hill, "Affect and Interaction in an Ambiguous Authority Relationship: New Zealand's Bureaucrats and the Ombudsman." *Journal of Comparative Administration* 4 (May, 1972), 35–58; and Kent M. Weeks, "Public Servants in the New Zealand Ombudsman System," *Public Administration Review* 20 (November/December, 1969), 633–638.

[29] Charles S. Ascher's caveat is well taken; "The Grievance Man or Ombudsmania?" *Public Administration Review*, 27 (June, 1967), 175.

Hegel's Critique of Liberal Theories of Rights*

PETER G. STILLMAN
Vassar College

In "Abstract Right," the first part of the *Philosophy of Right*, Hegel develops the rights of the person to life, liberty, and property; his right to self-defense; and his right to the wide-ranging exercise of his arbitrary freedom or free self-determination. Furthermore, in abstract rights Hegel finds the bases for what is made concrete in civil society: a capitalistic economic order ("the system of needs") and a liberal administration of justice—public, egalitarian, and characterized by due process. In "Abstract Right"—and in "Civil Society," where the concrete manifestations of abstract rights are discussed—Hegel holds, in their fullness, the central liberal positions.[1]

The Critique of the Content of Rights

But "Abstract Right" contains, usually implicitly, a critique of much liberal theory. Hegel's critique falls into two distinct categories. One major category of Hegel's critique has to do with the content of abstract rights or, as much liberal theory calls them, natural rights. Unlike most liberals, Hegel does not treat the state immediately after the discussion of the rights of man; rather, he takes a long route—through morality, the family, and civil society—before he comes to the state.

Both Hegel and Locke, for instance, explain the rights of men, and then turn to a consideration of punishment. Hegel next examines morality,[2] and then the family[3] and civil society,[4] before he finally gets to the state.[5] Locke, on the other hand, proceeds quickly to a discussion of the state.[6] Thus, Locke jumps from the natural man with his arbitrary will to the civil state which makes laws but otherwise lets man's arbitrary will have full play.[7] In other words, Locke generally assumes (or ignores) all those institutions and ways by which the natural man (or the natural child) becomes a civilized man capable of functioning well in the civil society of the *Second Treatise*.[8] But Hegel is surely correct in his argument that men plucked from the state of nature would not be able to maintain or govern a Lockean state.[9] In *On Liberty*, usually considered the classic defense of arbitrary freedom or individual freedom from social and political control, Mill is explicit in assuming—and then ignoring—the institutions and attitudes which make men civilized. *On Liberty* simply applies to the few advanced European countries where civilization can be assumed; other countries are lucky if they are ruled by a Charlemagne or the East India Company.[10]

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[1] The best discussion of the part "Abstract Right" is Joachim Ritter, "Person und Eigentum," in his *Metaphysik und Politik* (Frankfurt am Main: Suhrkamp, 1969) and (in French) as "Personne et Propriété selon Hegel," in *Archives de Philosophie*, 31, no. 2 (Apr.-June, 1967), 179–201. Ritter does not deal with the section on wrong and its annulment, for which see my "Hegel's Idea of Punishment," *Journal of the History of Philosophy* (forthcoming, 1975).

[2] G. W. F. Hegel, *The Philosophy of Right*, trans. T. M. Knox (Oxford: Clarendon Press, 1942; corrected ed., 1945), secs. 105–140. In citations to the *Philosophy of Right*, where the material cited is from the main text of the section, the section number alone is given; where it is from the "remarks" Hegel added to the text, the section number is followed by "R"; where it is from the "additions" which later editors appended to posthumous editions by collating student lecture notes, the section number is followed by "A."

[3] *Ibid.*, secs. 158–181.

[4] *Ibid.*, secs. 182–256.

[5] *Ibid.*, secs. 260–329.

[6] John Locke, *Two Treatises of Government*, revised edition, ed. Peter Laslett (New York: Mentor Books, 1963), *Second Treatise*, secs. 4–22. (Hereafter cited as *Second Treatise*.) See also Thomas Hobbes, *Leviathan*, intro. Richard S. Peters (New York: Collier Books, 1962), Part I, esp. chap. 14 and Part II, esp. chap. 17.

[7] Locke, *Second Treatise*, sec. 22. Interpretations of Locke on the question of freedom are numerous; the interpretation given is a literal—some would say superficial—one, in which the only "development" that the individual's freedom undergoes as the individual matures is to become sophisticated arbitrary freedom—for Locke's rationality is that of the "Industrious and Rational" (sec. 34) in the sense of economic rationality, where the arbitrary free will is calculating enough to postpone an immediate satisfaction in order to obtain a larger future satisfaction. See also Adam Smith's praise of "natural liberty" in *The Wealth of Nations*, ed. Edwin Cannan (London: University Paperbacks, Methuen, 1904; reprint ed., 1961), I, 157, 344; II, 37–38, 208.

[8] The case is similar for Hobbes, who excludes education from the text proper of *Leviathan*, relegating it to the "Review, and Conclusion," pp. 503–511.

[9] See esp. G. W. F. Hegel, *Philosophy of Mind*, trans. William Wallace (Oxford: Clarendon Press, 1894), sec. 502R, and, specifically in reference to Hobbes, G. W. F. Hegel, *Lectures on the History of Philosophy*, trans. E. S. Haldane and Frances H. Simson (London: Routledge & Kegan Paul, 1896; reprint ed., 1955), III, 317–319.

[10] John Stuart Mill, *On Liberty* (New York: Library of Liberal Arts, 1956), chap. I, p. 14. In fairness to Mill and in the interests of accuracy, it must be noted that his other works consider morality,

Hegel, on the other hand, spends much—it might be said, all—of the *Philosophy of Right* dealing not only with the political order and arbitrary freedom, but also with those various attitudes (like morality) and those various institutions (like the family and the pluralistic group[11]) by which men become fully developed men who have freedom, culture, and individuality, men who can give reasoned consent to a reasonable political order, men who are mature enough to be able to exercise their freedom. At the level of formal rights, then, for Hegel there is implicitly a serious defect, of narrowness; abstract rights alone cannot form the basis for the state; more is needed, and the sections between "Abstract Right" and the "State" describe those attitudes and institutions not explicitly present in abstract rights.

For Hegel, a basic inadequacy of abstract rights, and the reason for their narrowness, is to be found in the definition of the person. A theory of rights does not deal with man; it deals with a mere abstraction from man: the person, who is defined essentially in terms of his arbitrary free will and thus his capacity for rights.[12] Consequently, for the person, morality is irrelevant; persons may do whatever they have a right to do, regardless of morality. Indeed, not only morality, but also a whole range of human concerns—about social relations, human feelings,[13] culture, welfare, and individual development—are excluded from rights. Therefore, the person exercising his rights is not an especially attractive human, at least for Hegel:

> To have no interest except in one's formal right may be pure obstinacy, often a fitting accompaniment of a cold heart and restricted sympathies. It is uncultured people who insist most on their rights, while noble minds look on other aspects of the thing.[14]

Similarly, for Hegel the contractual relation is not the only legitimate human relation; yet the person, as the subject of rights, can only exercise his rights and can only relate to others by contract.[15] Hegel insists that neither marriage nor the state can be a contract, in part because of the strict definition of contract: its object must be "a single external thing."[16] Thus, for Hegel marriage and the state are areas of life in which contract and its concomitants—like the *Versachlichung* (literally translated, "thingification") of human relations[17] and the political theory of possessive individualism[18]—do not prevail, where feeling and community may have scope, and where human, not merely personal, characteristics exist.[19]

The person is but an abstract and narrow man; and the attenuation carries over into many areas. For instance, persons are not individuals with distinctive individual traits which set them apart from each other and give them individual value; persons are not individuated. One person differs from another simply because of external differences: color of hair, height, birthplace, impressions on his *tabula rasa* mind—in short, differences over which the person has no control and which are basically the products of nature or chance.[20] Consequently, the resultant differentiation among persons is one of eccentricity[21] and idiosyncrasy rather than of substance. For fully developed men in society, there are substantial differentiations—of class and occupation, for instance. While persons can only be eccentrics, men in society are individuals.

Further, even in regard to the free will, that important area which the person shares with the man, the person is limited. For the person is, in an important sense, not free, even though he has

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the family, the economic order, and the state (the topics of the *Philosophy of Right*); only by abstracting *On Liberty* from Mill's total work can he be considered a pure defender of arbitrary freedom or "freedom from." This illegitimate abstraction has been made by some contemporary defenders of "freedom from"; for an effective, indeed devastating criticism of their position, see the excellent but overlooked article by R. N. Berki, "Political Freedom and Hegelian Metaphysics," *Political Studies*, 16, no. 3 (Sept., 1968), 365–383.

[11] Hegel, *Philosophy of Right*, secs. 255 and 255R.

[12] *Ibid.*, secs. 35–59.

[13] Hegel's handwritten marginal note [*Randbemerkung*] to sec. 126 in his own copy of G. W. F. Hegel, *Grundlinien der Philosophie des Rechts*, fourth edition, ed. Johannes Hoffmeister (Hamburg: Felix Meiner, 1955), p. 393.

[14] Hegel, *Philosophy of Right*, sec. 37A.

[15] Thus, most legitimate human relations not based on passions (like the "concord of natural lust") are contract relations in Hobbes's *Leviathan* and Locke's *Second Treatise*. Even the relation of parent and child is a contract; see Hobbes, *Leviathan*, chap. 20, p. 152.

[16] Hegel, *Philosophy of Right*, sec. 75.

[17] *Ibid.*, sec. 40; Ritter, "Person und Eigentum," sec. 7; and, among others, Karl Marx, *The Manifesto of the Communist Party* (New York: Monthly Review Press, 1968), Part I, pp. 5–6, and Edmund Burke, *Reflections on the Revolution in France* (New York: Library of Liberal Arts, 1955), p. 86.

[18] See C. B. Macpherson, *The Political Theory of Possessive Individualism* (Oxford: Oxford University Press, 1962).

[19] Hegel's distinction here, between contractual areas of life (like civil society [*die bürgerliche Gesellschaft*]) and noncontractual or communal areas of life, is the precursor of, among others, Ferdinand Tönnies, *Community and Society* [*Gemeinschaft und Gesellschaft*], trans. and ed. Charles P. Loomis (New York: Harper & Row, 1957).

[20] See G. R. G. Mure, "Foreword," in Frederick G. Weiss, *Hegel's Critique of Aristotle's Philosophy of Mind* (The Hague: Martinus Nijhoff, 1969), pp. xiii–xiv.

[21] Thus, J. S. Mill warns, "that so few now dare to be eccentric marks the chief danger of the time" (Mill, *On Liberty*, chap. III, p. 82).

formal rights and formal freedom. He has only an "arbitrary will"; it wills whatever it wants, but its contents, from which it chooses, are given.

> If you stop at the consideration that, having an arbitrary will, a man can will this or that, then of course his freedom consists in that ability. But if you keep firmly in view that the content of his willing is a given one, then he is determined thereby and in that respect at all events is free no longer.[22]

The arbitrary will is able to choose among impulses, desires, passions, and the like; but the passions or impulses among which it chooses are a content over which it has no control. The Hegelian arbitrary will is thus quite similar to freedom as conceptualized by many liberals, like Locke and Hobbes. For them,[23] freedom and determination are compatible; as long as the individual has self-determination, it does not matter that the content of his choice is given. Locke agrees with Hobbes's reasoning that

> *liberty* and *necessity* are consistent . . . in the actions which men voluntarily do: which, because they proceed from their will, proceed from *liberty;* and yet, because every act of man's will, and every desire, and inclination proceedeth from some cause, and that from another cause, in a continual chain, whose first link is the hand of God the first of all causes, proceed from *necessity*.[24]

Locke also concurs in Hobbes's further argument that

> fear and liberty are consistent; as when a man throweth his goods into the sea for *fear* the ship should sink, he doth it nevertheless very willingly, and may refuse to do it if he will: it is therefore the action of one that was *free*.[25]

For Hegel, the self-determination of the arbitrary will is an essential element of freedom; but this freedom of the person is only one element in the full definition of freedom. Another element, for instance, is control over the contents of the choice, both in the sense that the contents be at least in part determined by the individual and not by an alien causal chain and in the sense that at least one of the choices be an alternative that is desirable and rational under any circumstances. (Thus, while Hobbes and other liberals see liberty as consistent with both fear and a restricted choice, Hegel regards a choice among unappealing alternatives as only limited freedom.[26] It is not true freedom, Hegel thinks, for a traveler to have to choose between losing his goods and death by water, for a subject to have to choose between living under an oppressive tyranny and a flight that would lose him his family and property, or for Hobbes to be given the choice, in England during the Civil War, only of fighting for the Roundheads or fighting for the Cavaliers—and not allowed the alternatives of flight or peace suitable for a man "born twins" with fear[27] and desirous of civil order.) The person of abstract rights, like the man of natural rights, has freedom in the sense of the arbitrary will or of self-determination; but the content of his choice is given, and thus in significant ways he is not free.

Like the will's freedom, political freedom is defined as severely limited by those theorists who proceed from natural rights directly to the state. When the state is built solely on formal rights and natural or abstract men, political obligation is based on a consent, given long ago or sometimes given only tacitly.[28] And obedience to specific laws is based, not on the validity or the rightness of the law, but on prudential considerations of the potential penalities for disobedience.[29] On the other hand, where the laws do not reach, the citizen can do anything, and can follow his natural impulses and arbitrary will wherever they lead him.[30] In the state based on the formal rights of natural or abstract men, who exercise tacit consent, prudential obedience, and full arbitrary freedom outside the law, the citizen has little scope for moral decisions and for questions of rightness, and little area in which he must or can develop or educate himself; the education that does occur is either education that makes the citizen more "Industrious and Rational," i.e., more eager to work and better able to calculate, than he was, or education that is peripheral to life's central concerns.[31] In

[22] Hegel, *Philosophy of Right*, sec. 15A.

[23] Among others, James Mill agrees with Hobbes and Locke; see John Stuart Mill, *Autobiography* (London: Oxford University Press, 1924), p. 91. J. S. Mill poses the dichotomy that allows for the coexistence of political (or social) freedom and philosophical determinism at the beginning of *On Liberty;* see Mill, *On Liberty*, chap. I, p. 3.

[24] Hobbes, *Leviathan*, chap. 21, p. 160; see also John Locke, *An Essay Concerning Human Understanding* (New York: Dover, 1894; reprint ed., 1959), Book II, chap. 21, esp. secs. 14, 27, and 29.

[25] Hobbes, *Leviathan*, chap. 21, pp. 159–160; see also Locke, *An Essay Concerning Human Understanding*, Book II, chaps. 21 and 27.

[26] Hegel here agrees with Aristotle; see Aristotle *Nicomachean Ethics* 3.

[27] Of himself Hobbes wrote, "Fear and I were born twins" (quoted in Peters, "Introduction," in Hobbes, *Leviathan*, p. 7).

[28] See Locke, *Second Treatise*, secs. 73, 98, 117, and 119; Hobbes, *Leviathan*, chap. 18, p. 136.

[29] See, e.g., Hobbes, *Leviathan*, chap. 17, p. 192; Elie Halévy, *The Growth of Philosophic Radicalism* (Boston: Beacon Press, 1955), esp. pp. 487–488.

[30] Hobbes, *Leviathan*, chap. 30, pp. 255–256; Locke, *Second Treatise*, secs. 22 and 57.

[31] For education in such characteristics and traits as virtue, liberality, and steadfastness is, in a sense, external to the essential and logical structure of the *Leviathan*, and therefore is described, not in the text proper of the *Leviathan*, but in the appended dedicatory and concluding material; see Hobbes,

general, political freedom—in the sense of making responsible choices and of having an environment in which the citizen must or will wish to improve and educate himself—is simply narrow in scope.[32]

Even in theory, political freedom is thus limited and meager in states built on the abstract or natural man and his rights. In historical practice, however, political conditions based on the arbitrary or self-determining wills of abstractly equal persons can produce terrifying results, as was demonstrated by the French Revolution, which aimed at full liberty and equality but produced the Reign of Terror. For arbitrary freedom in political practice is the desire to reduce all limitations on itself to nought, to be fully self-determining.[33] In society, unlimited self-determination faces institutions and social differentiations that provide frameworks and modes for choice. But absolute freedom feels such guides as limits; it desires and acts to eliminate them; and the eventual result of its action is "the destruction of the whole subsisting social order":[34]

> In this absolute freedom all social ranks or classes, which are the component spiritual factors into which the whole is differentiated, are effaced and annulled; the individual consciousness that belonged to any such group and exercised its will and found its fulfillment there, has removed the barriers confining it.[35]

With the attempt to realize absolute freedom comes the terror of destruction of the social order.

The demand for absolute freedom and the social destruction accompanying its attempted actualization are concurrent with the demand for absolute equality. Absolute equality is as ferocious as absolute freedom; from the removing of all social barriers and the attempted establishing of absolute equality follows "the elimination of individuals who are objects of suspicion to any social order, and the annihilation of any organization which tries to rise again from the ruins."[36] Absolute equality assumes that equality is defined as complete sameness and complete undifferentiation; to absolute equality, any difference, any quirk, any distinction implies inequality and must be eliminated. Since any organization or institution implies difference and distinction, no institutions—not even government—are permitted by absolute equality.[37] Everyone must be the same and have the same will: this dictum of absolute equality the government tries to enforce, by executing those who are different, even if they are different only because someone suspects them of difference; similarly the people tries to enforce the dictum against the government, deposing it when it seems to be setting itself up as different in status or in policy from the people, and thus the people rapidly overturns kings, constitutions, and parties.

The result of absolute freedom and equality, for Hegel, is simply negation; fully developed negative freedom "cannot arrive at a positive accomplishment of anything, . . . either in the shape of laws and universal regulations of conscious freedom, or of deeds and works of active freedom."[38] Any institution that develops to guide individuals, any government that tries to make any particular policy, any distinctions based on merit or worth: all are precluded by absolute freedom and equality. Whenever any such differentiation is developed, absolute freedom desires it destroyed. Consequently, "universal freedom can thus produce neither a positive achievement nor a deed; there is left for it only negative action; it is merely the rage and fury of destruction."[39]

Even without the extreme (but logically derivative) absolute freedom and terror, abstract or natural rights as the bases of a political state are, for Hegel, abstract and narrow. The person, that abstraction from man, is only a narrow—and amoral, unfeeling, and static—portion of man; he is not an individual, though he may be an eccentric; he has the arbitrary freedom of self-determination without control over the content of his choices; and his political freedom is both limited and limiting. A political order based on the abstract or natural rights of man is thus based on only a restricted portion of man; a political theory based only on abstract or natural man thus leaves unconsidered and indeed places beyond consideration questions of human morality, affective human relations, human culture and cultivation, and the development of freedom. In short, a world populated solely by abstract or natural persons with abstract or natural rights is a limited, inhuman, and inhumane world.

Because of the basic inhumanity of a world

Leviathan, "Dedicatory Epistle," p. 5, and "Review, and Conclusion," pp. 503–511; see also Locke, *Second Treatise*, esp. secs. 34 and 64.

[33] The denigration of consent and obligation, and the limiting of the scope of political freedom, form two basic elements of the depoliticization inherent in much liberalism; see Sheldon S. Wolin, *Politics and Vision* (Boston: Little, Brown, 1960), chaps. 9 and 10.

[32] Hegel, *Philosophy of Right*, sec. 5R.

[34] *Ibid.*, sec. 5R.

[35] G. W. F. Hegel, *Phenomenology of Mind*, trans. J. B. Baillie (New York: Harper Torchbooks, Harper & Row, 1910; reprint ed., 1967), pp. 601–602.

[36] *Ibid.*, pp. 605–606, and Hegel, *Philosophy of Right*, secs. 5R and 5A.

[37] Hegel, *Philosophy of Right*, sec. 5A and Hegel, *Phenomenology of Mind*, pp. 605–606.

[38] Hegel, *Phenomenology of Mind*, p. 603.

[39] *Ibid.*, p. 604; see also Hegel, *Philosophy of Right*, sec. 5R. Many current interpretations follow Hegel's; see, e.g., Alfred Cobban, *In Search of Humanity* (New York: George Braziller, 1960), esp. p. 210.

populated by abstract persons, Hegel's logic drives the *Philosophy of Right* to consider more aspects of man than solely "personhood." The limited world of persons or natural men is, with Hegel, not the final word, but rather is superseded until Hegel's political philosophy deals with men and individuals, with morality and education and culture, and with freedom in its full sense and the social institutions that produce and maintain it.

The Critique of the Liberal Formulation of Rights

The other major category of Hegel's critique centers on the crucial theoretical errors inherent in the concept of natural rights.[40] The first crucial error of the natural rights theories is that men are led almost inevitably to social contracts, for the natural man's only legitimate interpersonal political relation is contract, and thus the only way that natural men can relate, to form families and civil society, is to contract. Nonetheless, it is probable that some liberal contract theorists were not entirely pleased with the implications of their contract theories.[41] Locke, for instance, clearly tries to hedge both the marriage and social contract with noncontractual limits so that the full implications of contract would not be developed. Thus, while asserting a popular right to revolution whenever the government does not act as it was bound by the social contract, Locke emphasizes the "slowness and aversion in the People to quit their old Constitutions"[42] and the people's willingness to tolerate "*great mistakes* in the ruling part, many wrong and inconvenient Laws, and all the *slips* of humane frailty . . . without mutiny or murmur."[43] He also sees his doctrine as, in practice, "*the best fence against Rebellion.*"[44] Similarly, marriage is a contract which, at the same time "draws with it mutual Support, and Assistance, and a Common Interest too, as necessary . . . to . . . Care, and Affection [and] . . . to their common Offspring."[45] Locke is frequently regarded as "judicious" in doctrine and phrasing; Hegel, on the other hand, rather than adopt the Lockean hedging, is simply clear: marriage and the state are not in essence contracts.[46]

The second crucial theoretical error is to see natural or autonomous man as giving up his right to self-defense (or to punish) to the state in return for the benefits of society.[47] For, Hegel argues, at the level of the prepolitical, where there are no positive laws nor magistrates, the annulment of crime takes the form of revenge, not punishment.[48] Thus, the abstract right (and the natural right) to coerce is always revenge,[49] and individuals in a prepolitical condition have the right to revenge, not the right to punish. Clearly, then, it is a misconception to argue that men in the state of nature have a right to punish which they can transfer to the sovereign by compact. They cannot give up the right to punish, because they do not possess it; all they can give up is the right to revenge. Therefore, the liberal contract state gains, not the right to punish, but the right to revenge. As Hegel points out, however, no act of revenge is justified, because the agent is interested, arbitrary, and lacking in authority, and because any act of revenge can start an infinite chain of revenge.[50] Therefore, the liberal state has the right to revenge, a right which it ought not exercise in any specific case and a right which, if exercised, may well start a cycle of revenge and alienate some of the state's citizens for life or, indeed, for generations.[51] For Hegel, therefore, the assertion that the state obtains from its citizens the right to punish is incorrect; all that the state can obtain from natural (or abstract) men by compact is their right of revenge to annul crime in an undisinterested way to which the criminal rarely consents. To use the social contract, or the alienation of the right to punish, as the basis for the state is to commit a theoretical error.[52]

It might be noted that one possible liberal rejoinder would be to argue that the right of revenge of private individuals, when it is given to and accepted by the state, becomes the right of punish-

[40] Hegel's critique of the conceptualizations of natural rights is equally applicable as a critique of some liberals' equivalent of natural rights, the free and autonomous prepolitical man.

[41] Implications like the *Versachlichung* of human relations, possessive individualism, and the impermanence and easy dissolubility implied in contractual marriage and state; see note 17, above; note 18, above; Hegel, *Philosophy of Right,* secs. 75A, 161A, and 258A; and Emile Durkheim, *The Division of Labor in Society* (New York: Macmillan, 1933), pp. 203–204.

[42] Locke, *Second Treatise,* sec. 223.

[43] *Ibid.,* sec. 225.

[44] *Ibid.,* sec. 226.

[45] *Ibid.,* sec. 78.

[46] Hegel, *Philosophy of Right,* secs. 75, 75R, and 75A. J. S. Mill denies only implicitly that the state is a contract; see Mill, *Considerations on Representative Government* (New York: Library of Liberal Arts, 1958), chap. 1, pp. 6–11.

[47] As in, e.g., Locke, *Second Treatise,* secs. 13 and 88.

[48] Hegel, *Philosophy of Right,* secs. 102–103. This is a not uncommon distinction: see K. G. Armstrong, "The Retributivist Hits Back," *Mind,* 70, No. 280 (Oct. 1961), 487–488; and T. H. Green, *Lectures on the Principles of Political Obligation* (Ann Arbor, Michigan: University of Michigan Press, 1967), sec. 178.

[49] As in Hobbes, *Leviathan,* chap. 28.

[50] Hegel, *Philosophy of Right,* secs. 102, 102A, and 220; Stillman, "Hegel's Idea of Punishment," sec. I.

[51] It might be inferred that Hobbes and Locke, by seeing the right of punishment as a prepolitical right, do not regard as theoretically central the distinctions between an interested and a disinterested judge and between a consenting and a disaffected criminal. For T. H. Green (sec. 178), they also fail to understand rights correctly.

[52] Green (sec. 178) makes similar and related (and Hegelian) arguments along this line.

ment. But this argument contains within itself an interesting presupposition that actually runs counter to the liberal position. For if the state, by accepting rights from individuals, can transform or even redefine the individuals' rights—which are the bases and origin of the state—then the state must have some existence independent of the rights granted it; that is, the state must have some basis on which or from which it is able to transform or redefine rights. This basis cannot be obtained from the contracting or consenting individuals, because the ability to transform revenge to punishment is not an ability of the individual; he cannot give to the state something he does not have himself. Consequently, since the basis from which the state redefines revenge to be punishment derives not from individuals, it must rest in the state, and therefore the state does have some existence and content independent of those individuals from whose alienated rights alone the state is supposedly composed. A liberal rejoinder, then, can end up showing what it does not want to prove: that the origin and basis of the state is not solely derived from its citizens.

The final crucial theoretical error inherent in theories of natural rights is the conclusion that the state must limit man's natural freedom and autonomy. This error stems not only from the state of nature hypothesis. It also derives from the tendency of modern and liberal political philosophers to begin political reflection with a consideration of the "I" or "Of Man," as have moderns from Descartes and Hobbes,[53] and to develop this "I" or "man" to his full concreteness, humanity, and social behavior before a discussion of society, so that, for instance, the Hobbesian or the Lockean "man" has the same characteristics within civil society as outside it, in the state of nature as in the civil state. One result is, of course, that this completely civilized and human man, developed in a logical abstraction from society, must necessarily be limited in his rights as soon as he is put in a social context.

At best, this usual liberal point of view is slightly depressing: all men's rights and liberties must be limited. At worst, it is dangerous: if liberties must be limited for security or utility, the scope of limitation may be unlimited, as, for instance, in a "closed commercial state," or in extreme forms of pragmatism.[54] And, on the surface, it is paradoxical: for men join states in order to protect their life, liberty, and property, and yet immediately upon joining find their property and liberty limited, and—in times of war or in case of crime—their life endangered.

Hegel has a threefold answer to the problem that society must limit the rights of autonomous man. In the first place, the "I" or "man" that forms the first determination of modern and liberal political philosophy is not and cannot be a fully developed human being. If he is abstracted from civil society, those characteristics of his which are social must be omitted, and indeed any characteristics which man outside of society would have must be added. It is an invalid logical sleight of hand which abstracts a fully socialized man from society and proclaims him therefore an autonomous man whose characteristics are the opening determination of a political theory. Before Hegel, Rousseau recognized the logical problems of the prepolitical autonomous man;[55] Rousseau adopts a hypothetical historical approach to try to eliminate the problem.[56] Seeing the same problem, Hegel adopts the usual liberal approach of developing the rights of men in a prepolitical state. Hegel's prepolitical state is, however, not a Rousseauian natural state prior in time, but an abstract state which is prior only in terms of logic. In Hegel's prepolitical state, as in that of all liberals, the fully socialized human is abstracted from society; but Hegel's formulation is distinctive in also abstracting the prepolitical man from his socialized behavior and attitudes.[57] Thus, the Hegelian "I" or "man" of abstract right is simply an abstract "I" or abstract "man": a person.

The prepolitical man, the liberal's natural man, is not a full individual but merely an abstract person: Hegel's logic is accurate. And, by considering the abstract person rather than the autonomous man in the prepolitical condition, Hegel makes it clear at least that the prepolitical condition is a realm not only of greater freedom (from laws and other governmental limits) in some ways but also of lesser freedom and of lesser humanity than the social state: for the sphere of

[53] René Descartes, *Meditations on First Philosophy*, in *Descartes: Philosophical Writings*, trans. Norman Kemp Smith (New York: The Modern Library, 1958), esp. "Meditation II," pp. 182–187; Hobbes, *Leviathan*, Part I.

[54] Reinhold Aris, *History of Political Thought in Germany* (London: George Allen & Unwin, 1936), pp. 125–135; John Dewey, *Liberalism and Social Action* (New York: Capricorn Books, 1935; reprint ed., 1963).

[55] Jean-Jacques Rousseau, *The First and Second Discourses*, ed. Roger D. Masters (New York: St. Martin's, 1964), *Second Discourse*, "Preface," pp. 91–93.

[56] *Ibid.*, "Preface," p. 97.

[57] Hegel's abstract man, the person, is abstracted from his social contexts but not from his historical development; the person lacks socialized characteristics but is nonetheless an abstraction from contemporary man, and thus the person claims for himself the rights that a contemporary man claims. In contrast, Rousseau's primitive man in the *Second Discourse*, when logically isolated from society, loses all his socialized and historically-developed characteristics ("Preface," pp. 91–97); Hobbes's natural man, when logically isolated from society, retains all his socialized and historically-developed characteristics (Hobbes, *Leviathan*, chap. 13).

abstract or natural rights contains not only no governmental limitations but also, among other aspects, no morality, no feelings, no individuation, and an arbitrary freedom that is itself rather limited. It is clear in the Hegelian formulation that, while the prepolitical person may be limited when put in a concrete social context, he will also gain some desirable and human characteristics when he is put in a social context. What most liberals see simply as a limitation, Hegel's abstraction shows as also a development.

Hegel's second answer is that the limits imposed in society on the rights of prepolitical autonomous or abstract men are actually prepolitical limits as well. For the existence of the prepolitical annulment of crime makes quite clear that, purely at the level of rights, individuals are limited by rights. Thus, the progress from the prepolitical to the political is not the shift from autonomy and freedom to legal limits; rather, it is the shift from rights as limits to laws as limits.[58]

Hegel's third answer to the mistaken liberal conceptualization of the prepolitical autonomous man with rights and liberties is simply that only in civil society and the state do rights and liberties exist in an actualized and concrete form; therefore, the state, instead of limiting rights, is in fact the precondition for their existence.[59] Without the state and political order, rights are unsure; only with society do rights exist actually and securely. Indeed, Hegel's three points together indicate the necessity of state and society for individual rights and liberties.

[58] The shift is desirable, of course, because the enforcement of rights is uncertain, that of laws fairly sure. Locke and Hegel agree here; see Locke, *Second Treatise*, secs. 13 and 22–23, and Hegel, *Philosophy of Right*, secs. 103 and 217.

Conclusion

In sum, Hegel cogently criticizes natural rights theories for their narrownesses and for their theoretical inadequacies. Yet his criticisms are made in the context of his asserting abstract rights and thus do not lead to a rejection of the rights themselves, but merely of their previous formulations. Therefore, the criticisms point the way to later portions of the *Philosophy of Right*—to Hegel's treatment of morality, educative institutions, institutions that are not contractual—just as Hegel's assertion of abstract rights points the way to his exposition of civil society and other later sections of the text in which he discusses the concrete actualizations of rights.

[59] Hegel's argument is similar to Hobbes's here; see Hobbes, *Leviathan*, chap. 17, and Hegel, *Philosophy of Right*, secs. 142 and 260. For Hobbes, however, rights exist in the state of nature and war (*Leviathan*, chap. 14, pp. 103–104), whereas for Hegel rights are brought into existence only after centuries of civilization's cultural, social, spiritual, and political development (*Philosophy of Right*, secs. 341–360).

Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis*

LAWRENCE C. DODD
University of Texas, Austin

Since the days of A. Lawrence Lowell, students of parliamentary democracy have argued that durable cabinets require majority party government. As Lowell writes:

> It is an axiom in politics that, except under very peculiar circumstances, coalition ministries are short lived compared with homogeneous ones . . . .
> A cabinet which depends for its existence on the votes of the Chamber can pursue a consistent policy with firmness and effect only when it can rely for support on a compact and faithful majority; and therefore the parliamentary system will give the country a strong and efficient government only in case the majority consists of a single party.[1]

Following the publication of Lowell's book, *Government and Parties in Continental Europe* (1896), every generation of political scientists has witnessed the recitation and reaffirmation of Lowell's thesis. The list of advocates includes Lord Bryce (*Modern Democracies*, 1921), Harold Laski (*Parliamentary Government in England*, 1938), F. A. Hermens, *Democracy or Anarchy* (1941) and Maurice Duverger (*Political Parties*, 1951).[2] A major theme found in all of these classics is summarized concisely by Duverger: "Multipartism weakens the government in a parliamentary regime. . . . The absense of a majority party makes it necessary to form heterogeneous . . . cabinets based on a coalition, or else minority cabinets." As a consequence, "cabinet collapses which are exceptional and rare under the two-party system become normal and frequent and are scarcely mitigated by the fact that the same men are often to be found in different ministerial combinations."[3]

In our own generation, Lowell's thesis is reflected most clearly in the work of Jean Blondel. In "Party Systems and Patterns of Government in Western Democracies" (1968), Blondel writes that the "duration (of cabinet governments) is unquestionably influenced by the type of party system prevailing in the country." According to Blondel:

> Two factors . . . appear to contribute to the relative duration of governments in parliamentary systems: the fact that the system is a straight clash between two parties and the fact that one party government prevails. The two factors are quite clearly connected: one-party governments are not possible without at least one large party, except on a temporary basis. On balance, . . . it does seem that one-party government . . . is the factor contributing most decisively to the stability of governments.

In contrast to one-party governments, Blondel concludes that "Coalition, whether small or large, appears directly antagonistic to stable government . . . "[4] Blondel's index of stable government is cabinet durability.

The empirical evidence presented in these works has been of two different types. Scholars from Bryce to Duverger supported Lowell's thesis by comparing the unstable experience of France or Weimar Germany to the stable government of the United Kingdom, implying that the former were typical multiparty regimes whereas the latter was the typical majority party system. Blondel made a major advancement by expanding the number of countries examined; he analyzed 17 Western parliamentary democracies between 1946 and 1966, focusing on the average differences in cabinet durability between party systems. Blondel's analysis indicated that (1) majority party systems produce more durable cabinets on the average than multiparty cabinets and (2) single

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[1] A. Lawrence Lowell, *Governments and Parties in Continental Europe* (Boston: The Riverside Press, 1896), I, 70, 73; see also Lowell, *The Governments of France, Italy and Germany* (Cambridge: Harvard University Press, 1914), pp. 70–74.

[2] James Bryce, *Modern Democracies* (New York: Macmillan Company, 1921), I and II, 121–122, 347–348; Harold J. Laski, *Parliamentary Government in England* (New York: The Viking Press, 1938), pp. 56–57; Ferdinand A. Hermens, *Democracy or Anarchy?* (Notre Dame: University of Notre Dame Press, 1941), pp. 16–17, 68–69, 72–74; and Maurice Duverger, *Political Parties* (London: Methuen Press, 1951).

[3] Duverger, pp. 407–408.

[4] Jean Blondel, "Party Systems and Patterns of Government in Western Democracies," *Canadian Journal of Political Science*, 1 (June, 1968), 198–199.

party governments are more durable than coalition governments.[5]

These works clearly build on Lowell's analysis; they uncover patterns that support Lowell's predictions. Nevertheless, critical problems remain before Lowell's thesis can be accepted. First, these studies rely on broad contrasts of central tendencies between majority party systems and multiparty systems. They fail to examine systematically the variation around central tendencies. Second, Lowell's followers have offered no real theoretical justification for his thesis. They have repeated his axiomatic assertion that coalitions are nondurable, failing to note that this axiom, if genuinely true, would reduce political analysis to chaos, since political associations (particularly political parties) are all coalitions of some sort. Concern with Lowell's thesis has proceeded largely at the inductive level with each new generation offering new empirical evidence that presumably supports his thesis.[6] The thesis has yet to be subsumed within a coherent theory. Thus we are left with two questions. Is it really true that durable cabinets do not exist in multiparty parliaments? Is Lowell's axiom that coalitions are nondurable really axiomatic?

Based on analysis of the countries and time periods listed in Table 1, the answer to each question is a firm no. Lowell rests his thesis on the assertion that multiparty parliaments cannot produce durable cabinets; only majority party parliaments produce durable cabinets. From the perspective of central tendencies, these assertions seem correct. For the countries and time periods listed in Table 1, one party majority cabinets persisted in office for approximately 48 months on the average; cabinets in multiparty parliaments lasted 26 months on the average. As Table 2 indicates, however, the analysis of central tendencies is hiding a great degree of variation in cabinet durability among multiparty parliaments.

Table 2 presents patterns for all multiparty parliaments of the countries and time periods in Table 1. As throughout this study, (1) multiparty parliament refers to any parliament with no majority party; majority party parliament refers to any parliament with a majority party; (2) a cabinet is durable so long as there is no change in the parties within the cabinet. As Table 2 indicates, there is wide variation in the durability of cabinets formed in multiparty parliaments, with 23 per cent of all *cabinets* lasting 40 months or longer. In 34 per cent of the multiparty *parliaments*, the shortest cabinet lasted at least 40 months; the percentage would be higher if we included those parliaments in which no new cabinet formed but during which an old cabinet continued. And of those governments that were coalitions, approximately 22 per cent lasted 40 months or longer. These patterns indicate that while the average durability of cabinets may be low for multiparty parliaments, a large number of durable cabinets exist and are concentrated in certain specific parliaments rather than spread evenly throughout all parliaments. Finally, Lowell was simply wrong in his assertion that coalitions are necessarily transient. Even where periods of wartime are omitted, some coalitions are quite durable.

Clearly, the validity of Lowell's thesis must be challenged. Multiparty parliaments do not appear as detrimental as the scholarly consensus of the past 70 years would suggest; party coalitions are not necessarily antagonistic to durable government. A re-examination of the impact of party systems on cabinet durability is in order. Why are some coalitions durable whereas others are not? Why do some multiparty parliaments produce durable cabinets whereas others do not? What impact does the party system have on cabinet durability?

The answers to these questions lie in the complex world of multiparty parliaments. Politics in this world has been characterized quite aptly by Maurice Duverger as a "parliamentary game"; the object of the game is to form and control the government; in the game, a durable government "must find support from a coalition of associated parties: their alliance is always uneasy, and intrigues are perpetually being hatched in the lobbies of parliament to break up the existing combination and replace it with a new one."[7] Some intrigues succeed and a cabinet falls; other intrigues fail and a cabinet persists. The explanation derives from understanding the parliamentary game.

The purpose of the present study is to provide an explanation of cabinet durability based on a game-theoretic analysis of party coalitions in multiparty parliaments. The thesis of the study can be summarized as follows:

(1) Cabinet durability in multiparty parliaments is determined by the coalitional status of the cabinet in power. Minimum winning cabinets will be quite durable; oversized and undersized cabinets will be more transient.
(2) The coalitional status of the cabinet that forms in a parliament is determined by the bargaining conditions that exist in parliament. Two relevant conditions are: (1) information certainty; (2) the *a*

[5] Jean Blondel, *An Introduction to Comparative Government* (New York: Praeger Publishers, 1969), p. 342.

[6] For a recent study in this vein see Michael Taylor and V. M. Herman, "Party Systems and Governmental Stability," *American Political Science Review*, 65 (March, 1971), 28–37.

[7] Duverger, p. 400.

Table 1. Countries and Time Periods Investigated

Country	Time Period	Country	Time Period
Australia	1919–1940, 1945–1972	Great Britain	1919–1940, 1945–1972
Austria	1919–1932, 1945–1972	Iceland	1924–1939, 1945–1972
Belgium	1919–1939, 1946–1972	Ireland	1945–1972
Canada	1918–1940, 1945–1972	Italy	1946–1972
Denmark	1920–1940, 1945–1972	Luxembourg	1926–1940, 1946–1972
Finland	1918–1940, 1945–1972	Netherlands	1918–1940, 1946–1972
France	1918–1940, 1945–1958	New Zealand	1918–1940, 1945–1972
Germany	1918–1932, 1949–1972	Norway	1920–1935, 1945–1972
		Sweden	1920–1939, 1945–1972

priori willingness of parties to bargain. As these conditions vary within a parliament or between parliaments, the coalitional status of the cabinets will vary.

(3) The bargaining conditions that exist within a parliament are influenced by the nature of the parliamentary party system. Three relevant party system characteristics are fractionalization, instability and cleavage conflict.

The clear inference of the thesis is that the parliamentary party system does affect cabinet durability. In this sense the thesis is an extension of Lowell's emphasis on the role of the parliamentary party system. The thesis departs from Lowell's position, however, in three major ways: (1) cabinets within multiparty parliaments are viewed as subject to wide variation in durability; (2) party coalitions are viewed as a potential source of durability as well as transiency; (3) the impact of party systems on cabinet durability is a much more complex and multivariate process than Lowell and his followers indicate.

The remainder of this study analyzes the thesis in more detail. The first two sections present the theoretical explication of the thesis and an empirical interpretation of its conceptual elements. The next section presents initial tests of hypotheses drawn from the theory. The data bank employed in these tests covers all multiparty parliaments in the countries and time periods listed in Table 1 and includes a list of the cabinets that formed in the parliaments, the parties that held ministerial seats, a list of all parties in the parliament and the legislative strength of these parties in the parliament.[8] The concluding section discusses the implications of the study.

A Theory of Cabinet Formation and Maintenance

The distinguishing characteristic of parliamen-

[8] The cabinet data analyzed in this study were collected under the auspices of the Minnesota Historical Data Archives. The Archives are directed by William H. Flanigan and Edwin Fogelman under a grant from the Ford Foundation. The data were collected by Illa Miscoll, currently at the University of Heidelberg (Germany), and by the author. I am grateful to Flanigan and Fogelman for allowing me access to the data, and I owe a huge debt to Illa Miscoll for her patience and precision. The original sources from which the cabinet data were collected include literally hundreds of scholarly works and newspapers that cannot be cited here. Every attempt was made to verify all data by at least two sources. And, while most of our research was conducted prior to its availability, a useful work for our verification process was Klaus von Beyme, *Die parlamentarischen Regierungssysteme in Europa* (Munchen: R. Piper and Co., 1970), pp. 901–967.

The party system data were gathered by the author, relying basically on a series of specialized country documents as well as *Keesing's Contemporary Archives* (London: Keesing's Publication, 1945–1972); Dolf Sternberger and Dieter Nohlen, *Die Wahl der Parlamente* (Berlin: Walter De Gruyter and Co., 1969); Stein Rokkan and Jean Meyriat, *International Guide to Electoral Statistics* (The Hague: Mouton, 1969).

Table 2. Cabinet Durability in Peacetime Multiparty Parliaments: A Frequency Count, 1918–1972

Cabinet Durability (in months)	All Cabinets	Shortest Cabinet Per Parliament[a]	Coalition Cabinets
50 or more	28 (12.5%)	24 (18.9%)	25 (14.1%)
40 to 49	23 (10.3%)	20 (15.7%)	15 (08.5%)
30 to 39	22 (09.9%)	14 (11.0%)	12 (06.8%)
20 to 29	37 (16.6%)	15 (11.8%)	27 (15.3%)
10 to 19	45 (20.2%)	15 (11.8%)	40 (22.6%)
0 to 9	68 (30.5%)	39 (30.7%)	58 (32.8%)
Total	223 (100%)	127 (100%)	177 (100%)

[a] This count includes only those parliaments in which a new cabinet was formed.

tary democracy is that the chief executive and his cabinet are dependent upon parliament for their installation and maintenance.[9] Within the life of any parliament, numerous cabinet governments can be installed and deposed through party negotiation.[10] There are no general legal limitations to the nature and type of cabinets that are formed. The parties in a cabinet may control a bare majority of parliamentary seats, virtually all seats, or virtually no seats. The distribution of ministerial positions within a cabinet may be proportional to legislative strength in parliament, reflect ideological or emotional biases, or be totally disproportionate to legislative seats.[11]

In the following theory,[12] parliamentary parties are treated as the basic actors. This convention is followed to simplify and clarify the presentation. Clearly, parliamentary parties are not homogeneous and monolithic entities but are themselves coalitions of factions held together by leaders through the use of a variety of payoffs.[13] In addition, some members of parliament may not belong to parliamentary parties, or members of various parties may belong to supra party organizations that cut across various parties. These factors can influence cabinet formation and maintenance. They are complications that can be added after a simple framework has been constructed and tested. The present goal is to construct an explanatory framework and to examine its predictive utility.

The Basic Framework. Let us assume that political parties enter parliament in a quest for govern-

[9] On the nature of parliamentary democracy see Michael Ameller, *Parliaments: A Comparative Study* (London: Cossell and Company, Ltd., 1966); C. M. F. Campion and D. W. S. Liddendale, *European Parliamentary Procedure: A Comparative Handbook* (London: George Allen and Unwin, Ltd., 1953); Carl J. Friedrich, *Constitutional Government and Democracy* (Boston: Ginn and Company, 1950), pp. 222–237, 296–386.

[10] For discussion of party behavior in the various countries, see Walter Rice Sharp, *The Government of the French Republic* (New York: D. Van Nostrand Co., Inc., 1938), pp. 73–77; Sven Groennings, "Patterns, Strategies and Payoffs in Norwegian Coalition Formation," in *The Study of Coalition Behavior,* ed. Sven Groennings, E. W. Kelly, and Michael Leiserson (New York: Holt, Rinehart and Winston, Inc., 1970); Peter H. Merkl, "Coalition Politics in West Germany," in Groennings et al., *The Study of Coalition Behavior;* Herbert P. Secher, "Coalition Government: The Case of the Second Austrian Republic," *American Political Science Review,* 52 (September, 1958), 791–808; Duncan MacRae, Jr., *Parliament, Parties and Society in France, 1946–1958* (New York: St. Martin's Press, 1967); Giorgio Galli and Alfonso Prandi, *Patterns of Political Participation in Italy* (New Haven: Yale University Press, 1970); Dankwart K. Rustow, *The Politics of Compromise: A Study of Parties and Cabinet Government in Sweden* (Princeton, N.J.: Princeton University Press, 1956). See also the various essays in Robert A. Dahl, ed., *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966). For more general discussions, see Duverger, *Political Parties,* pp. 281–351; Sven Groennings, "Notes Toward Theories of Coalition Behavior in Multiparty Systems: Formation and Maintenance," in Groennings et al., *The Study of Coalition Behavior.*

[11] This study will not attempt to include in the analysis the influence of any procedures specific to particular parliaments although such procedures can certainly have a vital influence on cabinet formation and maintenance. For examples, see Gerhard Loewenberg, *Parliament in the German Political System* (Ithaca, N.Y.: Cornell University Press, 1966). A systematic cross-national analysis of the nature and consequences of parliamentary procedure is clearly needed by analysts of cabinet behavior. For a recent study of the actual distribution of ministerial positions among cabinet parties see Eric C. Browne and Mark N. Franklin, "Aspects of Coalition Payoffs in European Parliamentary Democracies," *American Political Science Review,* 68 (June, 1973), 453–469.

[12] Essentially, the thesis results from the convergence of two schools of analysis. The first school is that represented by Lowell, Blondel, Taylor-Herman, Giovanni Sartori, and others; this school emphasizes the influence of the parliamentary party system on cabinet durability. The second school includes von Neumann and Morgenstern, Riker, Leiserson, Axelrod, Browne, Groennings, and others; some of these scholars were instrumental in developing game-theoretic models, while others have emphasized the potential utility of game-theoretic models in the study of multiparty parliaments. The present study synthesizes the theses of these two schools into a broader theory of cabinet formation and maintenance in multiparty settings. See Jean Blondel, "Party Systems"; A. Lawrence Lowell, *Governments and Parties in Continental Europe;* Taylor and Herman, "Party Systems and Governmental Stability"; Giovanni Sartori, "European Political Parties: The Case of Polarized Pluralism," in *Political Parties and Political Development,* ed. Joseph LaPalombara and Myron Weiner (Princeton, N.J.: Princeton University Press, 1966); John von Neumann and Oskar Morgenstern, *The Theory of Games and Economic Behavior* (Princeton: Princeton University Press, 1944); R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley, 1957); William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962); Michael Leiserson, "Factions and Coalitions in One-Party Japan: An Interpretation Based on the Theory of Games," *American Political Science Review,* 62 (September, 1968) 770–787; Robert Axelrod, *Conflict of Interest* (Chicago: Markham, 1970); Eric C. Browne, "Testing Theories of Coalition Formation in the European Context," *Comparative Political Studies,* 3 (January, 1971), 1–15; see also the various articles in Groennings et al., *The Study of Coalition Behavior.*

[13] See Duverger, *Political Parties,* pp. 61–132; Leon D. Epstein, *Political Parties in Western Democracies* (New York: Praeger Pub., 1967), pp. 98–129; Frank J. Sorauf, *Party Politics in America* (Boston: Little, Brown and Co., 1968), pp. 82–92. At a more theoretical level see particularly Norman Frolich, Joe A. Oppenheimer and Oran R. Young, *Political Leadership and Collective Goods* (Princeton, N.J.: Princeton University Press, 1971).

mental power. This quest originates at the electoral level as the parties seek popular support for their candidates. At the electoral stage, each party articulates a political program designed, at least in part, to attract a political following so as to maximize the party's legislative strength within parliament. The quest for power culminates, in a narrow sense, as a party or set of parties forms the cabinet and successfully maintains control of the cabinet for a time sufficient to enact and administer their legislative program.[14] In a parliament with no majority party, the quest by each party for secure participation in and control of the cabinet becomes a search, in Middleton's terms, for "a coalition of natural allies."[15] Immediately upon the conclusion of the electoral stage, "Parties . . . (begin) trying to discern the physiognomy of the assembly, and particularly to forecast the majority—or the various possible majorities—which it is capable of producing. Groups are rarely content simply to represent shades of electoral opinion. They look forward to taking an active part in government. . . . Hence the importance of the calculation of the visible and latent tendencies of the newly elected assembly."[16]

In this calculation, the central motivation is the desire by each party to enter a durable cabinet and to maximize the significant positions that it will hold within the cabinet. As representative organizations mandated by their various constituencies to enact legislative programs, each party could seek to do no less. This motivation dictates that, in immediate negotiations, each party opposes every other party. Each party seeks its own maximum ministerial advantage: each parliamentary party behaves as an actor in a conflictual coalitional game. The resources that a party brings to the game are its legislative votes. The desired payoff, in an immediate sense, is one or more ministerial positions in the cabinet. In the game, each party seeks to enter a coalition of parties that will have a reliable majority with which to install and sustain the cabinet. At the same time, each party seeks to enter a coalition through which it can maximize its own ministerial status. This involves fairly complex calculations.

Each party must estimate not only which party coalitions can win but also (1) which coalitions are possible in the sense that each coalition partner would view the coalition as a desirable one, and (2) which coalition or coalitions will be favorable to it in terms of ministerial seat payoffs. This calculation is complicated because some ministerial positions are more desirable than others, so that the value of payoffs received in each potential coalition are not calculable strictly in terms of the number of ministerial seats obtained. Owing to the intricacy and complexity of these calculations, parliamentary parties can be overwhelmed in coalition negotiations. To avoid this result, parties employ a basic criterion in all calculations and negotiations: they seek first and foremost to enter a minimum winning coalition. That is, they seek to enter a coalition of parties that (1) has a reliable majority, yet (2) contains no party in the coalition that is unnecessary to majority status.

The minimum winning criteria provides a baseline against which all possible coalitions can be compared by the parties. If a coalition is not of minimum winning status, it is either (1) not of winning status or (2) contains some unnecessary party within it that could be removed from the cabinet with its ministerial payoffs distributed among the other coalition partners. The former coalition is undesirable to a party because it would not guarantee victory in the cabinet formation and maintenance process. The latter coalition is undesirable to a party because it would waste desirable payoffs unnecessarily, denying to the party the maximum ministerial status that it could receive if only the necessary parties were included in the coalition. Seen in this perspective, the search for natural parliamentary allies is a search for partners with whom to form a minimum winning coalition. In varying coalitional conditions parties also may bring other considerations to bear such as the programmatic perspective of their potential coalition partners.

In the effort to enter a minimum winning coalition of parties, as actors in any conflictual coalitional game, parliamentary parties are constrained by a number of parameters. First, events of overriding seriousness may threaten the existence of the parliamentary system, challenging the security of the coalitional game itself. In such a situation, most clearly seen in wartime, the motivation for parties to maximize their participation in the cabinet may be overridden by the desire to save the game. Second, parties are constrained by explicit and implicit rules of the parliamentary game. An explicit rule is that a majority of 50 per cent plus one of legislative votes is the level at which a coalition assures itself winning status. An implicit rule of the game is that the cabinet formation process cannot last indefinitely. The longer the coalition bargaining process lasts, the greater the pressure becomes for some cabinet to be formed. Eventually, if no majority solution can

[14] These are general assumptions shared with numerous analysts. See for example Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), pp. 21–35, 142–163; Gunnar Sjoblom, *Party Strategies in a Multiparty System* (Sweden: Lund, 1968), pp. 68–95, 250–279.

[15] W. L. Middleton, *The French Political System* (London: Ernest Benn, Ltd., 1932), p. 154.

[16] Middleton, pp. 107–108.

be reached, then that minority cabinet will be installed that can attain the largest plurality of votes. Such a cabinet will persist only until a larger coalition arises that is willing to take over cabinet responsibility or until parliament is dissolved.

Finally, parties are constrained in their calculations and negotiations by the bargaining conditions that prevail within parliament. As noted earlier, the quest for governmental power in a multiparty parliament can be conceived as a search by each parliamentary party for partners with which to form a minimum winning coalition. The calculations and negotiations in this search are intricate and complex. First, parties must estimate which of the possible party coalitions possess a reliable parliamentary majority and, among these coalitions, which ones achieve minimum winning status. These calculations require that parliamentary parties possess reliable information as to the voting strength of the various parliamentary parties. Second, as they negotiate parties must realistically estimate which parties are available to them as potential coalition partners. To do this, they must know the implicit as well as explicit commitments made by parties to one another. Third, if parties are to enter coalitions, they must be willing to negotiate with other parties, "to form coalitions and to compromise."[17] They must be willing, at least in principle, to enter into cabinets with other parliamentary parties.

These bargaining conditions can be subsumed under two categories: (1) information certainty, and (2) the generalized *a priori* willingness of parties to bargain.

(1) Following Riker,[18] information certainty entails two elements:
   (a) *complete information as to weights*—exists in a parliamentary setting when all parties know precisely the weight (that is, the reliable number of legislative votes) controlled by all parliamentary parties.
   (b) *perfect information as to prior moves*—exists in the bargaining arena when each party knows all of the moves (that is, offers, bargains, counter-offers, etc.) made among all of the various parties in coalition negotiations.
(2) A generalized *a priori* willingness of parties to bargain exists when all parties are willing to consider entering a cabinet coalition with any other party. In other words, there are no *a priori* constraints which circumscribe or inhibit the negotiation and coalition formation between any two parties.

If we assume (1) that most coalition bargaining between parties occurs under conditions unconstrained by extraordinary events such as war and (2) that the rules of the game specified earlier are indeed operative, then the critical factors in cabinet formation are the bargaining conditions that characterize a parliament.

Cabinet Coalitional Status and the Willingness of Parties to Bargain. If parties in parliament possess a fairly generalized willingness to bargain among themselves, majority or winning coalitions should always form. In other words, so long as there are no serious *a priori* constraints on parties which make them hesitant to negotiate or strike bargains, we can expect that parties can reach agreement on some majority that will govern. By contrast, if parliamentary parties are highly constrained in their willingness to bargain among themselves, there should be a tendency toward minority cabinets. The central question here is whether there is a set of parliamentary parties (1) that possess a majority and (2) each party in the set is willing to negotiate with all other parties in the set. In a parliament in which every party is willing to bargain with every other party, numerous majority sets exist. However, the more specific each party is in specifying the party or parties with which it will bargain, the greater the possibility becomes that there is no majority set of parties willing to bargain. Consequently, the less generalized the willingness of parties to bargain among themselves, the greater the possibility of a minority cabinet forming.

Figure 1 depicts the resulting relationship between the generalized *a priori* willingness of parties to bargain and cabinet coalitional status. In this graph the *y* axis indicates the coalitional status of the cabinet that forms in a parliament. This status can be of three general types:

(a) *greater than minimum winning status* (*>mwc*). Cabinets in this category consist of at least one

[17] This point is made clearly by Bracher:

> The cabinet crises which plagued the German Republic throughout its life were the direct consequences of the nature of German political parties, their unwillingness to form coalitions and to compromise, their rigid ideological stance, their preoccupation with prestige, and their authoritarian tradition.
> This was the price paid for the partly forced, partly voluntary rejection of parliament and parties during the monarchy, a policy whose negative aspects were pinpointed very clearly by Max Weber . . . (who) insisted that the absence of a two-party system was not the obstacle: "Far more important is another difficulty: parliamentary government is possible only if the largest parties of the parliament are in principle *on the whole* ready to take over the responsible conduct of state . . ."

See Karl Dietrich Bracher, *The German Dictatorship: The Origin, Structure and Effects of National Socialism*, (New York: Praeger Publishers, 1970), p. 78.

[18] Riker, *The Theory of Political Coalitions*, pp. 77–89.

party that could be subtracted from the coalition and the cabinet's majority status sustained; the greater the number of extra parties and the greater the weight (legislative voting strength) of these parties, the more oversized a cabinet coalition is.

(b) *minimum winning status* (*mwc*).
Cabinets in this category are (1) of winning or majority status and (2) do not contain any party that could be subtracted and winning (or reliable majority) status sustained.

(c) *less than minimum winning status* (*<mwc*)
These cabinets do not possess a reliable parliamentary majority; the greater the number and weight of the parties that would be required to make the coalition of minimum winning status, the greater the deviation of these cabinets from minimum winning status.

The *x* axis in this graph represents the generalized *a priori* willingness of parties to bargain among themselves. In conditions of extremely low constraints, every party is willing to bargain with every other party; in conditions of extremely high constraints, no party is willing to bargain with any other party.

The checkered space in Figure 1 specifies the relationship between cabinet coalitional status and the willingness of parties to bargain. Oversized (*>mwc*) cabinets form in conditions of low to moderate bargaining constraints. Undersized (*<mwc*) cabinets form in conditions of moderate to high bargaining constraints. Minimum winning cabinets form in a wide range of bargaining conditions: the main factor (from the standpoint of bargaining constraints) will be whether there is at least one majority set of parties that are willing to bargain. A great deal of variation in cabinet coalitional status cannot be explained by variation in the generalized *a priori* willingness of parties to bargain. The willingness of parties to bargain determines the general range in which the coalitional status of a cabinet should fall. Within this range the coalitional status of the cabinet that forms can vary; the critical element in this variation is the degree of information certainty that exists within the parliamentary bargaining arena.

Cabinet Coalitional Status and Information Certainty. In parliaments that experience low to moderate constraints on party bargaining, variation in information certainty produces variation between minimum winning and oversized cabinet status. If parties can estimate precisely the strength of all other parties and the moves being made by all other parties, they will act to form minimum winning coalitions. If parties are unsure of the reliable votes possessed by each other, however, or if they are unsure of each other's reliability as coalition partners, then parties will compensate for this uncertainty by entering into

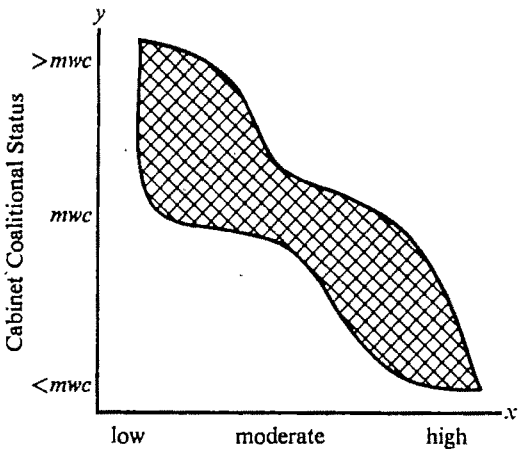


Figure 1. The Predicted Relationship Between the Willingness of Parties to Bargain and Cabinet Coalitional Status

oversized coalitions. A coalition of parties will form that is larger than would appear nominally necessary to the coalition's majority status. The greater the information uncertainty, the more oversized these coalitions will be.

By contrast, in parliaments experiencing moderate to high constraints in party bargaining, information uncertainty creates or augments a tendency toward undersized cabinets. This follows if we consider the nature of calculations and negotiations in such parliaments. On the one hand, parties want to enter minimum winning coalitions. When minimum winning cabinets are not possible because of the constrained nature of party bargaining, parties will seek to be included in the largest possible coalition. In this way they maximize the security of the cabinet by making it as difficult as possible for a larger coalition to arise. On the other hand, in highly constrained settings parties have *a priori* reasons to avoid serving in a cabinet with certain specific parties (say party A), while they may be more willing to serve with other parties (say party B). Party B and party C may or may not be willing to serve in cabinets together. Faced with a situation like this, complete information as to weights (reliable legislative votes) is needed if parties are to calculate the size of potential coalitions; perfect information about prior moves is needed by parties to guard against the possibility of being drawn into a cabinet with an undesirable partner.

In a situation in which information certainty is possessed by parties, each party will realize that it cannot secretly maneuver another party into an undesirable coalition. In addition parties can

clearly calculate which party coalitions are possible and desirable. Consequently, within the coalitional status range predetermined by the willingness of parties to bargain, minimum winning status will be approached as information certainty is approached. As information uncertainty increases in a parliament experiencing moderate to high constraints on party bargaining, parties will tend to form smaller cabinets than would be maximally desirable within the existing constraints. In such a situation, because of lack of perfect information, each party (A) will be hesitant to bargain with otherwise acceptable parties (B) out of fear that, in order to create a minimum winning coalition, their acceptable parties may attempt to maneuver party A into a coalition with an undesirable party (C). In addition, lack of certain information makes it difficult for parties to calculate reliably which coalition would be of the maximally desirable size; calculations and negotiations would thus take more time, and under the time constraints that exist on parliamentary bargaining, parties would be forced to settle for coalitions that might be suboptimal rather than enter coalitions that might be undesirable for *a priori* reasons.

The seemingly paradoxical conclusion of the foregoing analysis is that an increase in information uncertainty can give rise to both oversized and undersized coalitions. Parties should be able to calculate minimum winning coalitions only in conditions of low information uncertainty. As information uncertainty increases, the ability of parties to calculate reliably in coalition maneuvering decreases. The type of cabinet that forms should depart from minimum winning status, with the *degree* of that departure being at least partially a function of the degree of information uncertainty. The *direction* of cabinet deviation from minimum winning status is dependent on the bargaining constraints that exist. In other words, as conditions depart from information certainty we can only predict that the resulting cabinet coalitions should depart from minimum winning status. We cannot predict the direction of the departure unless we know the nature of the *a priori* constraints that exist within parliament. If those constraints are fairly low, oversized coalitions should form as parties attempt to compensate for uncertainty. If those constraints are fairly high, undersized coalitions should form as parties act to avoid being in coalitions with undesirable parties.

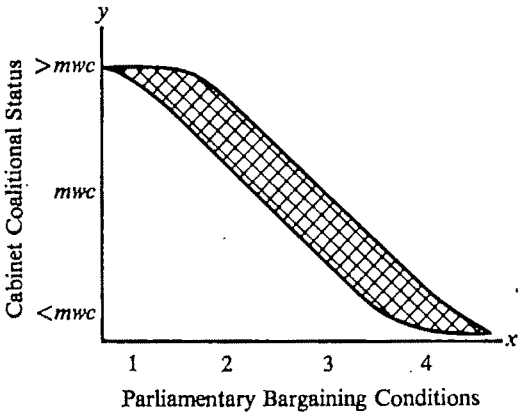
Cabinet Coalitional Status and Parliamentary Bargaining Conditions. The theme of the foregoing argument is that the coalitional status of cabinets is determined by the interaction of two bargaining conditions: (1) the *a priori* willingness of parties to bargain and (2) information certainty. This interaction is fairly difficult to state or to capture precisely. In the long run, as the model constructed here is modified and expanded in a more rigorously deductive manner, it should prove possible to predict precisely the coalitional status of cabinets under different interactive conditions. For the present, Figure 2 presents the general predicted pattern. The x-axis in this figure represents the bargaining conditions that exist in parliament. The y-axis indicates the coalitional status of the cabinet that forms under the specified bargaining conditions. As Figure 2 indicates, oversized coalitions will form if the parties are fairly unconstrained in their willingness to bargain and are uncertain in their bargaining information. Minimum winning coalitions will form under a wide range of *a priori* bargaining constraints as long as parties possess reasonably certain bargaining information. Undersized cabinets will form if parties are fairly constrained in their willingness to bargain and if parties are fairly uncertain as to the reliability of their bargaining information. At the extreme ends, the relationship may assume curvilinear characteristics since coalitions cannot increase or decrease in size beyond certain absolute limits so that increases in information uncertainty beyond certain threshold limits will only reinforce (rather than augment further) the tendency toward oversized or undersized cabinets. The curvilinear characteristic of the relationship is further aggravated by the possibility that under conditions of high unwillingness to bargain, the constraints may be so great that only a one party minority government is possible. Under such conditions, increases in information uncertainty will only reinforce the existence of a minority cabinet rather than activate such a tendency.

The Durability of Cabinets. The quest for governmental power does not cease with the formation of a cabinet. Parties outside of the cabinet still desire cabinet status. Parties within the cabinet may be unhappy about their ministerial payoffs. Consequently, there should be continued challenges to the composition of a cabinet both from parties outside and from parties inside the cabinet. The nature and success of these challenges will depend on the type of cabinet coalition that has formed. In particular, the minimum winning status of cabinets is vital. If a cabinet is of minimum winning status, it is in a relatively ideal position. On the one hand, it controls a majority of parliament's votes: unlike a minority cabinet it is not at the mercy of a hostile parliament that it cannot control. The cabinet should be able to meet any threat that comes solely from parties outside of the cabinet. On the other hand, no party in the cabinet could be removed without

impairing the cabinet's majority status. The significance of these factors can be clearly seen if we compare minimum winning cabinets to cabinets that are not of minimum winning status.

Consider first a comparison between minimum winning and oversized coalitions. If a cabinet is of minimum winning size, by definition no unnecessary party is sharing in the ministerial payoffs. By contrast, in an oversized cabinet at least one party is sharing ministerial status that was in reality unnecessary to attaining the ministerial payoffs. At least one party could be removed from the cabinet without destroying the reliable majority of the cabinet. The unnecessary status of the party(s) was not clear in the coalition formation stage because of information uncertainty. Once the cabinet is formed, however, it becomes obvious which parties were in on the final bargain and what the relative voting strength of these parties is. It thus becomes obvious that at least one party is expendable. If this party is omitted from the cabinet, some or all of the other coalition partners will gain in the sense that they will obtain new ministerial positions formerly controlled by the expendable party(s). Realizing this, the various parties within the coalition act to reduce the size of the cabinet by removing at least one party. Similar action will not be taken in a minimum winning cabinet because there is no expendable party among the coalition partners. Consequently, in comparing minimum winning cabinets to greater than minimum winning cabinets, minimum winning cabinets should be more durable. In addition, the more "extra" parties there are in a coalition and the greater the size of these parties, *ceteris paribus*, the smaller is the proportion of significant ministerial seats controlled by each cabinet party relative to the proportion it would control in a minimum winning cabinet. It is this relative deprivation that is important. The greater the deprivation parties suffer relative to a potential minimum winning size, the more quickly these parties should act to remove the deprivation: in the context of this study, the more oversized a cabinet coalition, the more rapidly parties will act to reduce the coalition toward minimum winning size. The more oversized the cabinet coalition is, the shorter the durability of the cabinet coalition should be.

A similar pattern occurs when we compare minimum winning cabinets with cabinets of less than minimum winning status. In a minority cabinet, the party or parties in the cabinet do not possess a reliable majority of legislative votes. The cabinet thus exists in a hostile environment that it cannot control. Because of information uncertainty, the hesitancy of various parliamentary parties to bargain among themselves, and time constraints on coalition negotiations, the minority cabinet was installed. This cabinet is faced constantly with the possibility that while it attempts to govern, other parties are negotiating their differences so that they can overthrow the cabinet and attain ministerial status, or at the least force the dissolution of parliament and new elections. In fact, the parties within the cabinet may themselves be negotiating with parties outside of the cabinet so as to form a larger and more secure coalition. Similar pressures do not exist for minimum winning coalitions. Consequently, minimum winning coalitions should be more durable than minority cabinets. In addition, the more a minority cabinet departs from minimum winning status (that is, the greater the number and size of the parties that it would take to transform the cabinet to minimum winning status), the more vulnerable the cabinet is to the rise of a larger coalition. Among minority cabinets, cabinet durability should be a function of the degree to which the cabinet departs from minimum winning status.



Code to Parliamentary Bargaining Conditions:

1 = low constraints on bargaining and high information uncertainty
2 = low to moderate constraints and low to moderate uncertainty
3 = moderate to high constraints and low to moderate uncertainty
4 = high constraints on bargaining and high uncertainty

Figure 2. The Predicted Relationship Between Cabinet Coalitional Status and the Interaction of Parliamentary Bargaining Conditions

An Empirical Interpretation

The thesis developed thus far can be summarized as follows: cabinet durability is a function of cabinet coalitional status; in turn, cabinet coalitional status is a function of the bargaining conditions that exist within parliament. The critical problem is to interpret the thesis in a justifiable

and useful manner that maintains the power of the theory while joining it to interesting and empirically observable phenomena.[19] The most difficult aspect of the theory to interpret is information certainty and the *a priori* willingness of parties to bargain. These conditions are attitudinal phenomena that, in the most rigorous sense, require us to focus on an attitudinal interpretation of the theory. This would necessitate survey data that are obviously unavailable for sufficient time periods to allow serious analysis. In addition, an attitudinal focus—while indicating the most immediate determinants of cabinet coalitional status—does not isolate those general structural variables that are of greater interest in the cross-national analysis of parliaments. In lieu of an attitudinal focus, the interpretive approach that I propose is to focus on party system characteristics as indicators of parliamentary bargaining conditions. My basic argument is that the parliamentary party system is among the determinants of parliamentary bargaining conditions. Since the latter cannot be measured directly, given current resources, the theory must be expanded an additional causal link, employing the party system referents to test the hypotheses presented earlier. This expansion is based on plausibility rather than determinative proof; the arguments need to be subjected to theoretical refinement and extensive empirical investigation. The current study serves to direct scholars in these directions and to present empirical patterns that can be studied to evaluate the probable relationships. The party system characteristics that I examine are fractionalization, instability, and cleavage conflict.

Parliamentary Party System Fractionalization. This refers to the number and relative strength of the parties within parliament. A highly fractionalized party system is one in which a large number of parties have seats in parliament and these parties are of relatively equal strength. The inference is that the greater the fractionalization of the parliamentary party system, the greater the uncertainty of bargaining within parliament, *ceteris paribus*. This inference is based on two considerations.

(1) The greater the fractionalization of the parliamentary party system, the greater the number of relevant party relations that must be observed in the coalition bargaining process. Any party attempting to follow the various possible bargaining moves is consequently faced with a difficult and, under highly fractionalized cases, virtually impossible task. Given this situation, as the number of relevant parties increases within parliament, a decrease occurs in the ability of parties (or party leaders) to follow or to feel that they are following all moves being made by the other parties in the bargaining arena. Consequently, *as party system fractionalization increases the perfection of information about moves should decrease.*

(2) The more parties that are relevant to the bargaining process, the more difficult it should be to estimate with certainty the reliable strength of the various parliamentary parties. This is true particularly if we assume that, in estimating relative strength of parties, any given party leader must observe the internal party relations of each party, or at least must feel that, if there were serious internal fights, he would be aware of them. In a parliament consisting of only three relevant parties, that might be true since a party leader would need follow only two parties. By contrast, in a parliament of ten relevant parties, the party leader must follow nine other parties in order to estimate their reliable strength. Consequently, *as parliamentary party system fractionalization increases, the completeness of information as to weights should decrease.*

As an operational indicator of parliamentary party system fractionalization, the following measure is employed:

$$F_t = \text{antilog}\left[-\sum_{I=1}^{m} P_I \cdot \log_e P_I\right]$$

where

m = number of parties in Parliament
P_I = proportion of parliamentary seats held by Party I in Parliament t.

This fractionalization (F_t) measure has two essential attributes: (1) it increases as the number of parties increases, and (2) it increases as the relative equality of the parties increases.[20] Because it re-

[19] For discussions of the nature of empirical interpretation see Robert T. Holt and John M. Richardson, "Competing Paradigms in Comparative Politics," *The Methodology of Comparative Research*, ed. Robert T. Holt and John E. Turner (New York: The Free Press, 1970), p. 25; Carl G. Hempel, *Aspects of Scientific Explanation* (New York: The Free Press, 1965), pp. 111–112, 130–133, 184–185; John G. Kemeny, *A Philosopher Looks at Science* (Princeton, N.J.: Van Nostrand and Co., Inc., 1959), pp. 134–138; Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), pp. 30–40. For a more general discussion of the relation of empirical and theoretical analysis in comparative politics see James A. Bill and Robert Hardgrave, *Comparative Politics: The Quest for Theory* (Columbus, Ohio: Charles E. Merrill Pub. Co., 1973), pp. 1–41. An excellent example of theory interpretation and testing is contained in Norman Frolich, Joe Oppenheimer, Jeffrey Smith and Oran Young, "Rationality and Voting," a paper prepared for the Public Choice Society Meetings, New Haven, Connecticut, March 21–23, 1974.

[20] See John K. Wildgen, "The Measurement of Hyperfractionalization," *Comparative Political Studies*, 4 (July, 1971), 233–243. This measure is associated

flects not only the number of parties but the relative strength of the parties, the index is a fairly good indicator of the number of relevant parties in the bargaining process. This property follows if we assume that, the more parties are of equal size, the less likely any party is either (1) to hold a pivotal position or (2) to be totally unimportant for coalition formation. In other words, the more parties are of equal legislative size, the greater likelihood that they are equally relevant in the coalition bargaining process. It should be noted that the fractionalization index employed here is not the one employed by Douglas Rae or Taylor and Herman. As Wildgren has shown, their fractionalization measure does not increase linearly with an increase in the number of parties in a party system, *ceteris paribus*. Rather, it fails to distinguish fractionalization scores clearly for party systems beyond that point where four to five parties exist.[21] This study deals with some systems that have as many as ten or twelve parties; the measure of fractionalization employed here provides the necessary differentiation.

Parliamentary Party System Stability. This refers to the continuity within parliament over a given set of years in the relative strength of the parliamentary parties. For the purposes of this study the following operational definition has been employed:[22]

a. To measure the continuity of the party system in Parliament t with the party system in Parliament $t-1$, the following index was used:

$$C_t = \frac{1}{2}\sum_{I=1}^{M} \left| L_{I_{t-1}} - L_{I_t} \right|$$

where

$$L_{I_{t-1}} = \frac{\text{number of legislators for Party } I \text{ at } t-1}{\text{number of legislators in Parliament } t-1}$$

$$L_{I_t} = \frac{\text{number of legislators for Party } I \text{ at } t}{\text{number of legislators in Parliament } t}$$

m = the number of parties in Parliament. Any party in parliament at $t-1$ or t is treated as a party in paliament during both time periods.

b. To determine the stability of the party system for any parliament t, the C (continuity) scores for all new parliaments held in the past five years, including parliament t, were calculated and the scores were summed. This sum is our Party System Stability (S_t) score.

Party system stability as measured above is an indicator of information certainty in the parliamentary bargaining process because:

(1) The greater the continuity of the parliamentary party system, the greater the probability that a stable partisan voting pattern will develop and the greater the probability that these patterns will be recognized by the leaders of the various parties. In other words, *as the stability of the parliamentary party system increases, the completeness of information as to weights (i.e., legislative votes) should increase.*

(2) As the stability of the parliamentary party system increases party leaders become more familiar with one another and with the behavior of the parties. Consequently it should be easier for them to anticipate and follow the various moves being made in the bargaining process. In other words, *as parliamentary party system stability increases, complete information about prior moves should increase.*

The overall inference is that as parliamentary party system stability increases, information certainty increases.

Interaction of Fractionalization and Instability. Special combinations of the two variables may affect information uncertainty in a manner not predictable from knowledge of the effects of each variable taken separately. For example:

(1) Behavior among a large number of parties may not be difficult to follow if these parties have been repeatedly represented in parliaments over a significant time period and if their strength has been fairly consistent so that prior behavior serves to guide current expectations.

(2) Discontinuity over time in the party system may not be a serious problem in less fractionalized systems since only a few parties must be followed.

(3) By contrast, the joint occurrence of high fractionalization and high instability together may activate or at least augment the potential effects of each: party leaders must follow manuevers among a large number of parties with little past behavior to rely on to delineate or predict parliamentary

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with information theory, particularly the concept of entropy or, as Galtung presents it, "uncertainty." See Claude E. Shannon and Warren Weaver, *The Mathematical Theory of Communication* (Urbana: University of Illinois Press, 1959); John Galtung, *Theory and Methods of Social Research* (New York: Columbia University Press, 1967); Henri Theil, *Economics and Information Theory* (Chicago: Rand McNally, 1967).

[21] See Douglas Rae, *The Political Consequences of Electoral Laws* (New Haven: Yale University Press, 1967); Taylor and Herman, "Party Systems and Governmental Stability"; Wildgen, "The Measurement of Hyperfractionalization," p. 236. For other approaches see Richard A. Pride, "Origins of Democracy," in *Sage Professional Papers in Comparative Politics*, ed. Harry Eckstein and Ted Robert Gurr, 1 (No. 12, 1970), p. 708; Lawrence S. Mayer, "An Analysis of Measures of Crosscutting and Fragmentation," *Comparative Politics*, 4 (April, 1972).

[22] This index is a variation on Przeworski and Sprague's measure of Party System Institutionalization. See Adam Przeworski and John Sprague, "Concepts in Search of Explicit Formulation: A Study in Measurement," *Midwest Journal of Political Science*, 15 (May, 1971), 183–218.

moves; stable partisan patterns do not exist to depress the complications created by a large number of parties, and *vice versa*.

If these projections are correct, the joint occurrence of high fractionalization and instability may produce an additional degree of information uncertainty that can not be predicted from knowledge of the effects of either variable examined separately or in an additive model. This relationship can be perceived as a multiplicative one. To test for its significance the fractionalization and instability scores for each parliament have been multiplied together creating a third variable that will be referred to as the $F_t \times S_t$ index.

Cleavage Conflict. The existing literature on Western party systems indicates that voters tend to align with parties according to the salient societal cleavages with which the parties are identified. Within parliament the parties represent the various cleavage constituencies by which they are supported. There are two vital elements to this representative function: (1) parties work to attain ministerial status so as to formulate, enact, and administer legislation that is favorable to their cleavage constituencies; (2) parties act in a manner that advances their constituency rather than another constituency, so there are limits on the bargains that they can strike in attaining power.[23]

Because of these two dimensions of representation, the cleavage system is both a major source of the quest for power and, at the same time, a major constraint on the behavior that is possible in the quest. In the party negotiations to form cabinets, parties must be willing to compromise on some cleavage related policies in order to join in a majority coalition and gain cabinet status. Some compromise can be justified if the cabinet positions gained thereby allow the party to largely satisfy the policy demands of its cleavage constituency. The greater the compromise required to enter a coalition, however, the more difficult the compromise is to justify. Ministerial status may be achieved but at the expense of the cleavage constituency, jeopardizing the party's future electoral success. This places a constraint on the willingness of party leaders to bargain: the greater the cleavage conflict between parties, the greater the constraints on *a priori* willingness to bargain.

Based on this perspective, party system cleavage conflict is used as an indicator of the *a priori* willingness to bargain.[24] As a rough measure of party system conflict, the degree of cleavage conflict index (*DCC*) has been employed:

$$DCC = \frac{\sum_{i=1}^{N_C} V_{C_i}}{N_C},$$

where

N_C = the number of salient cleavages along which parties are distributed in a given parliament as determined by a reading of the relevant literature for each country,

V_{C_i} = the variance of party positions on cleavage i in the given parliament and

$V_C = \sum f_j\%(X_j - \bar{X})^2$,

given that

X_j = an evaluative position given by the author to party j on cleavage i in the parliament being examined,

$\bar{X}$ = the mean of the X_j's,

$f\%$ = the percentage of legislative seats associated with party j in the parliament.

This index measures the average variance of parties along those cleavage dimensions that were salient for a given parliament.[25] The variance measure weights a party's position by the number of legislative seats associated with it. Low conflict scores indicate that the major parties cluster

[23] See Seymour Martin Lipset and Stein Rokkan, "Cleavage Structures, Party Systems, and Voter Alignments: An Introduction," in *Party Systems and Voter Alignments: Cross-National Perspectives*, ed. Lipset and Rokkan (New York: The Free Press, 1967); Seymour Martin Lipset, "Political Cleavages in 'Developed' and 'Emerging' Polities," in *Mass Politics: Studies in Political Sociology*, ed. Erik Allardt and Stein Rokkan (New York: The Free Press, 1970); Middleton, *French Political System*, pp. 62–133.

[24] Similar interpretations are reflected in Bracher, *The German Dictatorship;* Groennings, "Notes Toward Theories of Coalition Behavior in Multiparty Systems," pp. 445–465; Sartori, "European Political Parties" in LaPalombara and Weiner, *Political Parties and Political Development.*

[25] This index is similar to the one used by Taylor and Herman in "Party Systems and Governmental Stability," p. 34. Unlike Taylor and Herman, however, I have employed more than one cleavage dimension in my calculations. I do not maintain this measure is an optimal one. An exhaustive treatment of cleavage systems as constraints on party bargaining should include such elements as (1) the intensity of party conflict on cleavage dimensions; (2) the degree to which parties share similar relative positions on various cleavage dimensions (in other words, are cleavages cumulative or cross-cutting); (3) differences between parties' intensity and their perceptions of cleavage conflict. These factors as well as party positions on cleavage dimensions should be incorporated eventually into an Index of Cleavage Conflict. For the present, data and measurement problems inhibit this. On some of the problems see Douglas W. Rae and Michael Taylor, *The Analysis of Political Cleavages* (New Haven: Yale University Press, 1970).

closely together and thus, by inference, are willing to bargain with one another. High conflict scores indicate that the major parties are more widely dispersed and thus there should be a less generalized *a priori* willingness among them to bargain.

To determine which cleavages are salient in a given parliament and what the party positions are on these cleavage dimensions, the literature on cleavages and party systems was reviewed. The evaluations of the salient cleavages and the location of each party on these cleavage dimensions were examined by two scholars in the field and appropriate changes were made where necessary. Among the party system conflict scores, which range from 1.75 to 3.05, the largest scores were obtained by the First Austrian Republic, Denmark, Weimar Germany, and interwar Sweden, all of which had at least one parliament with a score above 2.90; the smallest scores were obtained by Australia, the Second Austrian Republic, Belgium, The Federal German Republic, and postwar Sweden, all of which had at least one parliament receive a score below 1.90. As a basis for evaluating the author's work, Table 3 presents the highest and lowest score obtained by a parliament for each country.

Table 3. Party System Conflict Scores

Country	Low Score	Year	High Score	Year
(1) Australia	1.85	1949+	2.09	1934
(2) Austria	1.85	1949+	3.05	1923
(3) Belgium	1.845	1925	2.46	1946
(4) Canada	2.12	1921	2.42	1962
(5) Denmark	1.91	1947	2.90	1945
(6) Finland	1.95	1966	2.73	1922
(7) France	2.04	1928	2.91	1956
(8) Germany	1.75	1965	2.92	1930
(9) Great Britain	2.33	1918	2.63	1923
(10) Iceland	2.23	1927	2.63	1959
(11) Ireland	1.91	1961	2.23	1951
(12) Italy	2.16	1946	2.53	1953
(13) Luxembourg	2.04	1968	2.56	1948
(14) Netherlands	2.00	1956	2.31	1933
(15) New Zealand	2.11	1931	2.36	1922
(16) Norway	2.13	1965	2.88	1924
(17) Sweden	1.83	1948	2.98	1920

Cabinet Coalitional Status. In this study, the following operational definition of cabinet coalitional status has been employed:

(1) The percentage of legislative seats held by each parliamentary party is determined.

(2) It is determined whether the parties in the cabinet hold a majority of the legislative seats within parliament.

(3) If the parties in the cabinet do hold a majority of the seats in parliament, *but* if the subtraction of any party from the cabinet would deprive the coalition of its majority status, the cabinet is termed a *minimum winning cabinet* (*mwc*).
   a. All such cabinets are given a score of zero.
   b. Since all parliaments with a majority party have been omitted from the analysis in this article, all minimum winning cabinets contain at least two parties.

(4) If the parties in the cabinet do not hold a majority of the seats in parliament, the cabinet is designated a *less than minimum winning cabinet* (*<mwc*). Such cabinets are also referred to as undersized or minority cabinets.
   a. Among less than minimum winning cabinets we are interested in determining the extent to which these cabinets deviate from minimum winning status.
   b. The following formula is used to determine the extent of this deviance: $<mwc = -D_{mwc} = N_a[\sum w_a]$.
      1. where w_a = the weight (percentage of legislative seats) of those parties whose addition to the cabinet would make it a minimum winning coalition. Since more than one set of such parties exists, choose that set whose $\sum_{w_a}$ is smallest, given that it would still produce a minimum winning cabinet.
      2. where N_a = the number of parties whose addition to the cabinet would produce the necessary $\sum_{w_a}$ to produce a cabinet of smallest minimum winning status. In case of ties choose that N_a which is smallest.

(5) If the parties in the cabinet hold a majority of the seats in parliament, and the subtraction of some party(s) will *not* deprive the coalition of its majority status. then the coalition is designated a *greater than minimum winning cabinet* (*>mwc*). Such cabinets are also referred to as oversized cabinets.
   a. Among greater than minimum winning cabinets, we are interested in determining the extent to which the cabinet deviates from (surpasses) minimum winning status.
   b. The following formula is used to determine the extent of this deviancy: $>mwc = +D_{mwc} = N_E[\sum w_E]$
   where w_E = the weight (percentage of legislative seats) of those parties that could be subtracted from the cabinet and its majority status maintained. Where more than one set of such extra parties exists, choose that set with the largest $\sum_{w_E}$; and where N_E = the number of parties that could be subtracted from the cabinet and its majority status maintained.

(6) Summary. These procedures give us an indicator of the coalitional status of cabinets for nonmajority party parliaments. A score of zero indicates that a cabinet is of *mw* status. A negative score indicates that a cabinet is of less than *mw* status. A positive score indicates that a cabinet is of greater than *mw* status. The larger the absolute score, the more a cabinet deviates from minimum winning status.

In measuring the deviation of cabinets from minimum winning status two dimensions were

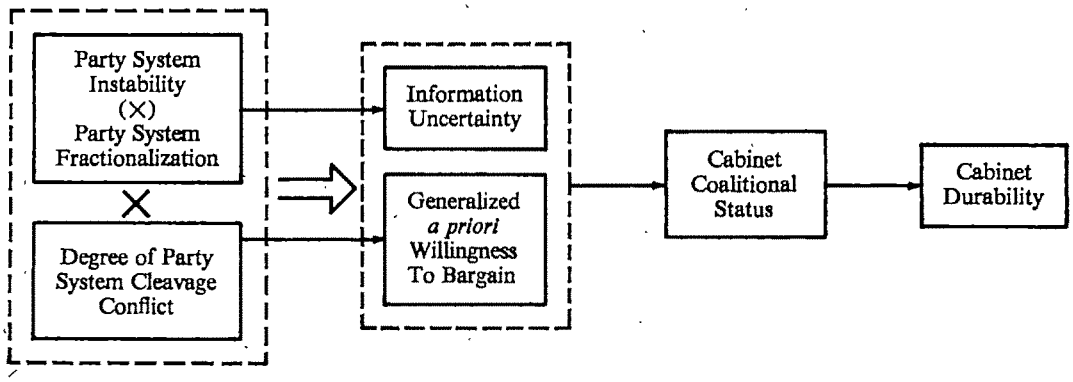


Figure 3

employed: the number of parties involved in the deviation and the overall weight of the parties involved in the deviation. This approach reflects the view that deviation is not just a question of weight but also of the number of parties.

Cabinet Durability. The final operational problem is delineating cabinet durability. Past scholars, notably Jean Blondel and Taylor-Herman, have stated that a cabinet exists so long as there is no change in the parties in the cabinet or in the Prime Minister.[26] For present purposes it is questionable whether this definition is useful. For example, does the death of a prime minister really constitute in any useful sense a change in cabinets if the new prime minister is of the same party and there is no other change in the cabinet? More important, definitions of concepts really depend on the theory within which one is working. In the present theory the key element is the coalitional behavior of parties. Because of this, the most consistent approach is to define cabinet durability solely in terms of the maintenance of the party coalition that exists within the cabinet. Thus the operational definition of cabinet durability is:

A cabinet exists so long as there is no change in the parties that compose the cabinet.

This definition is consistent with the theory stated in this study since the theory centers on the conditions under which party coalitions arise and on how durable different types of coalitions will be.[27]

[26] Blondel, "Party Systems and Patterns of Government," pp. 190–191; Taylor and Herman, "Party Systems and Governmental Stability," p. 29.

[27] This operational definition is not the only conceivable one that is consistent with the theory of cabinet durability outlined in this study. The key question is this: what constitutes a breakdown or change in a party coalition within the cabinet? A variety of answers can be given: (1) any alteration in the distribution of payoffs (ministerial seats) among parties could be viewed as a breakdown in the old coalition and the formation of a new one, even if the parties in the cabinet retained the same number of seats; (2) only "significant" alteration in payoff distribution could constitute a change; (3) only changes in the parties composing the cabinet could be viewed as a change in cabinet coalitional status. There is no easy way to choose among these approaches. My position is that the greater the number of defensible operational definitions that sustain the predictions of the theory, the more faith we can have in the theory. For the present, however, I am constrained by problems of data collection and verification. Consequently, in this study I have limited my definition to the third approach.

Party Systems and Cabinet Durability

According to the theory outlined in the first section and interpreted in the second, the coalitional status of a cabinet is determined by the nature of the bargaining conditions that exist in parliament. The characteristics of a parliament's party system constitute one set of factors determining the nature of these bargaining conditions. Consequently, a parliament's party system should influence the coalitional status of the cabinets that form in a parliament. Three key party system characteristics were posited to influence the bargaining conditions within a parliament. Including these three elements, the thesis can be diagrammed as in Figure 3. Certainly, the three party system characteristics specified in this figure are not the only factors that influence the bargaining conditions within a parliament. For example, information certainty may increase the longer a parliament lasts, the more structured the interaction of party leaders, the more frequent the sessions of parliament, or the more professionalized the legislature. Likewise, the willingness to bargain will not be a simple function of partisan cleavage conflict at the electoral stage but will be subject to shifts throughout a parliamentary session on the basis

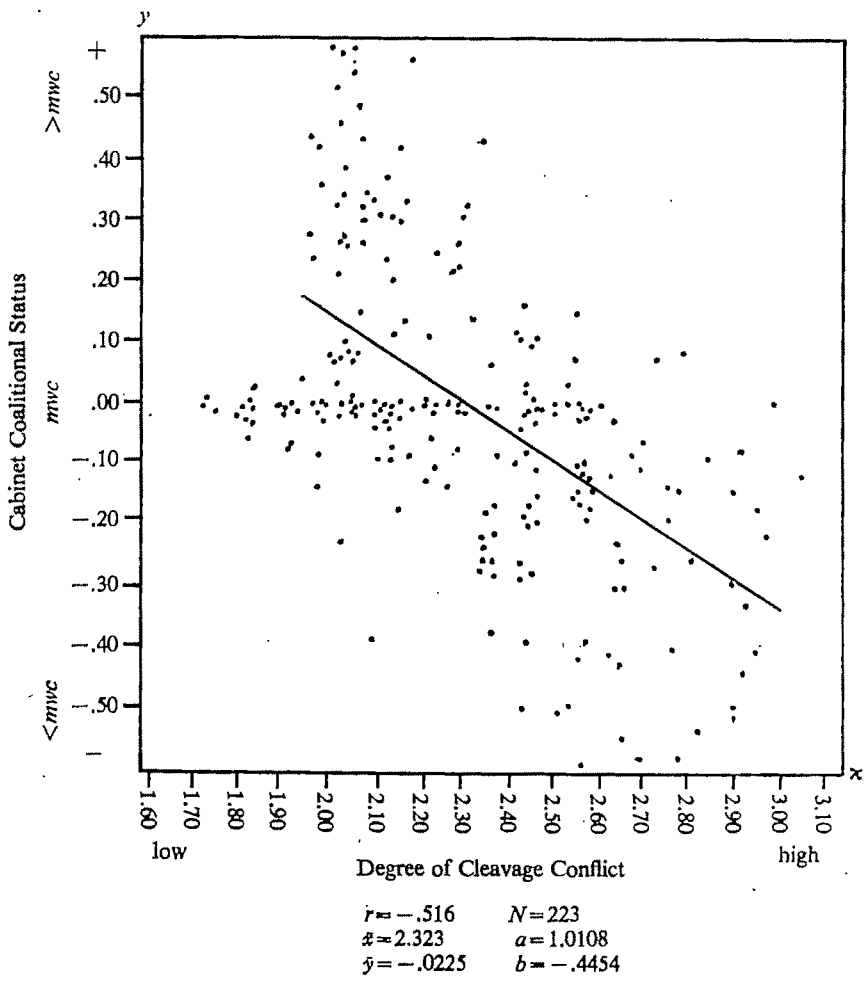


Figure 4. Covariation of Party System Conflict and Cabinet Coalitional Status

of external and internal events. Any attempt to predict or postdict cabinet coalitional status or cabinet durability on the basis of these three characteristics alone obviously will not account for all variations. In addition, the theory is difficult to test because of the complex interaction hypothesized between the various parliamentary bargaining conditions. The theory does not predict simple additive relations; rather, given different degrees of *a priori* willingness to bargain, information certainty may produce exactly the opposite behavioral patterns. This is a fairly difficult process to examine. The following sections present an exploratory test of the theory, but the conclusions must be seen as tentative, to be augmented through the application of other cross-national research methods, case study analyses, and experimental research in small group settings.

Party Systems and Cabinet Coalitional Status: Simple Relationships. The first test of the theory is to examine the simple relationships between party system characteristics and cabinet coalitional status. In these tests, cleavage conflict is viewed as an indicator of the generalized *a priori* willingness to bargain; the predicted relationship can be inferred from Figure 1. Party system fractionalization and instability are indicators of information uncertainty: in conditions of low fractionalization and low instability, minimum winning cabinets should form; as fractionalization and instability increase, cabinets should deviate from minimum winning status.

Figure 4 presents the pattern that obtains between party system conflict and cabinet coalitional status in multiparty parliaments, 1918–1972. This pattern indicates that there is a general

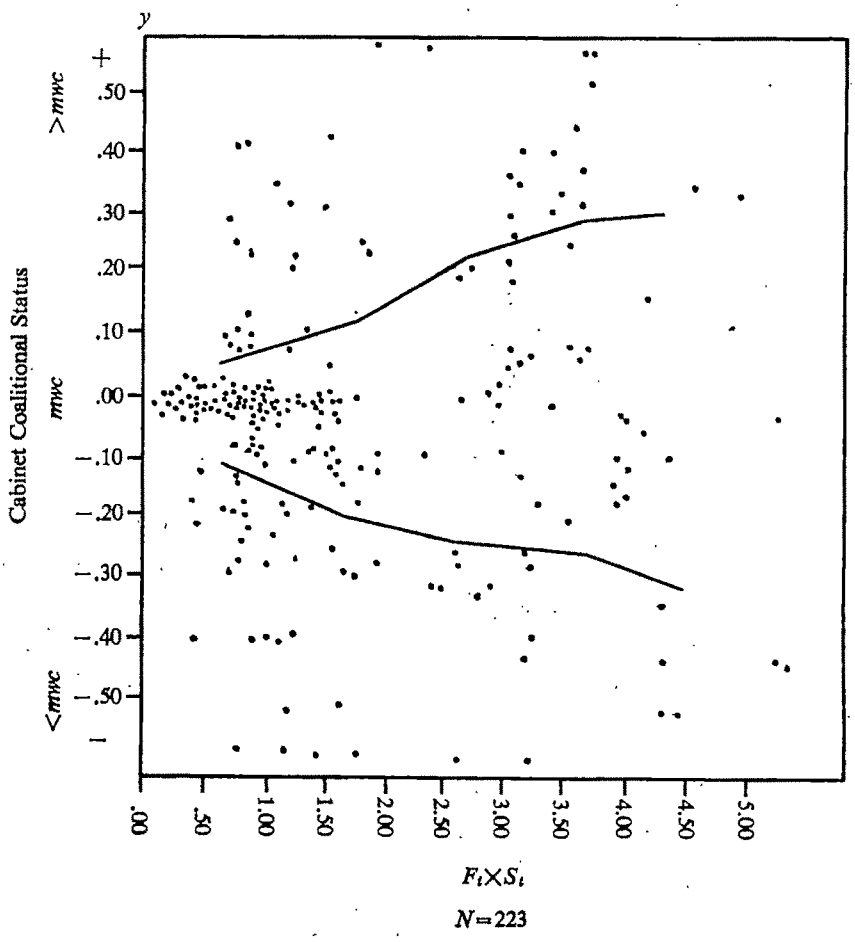


Figure 5. Covariation of $F_i \times S_i$ and Cabinet Coalitional Status

relationship between party system conflict and cabinet coalitional status. Oversized cabinets form predominantly in the area of low to moderate cleavage conflict. Undersized cabinets form primarily in areas of moderate to high cleavage conflict. Minimum winning cabinets form under virtually the entire range of cleavage conflict conditions although they predominate in the area of low to moderately high cleavage conflict. Overall, the correlation between party system conflict and cabinet coalitional status is −.516: cleavage conflict accounts for approximately 26 per cent of the variance in cabinet coalitional status.

Figure 5 indicates that there is a very general relationship between cabinet coalitional status and the interaction of fractionalization with party system instability. To analyze the linear relation between cabinet deviation and $F_i \times S_i$ we can drop the oversized-undersized designation, and find that a .353 correlation coefficient obtains. Similar patterns hold when we examine the statistical relationships between cabinet coalitional status and fractionalization or instability taken separately. The coefficients are .317 and .319 for fractionalization and instability, respectively, when correlated with the absolute deviation of cabinets from minimum winning status. Fractionalization, instability, and the interaction term all enter in a regression equation together at a statistically significant level. The multiple regression coefficient is .370, indicating that party system fractionalization, instability, and their interaction account for approximately 14 per cent of the absolute deviation of cabinets from minimum winning status.

These findings support the hypothesis that party system fractionalization and instability influence the deviation of cabinets from minimum winning status. Minimum winning cabinets do predominate in parliaments experiencing low party system fractionalization and instability, whereas deviant cabinets predominate in the more fractionalized and unstable party systems. Nevertheless, the

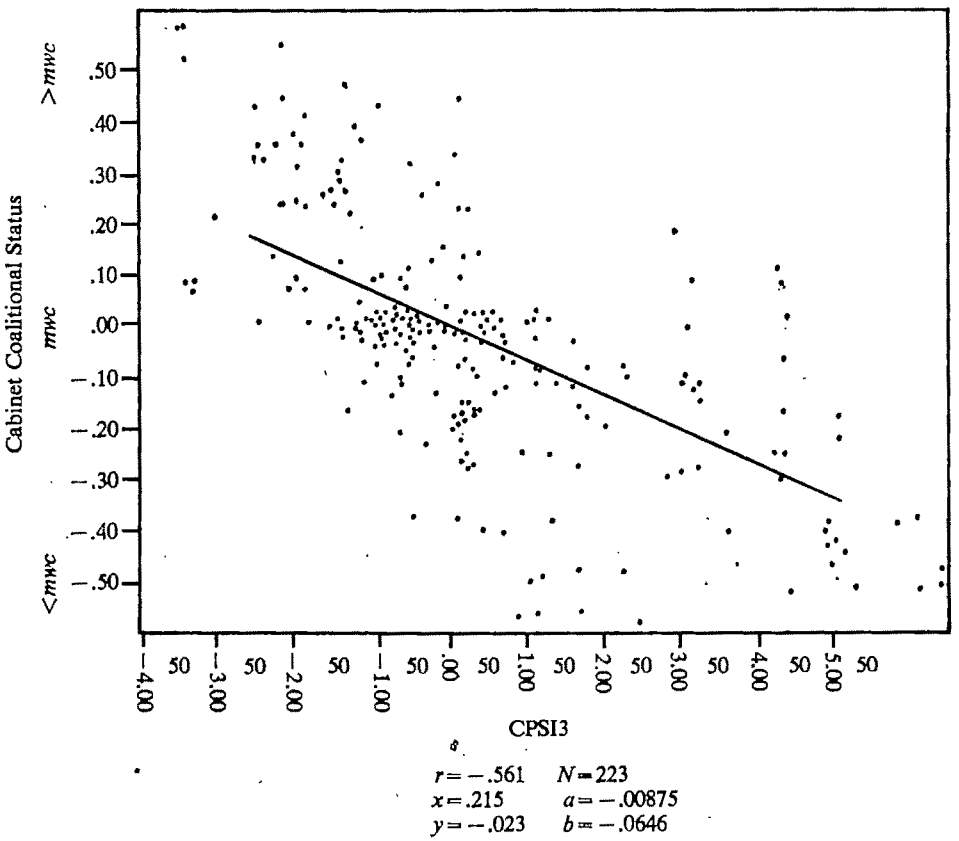


Figure 6. Covariation of Cabinet Coalition Status and CPSI3

analysis does not constitute a full test of the theory. It indicates nothing about the *direction* of cabinet deviation from minimum winning status and little about the *degree* of cabinet deviation. According to the theory, both the direction and degree are products of the complex interaction of fractionalization, instability, or both with party system conflict.

Party Systems and Cabinet Coalitional Status: Complex Relationships. The more complex predicted relationships between party system characteristics and cabinet coalitional status can be summarized as follows: at low levels of cleavage conflict, party system fractionalization and instability should be associated with oversized coalitions; as the degree of cleavage conflict between parties increases, this tendency should reverse and, at the higher levels of cleavage conflict, party system fractionalization and instability should be associated with undersized coalitions. To test for the relationship, the following strategy is employed: the party system cleavage scores are standardized—a procedure that does not change the strength of the relationship with cabinet coalitional status; it does, however, alter the nature of the cleavage conflict score: low scores are now negative and high conflict scores are now positive. These standardized scores have been multiplied with the unstandardized scores of party system fractionalization, instability, and $F_t \times S_t$ for each parliament. The resulting indices will be referred to as Complex Party System Interaction Indices: index 1 (CPSI1) represents the interactive score between party system conflict (*DCC*) and party system fractionalization; CPSI2 represents the interactive score between party system conflict and party system instability; CPSI3 represents the interaction of party system conflict with $F_t \times S_t$.

Large negative CPSI scores indicate parliamentary party systems that experience moderate to low cleavage conflict (thus the negative scores) and are highly fractionalized, unstable, or both, depending on the index employed. Using a variation on Sartori's terminology in order to characterize such parliaments,[28] these parliaments can be re-

[28] Sartori, "European Political Parties: The Case of Polarized Pluralism," pp. 137–140.

ferred to as experiencing unstable and *depolarized* hyperpluralism: the party system is unstable, basically moderate in terms of interparty conflict (i.e., major parties do not approach the ideological extremes), but highly fractionalized in number and strength of parties. In such a parliament, oversized cabinets are expected. France throughout much of the Third and Fourth Republics illustrates unstable, depolarized hyperpluralism.

Large positive CPSI scores indicate parliaments that experience moderate to high cleavage conflict between parliamentary parties (thus the positive scores) and are highly fractionalized, unstable, or both. For simplicity this condition can be referred to as unstable, *polarized* hyperpluralism: the parliamentary party system is unstable, highly fractionalized, and polarized with the major parties dispersed toward extreme positions on the salient cleavage dimensions. In such systems, undersized coalitions are expected. France from 1956 to 1958 and Weimar Germany throughout much of its existence illustrate unstable polarized hyperpluralism.

The intermediate CPSI scores indicate parliaments that experience lower degrees of party system fractionalization or instability: parliaments that obtain low but positive scores possess more polarized party systems; parliaments that obtain low but negative scores possess less polarized systems. The former can be referred to as experiencing stable, *polarized* pluralism; the latter experience is stable *depolarized* pluralism. In such parliaments, minimum winning cabinets are expected. First Republic Austria during the early 1920s illustrates stable polarized pluralism. Recent experience in West Germany illustrates stable *depolarized* pluralism.

The appropriate interaction terms have been calculated for all multiparty parliaments, 1918–1972. As an example of the resulting relationship, Figure 6 presents the covariation of cabinet coalitional status and CPSI3. In terms of its general form as predicted, the relationship is curvilinear, although the curvilinear characteristic obtains clearly only in conditions of high cleavage conflict. To capture this curvilinear form employing linear statistics the CPSI3 scores have been logarithmically transformed; the resulting product moment coefficient is −.626.

Similar patterns obtain for the covariation between cabinet coalitional status and both CPSI1 and CPSI2; the product moment coefficient associated with each independent variable (logged) is −.580 and −.614, respectively. The multiple regression coefficient obtaining when these two variables are entered in an additive model is .620. The inclusion of CPSI3 (logged in a regression equation with CPSI1 (logged) and CPSI2 (logged) raises the coefficient to .632 with all three variables entering the equation at a statistically significant level (.01). Neither DCC nor DCC (logged) will enter the regression equation at the .01 level. These findings suggest three conclusions:

(1) As we move from the simple relationship between cleavage conflict and coalitional status to the more complex interactive patterns suggested by the theory, we observe a 15 per cent increase in variance explained: overall the three CPSI variables (logged) account for approximately 40 per cent of the variance in cabinet coalitional status.
(2) It is the interactive occurrence of fractionalization and instability together, and their joint interaction with cleavage conflict that seems most influtial in determining cabinet coalitional status, with CPSI3 (logged) accounting for approximately 39 per cent of the variance in cabinet coalitional status.
(3) Once its complex interaction with party system fractionalization and instability is considered, cleavage conflict has no significant independent effect on cabinet coalitional status.

These findings support the theory that the complex interaction among party system fractionalization, instability and cleavage conflict determines cabinet coalitional status. This interaction process can be captured in a very parsimonious yet powerful fashion through the use of one variable (CPSI3-logged). The substantive relationship can be summarized as follows: oversized cabinets form primarily in unstable, depolarized hyperpluralism; undersized cabinets form in conditions of unstable, polarized hyperpluralism; minimum winning cabinets form under conditions of stable, polarized pluralism and stable depolarized pluralism. Of course, a good deal of variance does remain unexplained within these general patterns. Given the nature of the theory, unexplained variance is to be expected: party system characteristics influence only the general range of parliamentary bargaining conditions that will prevail; within that range an ongoing process of bargaining, maneuvering and cabinet transformations may take place. The critical question therefore is whether the findings matter: is cabinet coalitional status related to cabinet durability?

Cabinet Coalitional Status and Cabinet Durability. Figure 7 presents the relationship that obtains between cabinet coalitional status and cabinet durability for the 17 parliaments during peacetime years, 1918–1972. As can be seen, the general aggregate predictions derived from the theory are supported by the data. Minimum winning cabinets are more durable than oversized or minority cabinets. In addition, the durability of cabinets is a function of the degree to which cabinets deviate from minimum winning status. This relationship is curvilinear. Employing w^2 (an

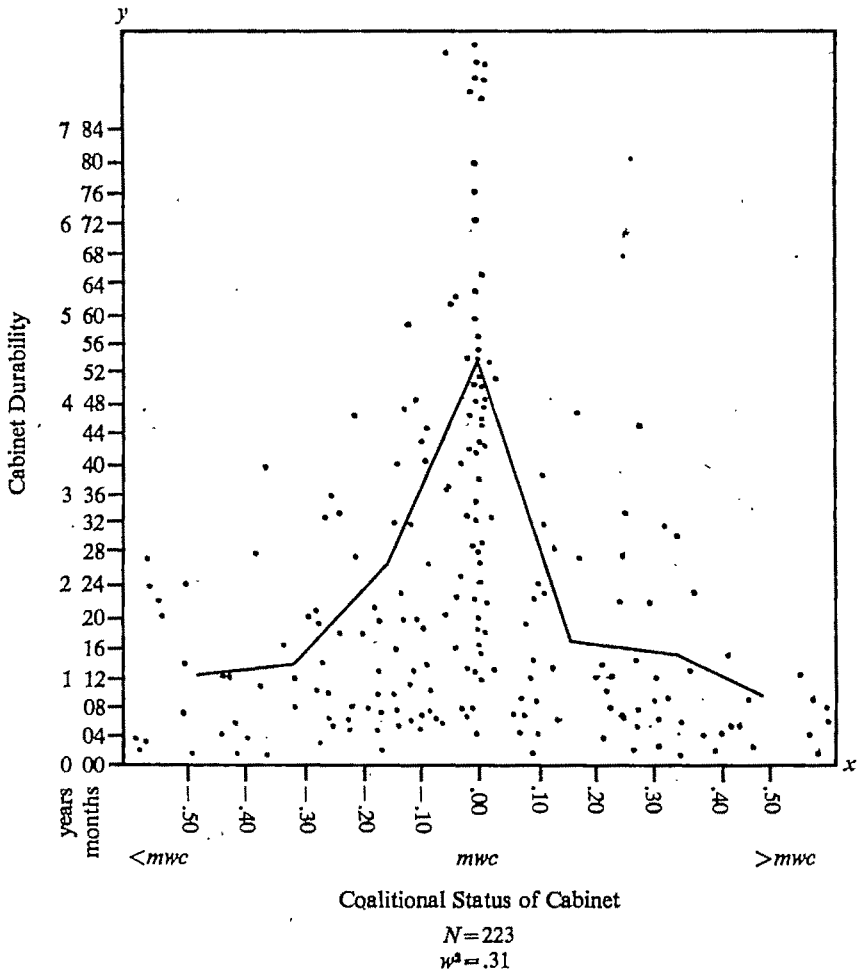


Figure 7. Covariation of Cabinet Durability and Cabinet Coalitional Status

analysis of variance statistic that indicates the percentage of variance accounted for in curvilinear relationships), cabinet coalitional status explains approximately 31 per cent of the variance in cabinet durability.

In order to examine the relationship employing linear statistics, I have collapsed Figure 7 and utilized the absolute deviation of cabinets from minimum winning status as the independent variable. In addition, cabinet durability has been logarithmically transformed. Using these procedures, cabinet coalitional status statistically explains approximately 24 per cent of the variance in cabinet durability (logged). A second procedure which can be employed to estimate the significance of this relationship with linear statistics is to transform the independent variable according to the following procedure: $X'=1/(1+x)$. If this transformation is conducted, cabinet coalitional status accounts for approximately 27 per cent of the variance in cabinet durability (logged). Judging from these various statistical procedures, we can conclude that cabinet coalitional status does account for a sizable proportion of the variance in cabinet durability. These findings support the hypothesis that cabinet durability is at least partially a function of cabinet coalitional status.

Party Systems and Cabinet Durability. Thus far we have seen that the coalitional status of cabinets is at least partially a function of the parliamentary party system, whereas cabinet durability is a function of cabinet coalitional status. It should follow, therefore, that cabinet durability is an indirect function of the parliamentary party system. Figure 8 presents the pattern that obtains between CPSI3 and cabinet durability. Employing w^2 analysis, we note that the complex interaction of party system fractionalization, instability, and

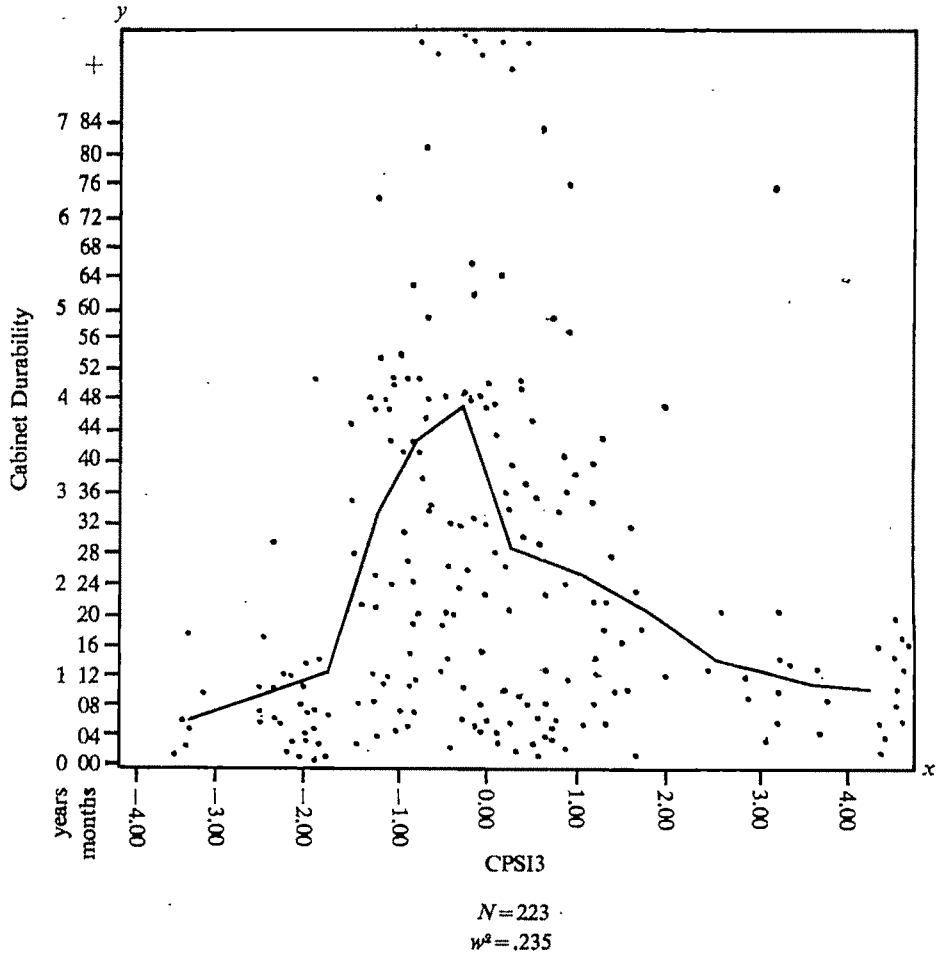


Figure 8. Covariation of Cabinet Durability and CPSI3.

cleavage conflict (CPSI3) explains 23 per cent of the variance in cabinet durability among peacetime multiparty parliaments, 1918–1972. When the CPSI3 (logged) scores are collapsed around the modal point in Figure 8, the product moment coefficient between cabinet durability and CPSI3 (logged) is −.4402. The similar product moment coefficients for CPSI1 (logged) and CPSI2 (logged) are −.293 and −.195 respectively. The multiple regression coefficient for these two independent variables in an additive model is .295. Adding CPSI3 (logged) to the regression equation raises the coefficient to .4602.

These findings indicate that cabinet durability is at least partially a function of the nature of the parliamentary party system. If multiple regression analysis is employed to estimate very roughly the strength of the overall relationship, the complex interaction of the party system characteristics accounts for approximately 22 per cent of the variance in cabinet durability (logged). More importantly, one variable alone (CPSI3 logged) accounts for approximately 20 per cent of the variance in cabinet durability. The substantive relationship can be summarized as follows: durable cabinets form under conditions of stable, depolarized pluralism and stable, polarized pluralism; transient cabinets form under conditions of unstable *depolarized* hyperpluralism and unstable, *polarized* hyperpluralism.

Clearly, the theory developed in earlier sections has yielded a parsimonious yet reasonably powerful statistical explanation of the relationship between cabinet durability and parliamentary party systems. This conclusion is particularly significant in light of three considerations: (1) the analysis here applies only to multiparty parliaments, a set of parliaments for which cabinet durability has been largely unexamined heretofore; (2) cabinet durability has not been averaged by parliament

and thus a good deal of unexplained variance is due to within-parliament variation in cabinet durability; (3) the timespan covered is quite wide and includes some deviant cases that could be justifiably omitted in the context of the theory but which, in the current study, I have included since exclusion would require a case-by-case discussion that cannot be attempted here. In addition, the findings are substantively significant because they can clarify our understanding of the failure of other theories to explain cabinet durability.

A "Nonobvious" Conclusion. In *Division and Cohesion in Democracy*, Harry Eckstein writes that

> the most stable democracy is one in which men disagree, without intensity and within a narrow range, on specific issues, widely share basic orientations, and have politically weak segmented identifications. The least stable case has the opposite characteristics: extensive and intense disagreements, flowing from highly divergent orientations and strong segmental cleavages.

In defining stable democracies, Eckstein maintains that they are polities "marked by what might be called intrasystem stability, especially the stability of governments." His illustrative index of governmental stability is the durability of cabinets.[29] Eckstein's position is the primary supplement in the literature to Lowell's thesis. It is an intriguing position and, indeed, it is the position that I held as I began my analysis of cabinet durability. Nothing could be more obviously true than the assertion that cabinet durability is a function of the degree of cleavage conflict or polarization that exists in a society or in a parliament.

Despite the plausibility of the hypothesis, systematic cross-national analysis does not appear to confirm it. Analyzing 19 Western nations, 1946–1970 (including majority party systems), Michael Taylor and V. H. Herman found only a slight relationship between partisan ideological differences and cabinet durability.[30] The present study reinforces their findings. The product moment correlation coefficient between cabinet durability (logged) and party system cleavage conflict (DCC) among multiparty parliaments, 1918–1940, 1945–1972 is only −.111. This does not change, furthermore, if we examine the relationship for curvilinear patterns: the ν^2 coefficient is .035, and the product moment correlation coefficient between cabinet durability and DCC (logged) is −.117. Clearly, the degree of cleavage conflict or party system polarization by itself does not greatly influence cabinet durability. Why has this "obvious" generalization been rejected?

The resolution of this dilemma lies, I believe, in the theory outlined in previous sections: the low relationship between cleavage conflict and cabinet durability is due to the nature of coalition formation and maintenance in multiparty parliaments. An essential determinant of cabinet durability is the coalitional status of the cabinet. A minimum winning cabinet will generally be durable; the more a cabinet deviates from minimum winning status, the less durable it will be. The formation of *deviant* cabinets is not a simple function of cleavage conflict. It depends on a complex interaction of cleavage conflict with party system fractionalization and instability. There is a difference in the *direction* of cabinet deviation under different cleavage conflict conditions. If the degree of cleavage conflict is low, cabinets in a fractionalized and unstable party system will deviate toward an oversized coalitional status. If cleavage conflict is high, cabinets in a fractionalized and unstable party system will deviate toward an undersized coalitional status. In *either* case, cabinets will tend to be transient.

Once we perceive the relationship in this manner, there is a sense in which Eckstein's general theory may be more plausible. A critical question is this: should the same meaning, interpretation or systemic consequences be attached to transient oversized cabinets and to transient minority cabinets? Are there potentially detrimental consequences that obtain from nondurable minority cabinets that do not accrue (or at least do not accrue as often or as immediately) from nondurable oversized cabinets? For example, in terms of partisan or policy continuity, the avoidance of disruptive elections, or the maintenance of widespread popular attachment to the cabinet ministry, it is at least conceivable that transient oversized cabinets are preferable to transient minority cabinets. If greater significance is attached to transient minority cabinets, then Eckstein's theory assumes a more defensible stance since minority cabinets form primarily in the more conflictual or polarized cleavage systems.

In addition, it is important to realize that the association between cleavage systems and cabinet durability is not just a question of the relationship of cleavage polarization to cabinet durability. A cleavage system has at least three separate characteristics: (1) the complexity of the cleavage system structure; (2) the institutionalization of cleavage structure attachments; (3) the degree of cleavage dimension conflict. The complexity of a cleavage system's structure refers to the number of cleavage dimensions that are salient in a polity and the degree to which these dimensions cut across one

[29] Harry Eckstein, *Division and Cohesion in Democracy: A Study of Norway* (Princeton, N.J.: Princeton University Press, 1966), pp. 38, 17–18.

[30] Taylor and Herman, "Party Systems and Governmental Stability," pp. 32–35.

another. The degree of cleavage dimension conflict refers to the intensity and extremity of electoral polarization on the salient cleavage dimensions. Cleavage structure institutionalization refers to the degree to which (1) the cleavage dimensions salient to an electorate are continuous overtime; (2) cleavage positions of the electorate on each cleavage dimension have solidified; and (3) these cleavage positions have been formalized through stable electoral attachments to specific political parties.

If we accept the proposition that a parliamentary party system is partially a product of a polity's cleavage system,[31] then the three cleavage characteristics specified above are factors that give rise to the party system characteristics analyzed in this study. Cleavage system complexity influences how fractionalized a party system will be. The extremity of electoral polarization on cleavage dimensions influences the degree of cleavage conflict that will characterize a parliamentary party system. Cleavage system institutionalization influences how stable the parliamentary party system will be. Interpreted in this manner, the findings presented in this study do suggest a relationship between cleavage systems and cabinet durability. Nondurable cabinets will exist in polities possessing a highly complex and uninstitutionalized cleavage system. If the cleavage system is highly conflictual or polarized the transient nature of cabinets will be due to a tendency toward minority cabinets. If the cleavage system is not highly conflictual, the transient nature of cabinets will be due to a tendency toward oversized cabinets.

Toward a General Theory

The purpose of this study has been to present a theory of cabinet formation and maintenance in multiparty parliaments and to present initial tests of hypotheses derived from the theory. The significance and underlying thesis of the theory is summarized best perhaps by the British parliamentary analyst, W. L. Middleton, writing in 1932:

> No political system can evade the problem of authority. It must be a principle aim of any constitution, even of the most democratic, to produce Governments which shall govern, which shall possess real initiative and command. . . . If a wide stretch of experience be reviewed it will be found that the power of the Executive . . . has varied very greatly from one legislature to another. . . . The explanation of these variations is not far to seek. . . . A ministry exercises its power . . . not directly, but through the majority to which it looks for support. If a Government has a confident and continuous command of its majority, it can be firm in its handling of the Chamber. It can be a Government which governs. If, on the contrary, it has only a loose hold over its majority, it is less able to impose respect for the proper attributes of the Executive. It is the variation in the efficiency of this machinery which accounts for the fluctuation of executive authority. Everything depends on the majority . . . on the degree of confidence in the Ministry which it possesses. . . .

And, as Middleton concludes, "where all majorities are coalitions, this means that everything depends on the nature of the coalition. The Executive is strongest when the Government is the trusted agent of a coalition of natural allies. . . ."[32] In a very real sense the theory and analysis of this study is an attempt to explicate, operationalize, and refine Middleton's insight. In the process, A. Lawrence Lowell's thesis is modified.

In his work, Lowell stressed the consequence of a cabinet's composition for cabinet durability. In particular, he argued that "A cabinet which depends for its existence on the votes of the Chamber can pursue a consistent policy with firmness and effect only when it can rely for support on a compact and faithful majority." Unfortunately, in making this argument and in investigating its consequences, Lowell was not working within a broader set of clearly and explicitly articulated assumptions about partisan motivations within parliament. He did not attempt to systematically explicate the meaning of "compact and faithful majority."[33] He did not clearly specify why one

[31] See Erik Allardt and Stein Rokkan, eds., *Mass Politics* (New York: The Free Press, 1970); Seymour M. Lipset and Stein Rokkan, eds., *Party Systems and Voter Alignments: Cross-National Perspectives* (New York: The Free Press, 1967); Stein Rokkan, *Citizens, Elections, Parties* (New York: David McKay Company, 1970); Robert A. Dahl, ed., *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966).

[32] Middleton, *French Political System*, pp. 151–154. For relevant discussions on the nature of cabinet governments and their significance for the regime stability of parliamentary democracies, see W. G. S. Adams, "Has Parliamentary Government Failed?," *The Modern State*, ed. Mary Adams (New York: The Century Company, 1933); Gerhard Loewenberg, "The Influence of Parliamentary Behavior on Regime Stability," *Comparative Politics*, 3 (Jan., 1971), 177–179; Ernest Fraenkel, "Historical Handicaps of German Parliamentarianism," *The Road to Dictatorship*, trans. Lawrence Wilson (London: Oswald Wolff, 1964), pp. 27–37; Karl Dietrich-Bracker, "Problems of Parliamentary Democracy in Europe," in *Comparative Legislative Systems: A Reader in Theory and Research*, ed. Herbert Hirsch and M. Donald Hancock (New York: The Free Press, 1971), pp. 343–359; William L. Shirlr, *The Collapse of the Third Republic* (New York: Simon and Schuster, 1969); Richard K. Ullman and Sir Stephen King-Hall, *German Parliaments: A Study of the Development of Representative Institutions in Germany* (New York: Frederick A. Praeger, 1954), esp. pp. 87–91; Philip M. Williams, *Crisis and Compromise: Politics in the Fourth Republic* (Hamden, Conn.: Archon Books, 1964).

[33] Lowell, *Governments and Parties*, 73.

type of cabinet composition might have a more faithful majority and the relationship of this faithfulness to the compact nature of the cabinet. He did not consider whether the conditions which would underlie the explanation of cabinet durability in single-party majority cabinets might have an analogous form in a multiparty system.

The linkage which Lowell missed, which I believe exists in the cabinet behavior of multiparty and majority-party systems, centers on the role of a cabinet's minimum winning status. In a multiparty setting, as I have emphasized, a minimum winning *coalition* of parties can form under certain conditions; if one does form, it probably will be quite durable. Analogously, a minimum winning cabinet can form in a majority party system. It will not be a *coalition* of parties. It will be a cabinet composed of one party. Nevertheless, it is of *minimum winning status* in the sense that (a) there is no unnecessary party in the cabinet and (b) the cabinet does control a majority of the seats in parliament. This perspective shows us a foundation for the construction of a general theory of cabinet formation and maintenance in parliamentary democracies. In the present study theory development has been limited to cabinet formation and maintenance in multiparty settings because the central concepts of the theory (such as bargaining conditions, information certainty, willingness to bargain, cabinet coalitional status) can be most clearly grasped in this more limited setting. It should be clear, however, that this focus in no way limits these concepts or the broader theory that they compose to a multiparty parliament.

Once the significance of minimum winning status is grasped, it is only a short step to discover that Lowell's thesis derives its power from having isolated within a majority party system a situation in which minimum winning cabinets are virtually guaranteed under normal conditions. In such an unfractionalized system, information certainty should be approached. Unless the majority party is unsure of its own voting cohesion (owing, for example, to a dramatically unstable party system), a single-party, majority cabinet will form. This cabinet will be of minimum winning status. The cabinet will not face internal dissension or disruption because of conflict between cabinet parties over ministerial payoffs to unnecessary parties; nor can the cabinet be overthrown by parties outside of the cabinet which coalesce to form a larger cabinet. The single party cabinet, as a minimum winning cabinet, will be quite durable.

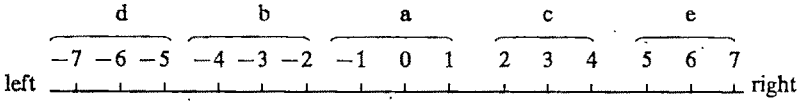
Seen in this context, the analysis of cabinet durability within majority party systems can be integrated into a larger theory along the form outlined in the first two sections. If we employ the concept of 'pluralism' to indicate parliaments experiencing low to moderate fractionalization and 'hyperpluralism' to indicate moderate to high fractionalization, we can summarize the propositions of a general theory as follows:

(1) In parliaments with party systems characterized by stable depolarized pluralism or stable polarized pluralism, minimum winning cabinets will form and be quite durable; the primary exception to this rule will be in extremely polarized multiparty parliaments where only minority cabinets are possible because of extreme interparty animosities; such cabinets will tend to be nondurable, falling primarily through the dissolution of parliament;
(2) In parliaments with party systems characterized by unstable depolarized hyperpluralism, oversized cabinets will form and be nondurable;
(3) In parliaments with party systems characterized by unstable, polarized hyperpluralism, undersized cabinets will form and be nondurable;
(4) A strong relationship between cabinet durability and partisan cleavage conflict or polarization should not exist: deviant (and thus nondurable) cabinets can form in polarized or depolarized party systems.

These propositions emphasize that durable cabinets can exist in multiparty settings as well as majority party settings. Lowell's generalization that cabinets in majority party systems will be durable can be subsumed in a more general theory of cabinet formation and maintenance. A key to durable government in peacetime parliamentary democracies is the minimum winning status of the cabinet; minimum winning cabinets can form in both majority party and multiparty parliaments.

Methodological Appendix

Index of Cleavage Conflict. In placing parties on the relevant cleavage dimensions, the following scale was used:



Positions on this scale were attributed to parties on the basis of the following guidelines:

(1) If the cleavage dimension related to basic policy dimensions such as economic intervention in the economy, secularization of the polity, etc., then the following meaning was imputed to the various categories of the model:

a—these are parties that are pro-system as it currently is, zero being the absolute center and −1

or +1 being parties that are center parties but that lean slightly toward the left (−) or toward right (+).

b—these parties want moderate restructuring of the social system in order to institute some new program in regard to cleavage *x*.

c—these are parties that are moderate in their opposition to change toward the left and in their opposition to the present system and hark back to some aspect of prior policies in search of the desirable policy on cleavage *x*.

d—these parties want fundamental, radical change of the social system in regard to cleavage *x*.

e—these parties are reactionary in opposition to leftist change and want to return to a societal position that they maintain realizes the traditions of the past.

(2) In some countries at certain times a critical salient cleavage is support of or opposition toward the political system itself. In those situations where this cleavage is salient the following interpretation is given.

a—those parties that accept the political system but very hesistantly

b—parties that moderately support the present system

c—parties that moderately oppose the present system

d—parties that are extremely supportive of the present system

e—parties that extremely oppose the present system

Within the various categories above, three different numerical positions can be given depending on whether a party falls clearly and precisely within the category designated or whether it leans slightly leftward or rightward within the given category.

My evaluations of party positions on the cleavage dimensions are contained in Dodd "Party Coalitions and Cabinet Government" (doctoral dissertation, University of Minnesota, 1972), Appendix A. After the publication of my evaluations in the dissertation, I became aware of similar efforts by two other political scientists, Kenneth Janda and John C. Thomas, both of whom have been kind enough to share their data with me. Janda analyzes party systems in 48 countries during the postwar years; Thomas analyzes party systems in 12 countries from the late 1800s to 1960. Between them the two studies cover 14 of the 17 countries in my study, all except Finland, Norway and Belgium. Both Janda and Thomas place parties on "issue" dimensions in which parties can receive scores ranging from −5 to +5. Among the issues they study are four economic issues, one issue related to clericalism, and two issues related to regime support.

Since the three most recurrent cleavages in the present study were conflicts over economics, clericalism, and regime support, the work by Janda and Thomas provides an independent basis by which to evaluate my scales. To obtain comparable codings, party positions on their four economic issues and on their two regime support dimensions have been averaged, creating one economic dimension and one regime support dimension. Their clericalism scale is used unaltered. The resulting product-moment coefficients are as follows:

		Interwar	Postwar	Total
Economic cleavage	Janda	*X*	.852 (32)	*X*
	Thomas	.810 (27)	.833 (25)	.814 (52)
Clerical cleavage	Janda	*X*	.800 (20)	*X*
	Thomas	.782 (12)	.779 (15)	.776 (27)
Regime Support cleavage	Janda	*X*	.954 (4)	*X*
	Thomas	.850 (8)	.800 (7)	.828 (15)

The parentheses indicate the number of parties on which the correlations are based. The numbers vary according to whether cleavages were deemed salient for a given country and according to whether complete data existed for given parties on the issue dimensions.

Considering that we used different coding specifications and different spatial dimensions, the product-moment coefficients are high, and these tests appear to provide a good basis for assuming the concurrent validity of our scales. For additional information on the coding procedures employed by Janda and Thomas see Kenneth Janda, *Comparative Political Parties: A Cross-National Handbook* (The Free Press, forthcoming), and John C. Thomas, "The Decline of Ideology in the West: A Longitudinal Study of the Changing Public Policy Orientations of Political Parties in 12 Nations" (Ph.D. dissertation, Northwestern University in progress). My thanks to Janda and Thomas for sharing their data with me.

Party System Stability Index. The PSS Index employed in this article is based on a sum of the *C* (continuity) scores for all new parliaments held in the preceding five years. Obviously, some other time span could have been selected. Because of this, all relevant hypotheses have been tested using the following alternate indices:

PSS-A—the stability score is the *C* score for parliament *t* only

PSS-B—the stability score is the sum of the *C* scores for parliaments t and $t-1$
PSS-C—the stability score is the sum of the *C* scores for parliaments t, $t-1$ and $t-2$
PSS-D—the stability score is the sum of the *C* scores for all parliaments held in the past five years
PSS-E—the stability score is the sum of the *C* scores for all parliaments held in the past ten years

The party system stability index employed in this article is PSS-D. The appropriate product moment coefficients for the other indices are reported in Dodd, *Party Coalitions and Cabinet Government*. All findings approximate those reported in this article.

Cabinet Coalitional Status. Other measures of cabinet coalitional status that are consistent with the theory could have been employed in this article. To examine the consequences of different measures, all relevant hypotheses have been tested using the following indices:

CCS1—cabinet coalitional status 1 (the index used in this study)
CCS2—cabinet coalitional status 2. The same procedures followed in CCS1 were maintained with these exceptions: (a) for less than minimum winning coalition, choose that $N_a \sum wa$ that is smallest; (b) for greater than minimum winning coalitions, choose that $N_a \sum w_a$ that is largest.
CCS3—cabinet coalitional status 3. The scores are based solely on $\sum wa$ or $\sum w_a$ without weighting by N_a or N_a. All other aspects of CCS1 are followed.

This study reports the results based on CCS1 but similar findings are approximated when either CCS2 or CCS3 are employed. See Dodd, *Party Coalitions and Cabinet Government*.

Of course, the above alternate operational definitions do not exhaust the possible alternate approaches to cabinet coalitional status. For example, Robert Axelrod has developed a very intriguing concept of minimal connected winning coalitions that moves coalitional analysis from a nominal to an ordinal level: coalitions must not only be of minimum winning status but also composed of parties that are adjacent on ideological or cleavage dimensions in terms of spatial ordering. His approach can be extended further to an interval level: coalitions must only be composed of that set of parties *closest* to one another on ideological or cleavage dimensions in terms of spatial distance; I refer to this as minimal winning compact coalitions. In this study neither minimal connectedness or minimal compactness has been investigated because such an approach would introduce a great deal of complexity to the data analysis considering the number of coalitions to be analyzed and the assumption that multiple cleavage dimensions will require connectedness or compactness at a multidimensional level. Second, by focusing on a nominal approach to minimum winning coalitions I am providing a baseline against which to compare future work that employs ordinal or interval approaches to coalitional status. For a fuller discussion of minimal connectedness see Axelrod, *Conflict of Interest*.

The Mediating Influence of State Legislatures on the Linkage Between Interparty Competition and Welfare Policies*

EDWARD G. CARMINES
State University of New York at Buffalo

Introduction

Does interparty competition significantly influence the level of welfare expenditures in the American states? From a theoretical perspective, this would seem to be a plausible contention. Essentially, students of state politics have pointed to two basic arguments as underscoring its theoretical plausibility. First, it is argued that in highly competitive situations political parties compete for support of all groups in the society. They do so by supporting public policies which are beneficial to these groups. Since welfare expenditures are directly relevant to the interests of lower socioeconomic groups, it follows that to gain the support of these groups, political parties should support the expenditure of public monies in this area. This is to be contrasted with noncompetitive situations in which the majority party does not have to be greatly concerned with the interests of lower socioeconomic groups.[1]

The second, complementary argument[2] is based on the following interrelated assumptions: (1) that the degree of interparty competition can be thought of as an index of the degree to which politics is organized in the states; (2) that organized politics "lessens the difficulty of lower status groups in sorting out political actors and issues, thereby enabling them to promote their own interests more effectively;"[3] and (3) that an important component of the interests of lower status groups is welfare expenditures.

The logic of these arguments leads to an important proposition: that interparty competition should be positively and significantly associated with welfare expenditures in the American states. But despite the plausibility of the proposition, much of the recent, empirical research in state politics has not supported it.[4] Rather, these studies have indicated that when socioeconomic factors are controlled, the initial relationship between interparty competition and welfare expenditures is greatly reduced and often disappears. These investigations have supported an alternative proposition: that interparty competition and welfare expenditures are both dependent on the level of the states' socioeconomic development and that any association between interparty competition and welfare expenditures is largely spurious.

But still other researchers, using different types of data and different data manipulation techniques, have concluded that there is a substantial link between interparty competition and welfare expenditures even after controls have been introduced for various socioeconomic factors.[5]

* This paper grew out of the author's participation in a graduate seminar at the State University of New York at Buffalo conducted by Kenneth N. Vines. In addition to Professor Vines, I would like to thank Roberta S. Sigel of Douglass College, Rutgers University, James A. Stimson of the State University of New York at Buffalo, and Richard A. Zeller of the State University of New York at Buffalo for useful comments on the paper. The paper has also benefited from many stimulating discussions on this general topic with John E. Sinclair of the University of Texas at Austin.

[1] For statements of this argument see, in particular, V. O. Key, Jr., *Southern Politics in State and Nation* (New York: Alfred A. Knopf, 1949), pp. 298–311; Duane Lockard, *New England State Politics* (Princeton: Princeton University Press, 1959), pp. 320–340; and Richard E. Dawson and Virginia Gray, "State Welfare Policies," in *Politics in the American States: A Comparative Analysis*, ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Company, 1971), p. 461.

[2] This argument is stated most succinctly in Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review*, 63 (September, 1969), 858.

[3] *Ibid.*

[4] See especially Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables and Welfare Policies in the American States," *Journal of Politics*, 25 (May, 1963), 265–289; Thomas R. Dye, *Politics, Economics and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally and Company, 1966), pp. 143, 144; and Richard I. Hofferbert, "The Relationship between Public Policy and Some Structural and Environmental Variables in the American States," *American Political Science Review*, 60 (March, 1966), 73–82. For an analysis which uses a more extended time period and comes to essentially the same conclusion see Richard E. Dawson, "Social Development, Party Competition, and Policy," in *The American Party Systems: Stages of Political Development*, ed. William Nisbet Chambers and Walter Dean Burnham (New York: Oxford University Press, 1967), pp. 203–237.

[5] See, in particular, Ira Sharkansky, *Spending in the American States* (Chicago: Rand McNally, 1968); Ira Sharkansky and Richard I. Hofferbert, "Dimensions of State Politics, Economics, and Public Policy," *American Political Science Review*, 63 (September, 1969), 867–879; Duane Lockard, "State Party Systems and Policy Outputs," in *Political Research and Political Theory*, ed. Oliver Garceau (Cambridge: Harvard University Press, 1968), pp. 190–220. Also relevant to this point is Cnudde and McCrone, pp. 858–866.

The Mediating Influence of the State Legislature

In this paper I do not wish to enter this debate, much less to attempt to resolve it.[6] The purpose of this paper, rather, is to reformulate the theoretical link between interparty competition and welfare policies and to present some evidence which indicates that the reformulation is empirically fruitful.

The major problem with the party competition-welfare expenditures hypothesis is that it does not consider the influence that state political institutions can have on the determination of public policy. In particular, to argue that interparty competition significantly influences the level of welfare expenditures assumes that state legislatures are able to convert party conflict into public policies. But given the weak organizational structure of many state legislatures, this would seem to be a questionable assumption at best. Indeed, since many state legislatures provide only a minimum of services to legislators, meet relatively infrequently, and in general, can be considered organizationally ineffective, it is understandable that even the most salient forms of interparty conflict do not have a firm, consistent impact on state policies. On the other hand, in those states which have organizationally strong, effective legislatures, the political system at least has the potential capacity to translate party conflict into state policies. Understood in this way, the legislature can be seen to function as a mediating influence in the state policy-making process.

The above reasoning leads to the specific proposition which will be tested in this paper: that interparty competition and welfare expenditures should be more strongly associated among those states which have organizationally strong, effective legislatures than among those states which have weak legislative systems. A logical implication of this proposition will also be investigated: that after controlling for various socioeconomic factors the partial correlations between interparty competition and welfare expenditures will be significantly stronger among those states which have strong, as opposed to weak, legislative systems.[7]

[6] For discussions of these conflicting findings and explanations see John H. Fenton and Donald W. Chamberlayne, "The Literature Dealing with the Relationships between Political Processes, Socio-economic Conditions and Public Policies in the American States: A Bibliographical Essay," *Polity*, 1 (Spring, 1969), 388–394; Herbert Jacob and Michael Lipsky, "Outputs, Structure, and Power: An Assessment of Changes in the Study of State and Local Politics," *Journal of Politics*, 30 (May, 1968), 511–519; Brian R. Fry and Richard F. Winters, "The Politics of Redistribution," *American Political Science Review*, 64 (June, 1970), 508–522; and John L. Sullivan, "A Note on Redistributive Politics," *American Political Science Review*, 66 (December, 1972), 1301–1305.

[7] Throughout this paper when correlations and differences between correlations are described as significant, I do not mean to imply that these correlations are statistically significant at a given level. This classical notion of statistical inference from sample to population would be inappropriate in this case, since the data came from the population of states, not a sample thereof. Rather, my use of the term "significant" means simply that the correlation or difference between two correlations is not a trivial one; that it is substantively important. The statistics provided in this paper, therefore, are intended to make inferences from the data to specified theoretical notions, not to estimate population parameters.

Measurement of Variables

Interparty Competition. The measure of interparty competition used in this research was developed by Austin Ranney.[8] The Ranney index covers the 1956–70 time period and applies only to state offices. The measure is based on four components: (1) the average percentage of the popular vote won by the Democratic gubernatorial candidate; (2) the average percentage of the seats in the state senate held by the Democrats; (3) the average percentage of the seats in the state house of representatives held by the Democrats; and (4) the percentage of all terms for governor, senate, and house in which the Democrats had control. For each state Ranney averaged together all four percentages to produce an "index of competitiveness."[9]

Welfare Policies. Since many state policies have a welfare dimension I have defined welfare policies broadly to include: (1) per capita state and local education expenditures (1968–69); (2) per capita state and local welfare expenditures (1969); (3) average monthly payment for old-age assistance (1969); (4) average monthly payment for aid to dependent children (1969); (5) average monthly payment for aid to blind (1969); (6) average monthly payment for aid to disabled (1969); and (7) percentage of public assistance supplied by state and local governments (1968).

Socioeconomic Factors. Of the many socioeconomic characteristics of the states that are relevant to public policy generally and welfare policies in particular, the present analysis considers the following four factors: (1) per capita personal income; (2) per capita retail sales; (3) percentage of population employed in manufacturing; and (4) percentage of population living in urban areas. These measures were selected because they

[8] Austin Ranney, "Parties in States Politics," in *Politics in the American States*, ed. Jacob and Vines, pp. 82–121. The index appears on page 87.

[9] All index scores above .5000 were subtracted from 1.0000 in order to transform the index from one of degree of Democratic control to degree of party competition, with higher scores representing greater degrees of competition.

Table 1. Zero-Order Correlation Coefficients (Pearson r's) Between Interparty Competition and Welfare Expenditures, by Level of Legislative Professionalism[a]

	Among States High in Legislative Professionalism	Among States Low in Legislative Professionalism	Difference Between Correlations
1. Education Expenditures	.62	.41	.21
2. Total Welfare Expenditures	.30	−.15	.45
3. Old-Age Assistance	.51	.23	.28
4. Aid to Dependent Children	.79	.45	.34
5. Aid to Disabled	.79	.32	.47
6. Aid to Blind	.61	.35	.26
7. Percentage of Public Assistance Supplied by State and Local Governments	.86	.68	.18
Mean Correlation	.64	.33	.32

[a] Alaska and Hawaii not included.

seem to be valid indicators of the social, economic, and ecological development of the states and because they have been used extensively in previous research on state politics.

This study also requires a measure of the organizational effectiveness of state legislatures. I have utilized Grumm's legislative professionalism index as the closest approximation to the desired measure.[10] Grumm's index provides a single, summary score for each state based on a factor analysis of the following variables: the biennial compensation of legislators; expenditures for legislative staff, services, operations, and printing; number of bills introduced in a session; length of sessions in calendar days; and a score measuring legislative services. The time period for most of these components is 1963–64. The higher the score for the state, the more professional the state legislature is thought to be. The scores were dichotomized at the median. Those states with a score above the median were considered to be high in legislative professionalism while those below the median were considered to be low in legislative professionalism.

Analysis

Table 1 presents the correlations between interparty competition and welfare expenditures among states high and low in legislative professionalism. If the theoretical notions outlined above are empirically valid, we should observe a stronger set of correlations among states high in legislative professionalism than among states low in legislative professionalism. Turning first to states that are high in legislative professionalism, one can see that the correlations range from .30 to .86 with the mean correlation being .64. There is, in other words, a strong, positive relationship between party competition and level of welfare expenditures among states with professional legislatures. Among states low in legislative professionalism, the correlations are, with one exception, positive, but in every case, they are lower than the corresponding correlations among states high in legislative professionalism. The magnitude of the differences range from a low of .18 to a high of .47 with the mean difference being .32. Overall, the results presented in Table 1 offer strong and consistent support for the central proposition advanced in this paper: that the linkage between party competition and welfare policies is greater for those states with effective legislatures than for those states with ineffective legislative systems.

Table 2 presents the first-order partial correlations between interparty competition and welfare expenditures after controlling for each of the socioeconomic factors. The results are shown separately for states high and low in legislative professionalism. While the control for per capita income substantially reduces the correlations among states with professional legislatures, controlling for the other socioeconomic factors does not greatly alter the zero-order relationships. Among states low in legislative professionalism, controlling for both per capita retail sales and per capita income significantly reduces the original relationships.

The more important point to note about Table 2, however, is that when each SES factor is controlled for, the set of partial correlations remains

[10] John G. Grumm, "The Effects of Legislative Structure on Legislative Performance" in *State and Urban Politics: Readings in Comparative Public Policy*, ed. Richard I. Hofferbert and Ira Sharkansky (Boston: Little, Brown, and Company, 1971), pp. 298–322. The index appears on page 317. Evidence relevant to the construct validity of Grumm's index appears in The Citizens Conference on State Legislatures, *State Legislatures: An Evaluation of Their Effectiveness* (New York: Praeger Publishers, 1971), p. 70. Their analysis shows that Grumm's index is significantly related to a measure of legislative capability which they devised for each state.

Table 2. First-Order Partial Correlation Coefficients (Pearson r's) Between Interparty Competition and Welfare Expenditures, by Level of Legislative Professionalism[a]

	Legislative Professionalism							
	High				Low			
	Controlling For							
	SES1	SES2	SES3	SES4	SES1	SES2	SES3	SES4
1. Education Expenditures	.76	.61	.41	.65	.46	.01	.23	.41
2. Total Welfare Expenditures	.20	.26	.28	.22	−.17	−.16	−.08	−.15
3. Old-Age Assistance	.36	.42	.05	.37	.26	−.08	.07	.27
4. Aid to Dependent Children	.68	.75	.51	.69	.45	.05	.27	.52
5. Aid to Disabled	.69	.73	.34	.69	.32	.20	.09	.16
6. Aid to Blind	.53	.51	.22	.46	.37	−.17	.08	.35
7. Percentage of Public Assistance Supplied by State and Local Governments	.78	.82	.60	.81	.70	.30	.24	.59
Mean Correlation	.57	.59	.34	.56	.34	.02	.13	.31

[a] Alaska and Hawaii not included.
SES1—Percentage of population employed in manufacturing.
SES2—Per capita retail sales.
SES3—Per capita personal income.
SES4—Percentage of population living in urban areas.

substantially stronger among states high in legislative professionalism than among states low in legislative professionalism. In fact, of the 56 correlations presented in Table 2, 55 are consistent with this pattern and the remaining partial (old age assistance controlling for per capita personal income; .07 versus .05) is more a tie than a complete reversal. The major conclusion to be drawn from this analysis is that the effectiveness of state legislatures continues to mediate the link between party competition and welfare policies even after controls are introduced for each of the socioeconomic factors.

Table 3 shows the correlations between interparty competition and welfare expenditures after simultaneous controls have been introduced for the four socioeconomic factors. Examining those states which are low in legislative professionalism, one can see that three of the partial correlations are negative and the other correlations though positive are very weak. Conversely, among those states high in legislative professionalism, all seven partials are positive, and three of them (education expenditures, aid to dependent children, and percentage of public assistance supplied by state and local governments)[11] are quite impressive. Indeed, only the partial correlation between party competition and old-age assistance is insubstantial.

A comparison of the two sets of fourth-order partials indicates that for each of the welfare measures the partial is higher (i.e., has a higher positive value) for those states with professional legislatures than for those states with unprofessional legislatures. Similarly, an examination of the mean partials shows that there is a practically negligible correlation (.01) among states low in legislative professionalism, while the average partial among states high in legislative professionalism is substantial (.38). Thus, the link between party competition and welfare expenditures continues to be mediated by the differential effectiveness of state legislatures even after simultaneous controls are introduced for the socioeconomic factors.

Table 4 presents the results of a multiple regression analysis which utilizes the four socioeconomic factors and the interparty competition measure as predictor (independent) variables for each of the seven welfare policies. The analysis is performed separately for states high and low in legis-

[11] Among states high in legislative professionalism, then, these three welfare expenditures (of the seven studied) are the most significantly affected by interparty competition. In this sense, they may be described as the most politically controversial and perhaps publically salient of the policies in the welfare domain. While it may be obvious that aid to dependent children and percentage of public assistance supplied by state and local governments are central elements in the welfare domain, it seems somewhat more difficult to justify the centrality of educational expenditures in terms of the struggle between the haves and have-nots. A possible reason for its centrality in the present case is that the measure of educational expenditures used in this paper does not include capital expenditures but reflects instead, such controversial items as the salaries of teachers, cost of books, special education costs, etc. See Cnudde and McCrone, pp. 859, 862, and 865 for a more extended discussion and analysis of the relation of educational expenditures to the struggle between the haves and have-nots.

Table 3. Fourth-Order Partial Correlation Coefficients (Pearson r's) Between Interparty Competition and Welfare Expenditures, by Level of Legislative Professionalism[a]

	Legislative Professionalism	
	High Controlling For	Low Controlling For
	Four SES Variables	Four SES Variables
1. Education Expenditures	.65	.18
2. Total Welfare Expenditures	.27	−.13
3. Old-Age Assistance	.02	−.14
4. Aid to Dependent Children	.54	.10
5. Aid to Disabled	.31	.11
6. Aid to Blind	.24	−.25
7. Percentage of Public Assistance Supplied by State and Local Governments	.63	.18
Mean Correlation	.38	.01

[a] Alaska and Hawaii not included.
SES1—Percentage of population employed in manufacturing.
SES2—Per capita retail sales.
SES3—Per capita personal income.
SES4—Percentage of population living in urban areas.

lative professionalism. The coefficients are standardized partial regression coefficients (beta weights) indicating the direct explanatory contribution of interparty competition for each welfare policy. These beta weights can be interpreted as the average change in the specified welfare expenditure per standardized unit change in interparty competition with the influence of the four SES factors statistically removed.[12] A comparison of these betas across states high and low in legislative professionalism indicates: (1) that for five policies—education expenditures, total welfare expenditures, aid to dependent children, aid to blind, and percentage of public assistance supplied by state and local governments—the betas are significantly higher among states with professional, as opposed to unprofessional, legislative systems; (2) that, on the whole, all the betas are fairly weak among states low in legislative professionalism; and (3) that among states high in legislative professionalism, only expenditures for old-age assistance is not significantly affected by party competition.[13]

[12] See Karl Schuessler, *Analyzing Social Data: A Statistical Orientation* (Boston: Houghton Mifflin Company, 1971), p. 24.

[13] The comparison of beta-coefficients across different samples or different subsets within a sample can be distorted by unequal variances. However, an examination of the unstandardized partial regression coefficients, which are not affected by unequal variances, confirms the interpretation offered here in all essential respects. For a discussion of the use of standardized versus unstandardized regression coefficients see H. M. Blalock, Jr., "Causal Inferences, Closed Populations, and Measures of Association," in *Causal Models in the Social Sciences*, ed. H. M. Blalock, Jr. (New York: Aldine Atherton, 1971), pp. 139–151.

Summary and Conclusions

In this brief paper I have argued that the hypothesis linking interparty competition and welfare policies was basically incomplete, for it, in effect, "assumed away" the decisive influence that political institutions can have on policy making. My alternative formulation presumes that the degree of competition exhibited by political parties does influence the nature and scope of public policy in general and welfare policy in particular but that the precise dimensions of its influence are determined by the states' formal decision-making institutions. Put somewhat differently, this formulation asserts that political institutions can affect the intensity of the linkage between party competition and welfare policies.

I argued, specifically, that the relationship between interparty competition and welfare expenditures should be mediated by the differential effectiveness of state legislatures. Because the

Table 4. Standardized Partial Regression Coefficients Regressing Welfare Expenditures on Interparty Competition With SES Controlled, By Level of Legislative Professionalism[a]

	Among States High in Legislative Professionalism	Among States Low in Legislative Professionalism
1. Education Expenditures	.76	.22
2. Total Welfare Expenditures	.45	−.21
3. Old-Age Assistance	.03	−.20
4. Aid to Dependent Children	.59	.13
5. Aid to Disabled	.27	.17
6. Aid to Blind	.32	−.31
7. Percentage of Public Assistance Supplied by State and Local Governments	.53	.18

[a] Alaska and Hawaii not included.
SES1—Percentage of population employed in manufacturing.
SES2—Per capita retail sales.
SES3—Per capita personal income.
SES4—Percentage of population living in urban areas.

state legislature is a principal political institution for converting party conflict into public policy, and because the conversion process should be effective to the degree that the legislature is organizationally capable, it seemed likely that the linkage between party competition and welfare expenditures would be more pronounced among states with strong, effective legislative systems than among those with weak legislative systems.

On the whole, the empirical results of the analysis were highly congruent with these theoretical expectations. When legislative effectiveness was defined in terms of professionalism and welfare effort was assessed by seven specific measures, it was found that the link between party competition and welfare expenditures was indeed stronger among states with effective, as opposed to ineffective, legislative systems. The differences, moreover, were not marginal and did not lack consistency; on the contrary, the correlations were substantially stronger for states with professional legislatures with respect to all seven welfare policies[14] (i.e., the mean difference in correlations was .32).

This differential pattern of correlations persisted when controls were introduced for four socioeconomic factors. That is, after controlling for socioeconomic development, the partial correlations between interparty competition and welfare expenditures were significantly stronger among those states high in legislative professionalism than among those low in legislative professionalism. Among those states low in legislative professionalism, three of the partials were negative and the other four were exceedingly weak. This indicates that the link between party competition and welfare policies is not only tenuous but may, in fact, be spurious for some welfare expenditures (i.e., total welfare expenditures, old-age assistance, and aid to blind). Figure 1 indicates the overall pattern of intercorrelations found among socioeconomic development, party competition, and welfare policy among states low in legislative professionalism. The broken arrow connecting party competition and welfare policies indicates that there is, at best, only a weak linkage between them after controls are introduced for the SES factors.

[14] The states were also trichotomized according to their scores on the legislative professionalism index and relevant correlations were computed between interparty competition and welfare expenditures within each set of states. As would be expected on the basis of findings reported in this paper, the correlations became stronger as the set of states became more professionalized.

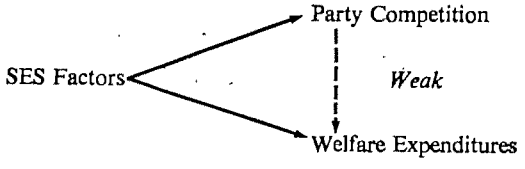


Figure 1. Pattern of Intercorrelations Among SES Factors, Party Competition, and Welfare Expenditures Among States Low in Legislative Professionalism.

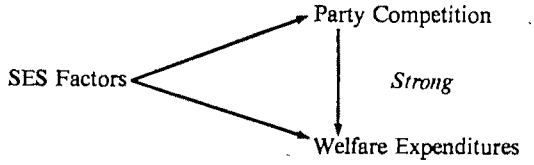


Figure 2. Pattern of Intercorrelations Among SES Factors, Party Competition, and Welfare Expenditures Among States High in Legislative Professionalism.

The partials between interparty competition and welfare expenditures among states high in legislative professionalism present an entirely different situation. When the SES factors were controlled for, only the correlation between party competition and expenditures for old age assistance proved insubstantial; three of the other partials were moderately high and three of them were quite high. In other words, the introduction of controls for the SES factors did not produce insignificant correlations for six of the seven welfare measures. The unbroken arrow connecting party competition and welfare policies in Figure 2 schematically depicts the strong link between these variables for states high in legislative professionalism.

This paper, in sum, has attempted to do more than simply describe the relationship between party competition and welfare policies in the American states. By demonstrating that the state legislature can mediate this linkage, it has also sought to reaffirm the importance of political institutions in any explanatory model of the policy-making process.

(*Appendix follows on next page*)

APPENDIX: SOURCE OF VARIABLES

1. Per capita state and local education expenditures (excludes capital expenditures)	Council of State Governments, *The Book of the States, 1970–71* (Lexington, Kentucky: Council of State Governments, 1970), p. 306.
2. Per capita state and local welfare expenditures	U.S. Bureau of the Census, *Governmental Finances in 1968–69* Washington, D.C., 1969
3. Average monthly payment for old-age assistance	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1970* (91st ed.) Washington, D.C., 1970, Table 455, p. 301.
4. Average monthly payment for aid to dependent children	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1970* (91st ed.) Washington, D.C., 1970, Table 455, p. 301.
5. Average monthly payment for aid to disabled	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1970* (91st ed.) Washington, D.C., 1970, Table 455, p. 301.
6. Average monthly payment for aid to blind	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1970* (91st ed.) Washington, D.C., 1970, Table 455, p. 301.
7. Percentage of public assistance supplied by state and local governments	Council of State Governments, *The Book of the States, 1970–71* (Lexington, Kentucky: Council of State Governments, 1970), p. 375.
8. Percentage of population employed in manufacturing	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1965* (86th ed.) Washington, D.C., 1965, Table 308, p. 225.
9. Per capita personal income	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1965* (86th ed.) Washington, D.C., 1965, Table 458, p. 334.
10. Per capita retail sales	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1965* (86th ed.) Washington, D.C., 1965, Table 1195, p. 824.
11. Percentage of population living in urban areas	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1965* (86th ed.) Washington, D.C., 1965, Table 11, p. 16.

Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities*

WAYNE A. CORNELIUS
Massachusetts Institute of Technology

The delegation of petitioners from Community "X" has been waiting patiently for several hours: seven entire families including restless children and family heads who have taken time off from their jobs, led by officers of the community's improvement association. The anteroom leading to the office of a government bureaucrat is crowded with dozens of similar delegations from other neighborhoods of the city. The petitioners from Community "X" are finally granted admittance. They are urban squatters, and present a laboriously typed petition signed by virtually all residents of their community, requesting government recognition of their land tenure rights. Acting as spokesman for the group, one of the petitioners argues that land titles are essential to assuring a modest inheritance for their children. Moreover, he continues, security of tenure stimulates investment in permanent house construction and commercial enterprises within the community. The spokesman also observes pointedly that with property titles, community residents may begin contributing to government tax revenues. To strengthen the image of their community as one deserving of government aid, other members of the delegation cite examples of the hardships they have endured in settling the land and the self-help efforts they have undertaken to improve the community. The petitioners stress their belief in the government's commitment to humanitarian concerns, and the desirability of granting their request in the interests of social justice and social tranquility. The official promises that the community's situation will be thoroughly investigated. The session is concluded amid fervent expressions of the petitioners' support for the incumbent regime.

This scene is repeated many times daily in government offices in many Third World cities. It represents the first step in a long, complex, and often frustrating effort to manipulate the political system to satisfy a deeply felt need. The petitioners from low-income urban settlement zones such as Community "X" are typically of rural origin. They have migrated to the city in search of economic opportunity, and find that rent-free housing provided by residence in squatter settlements is a key element in fulfilling their material aspirations for themselves and their children.

Petitioning of government officials for such benefits as land titles and basic urban services also represents a form of political participation which is all too often overlooked in research and writing on the political behavior of cityward migrants or the urban poor in general. In fact, most researchers who have contributed to this literature have been concerned primarily with the potential for political radicalism or violence among such populations. The present study is an attempt to fill in some of the numerous gaps in our understanding of how migrants to the city become involved in the political process in nonviolent, nondisruptive ways.

The particular mode of political participation upon which this paper focusses is political demand making, defined as individual or collective activities aimed at extracting certain types of benefits from the political system by influencing the decisions of incumbent government officials. Demand making is thus differentiated from political participation aimed at influencing government resource allocation by replacing or retaining the incumbent authorities (i.e., electoral participation) or by overthrowing or restructuring the political system itself (e.g., through revolutionary violence). Political demands are defined as needs whose satisfaction is felt to depend upon governmental action, and which are asserted by indi-

* This paper represents a report of findings from the first stage of a cross-national study of politicization and demand-making behavior among low-income migrants to large cities, being conducted in collaboration with Henry A. Dietz (University of Texas, Austin) and James W. White (University of North Carolina, Chapel Hill). The study was supported from 1972–1974 by a grant from the Ford and Rockefeller Foundations' Program of Social Science and Legal Research on Population Policy. The author is also indebted to the Danforth Foundation, the National Science Foundation (Grant GS-2738), the Foreign Area Fellowship Program, and the Center for International Affairs at Harvard University for financial support during the periods of data collection in Mexico City and data processing at Stanford University and Harvard University. The assistance of Harley Browning, Alex Inkeles, and Sidney Verba in construction of the basic survey instrument used in this study is gratefully acknowledged. Helpful comments on a preliminary version of this paper were provided by Ann L. Craig, Henry A. Dietz, Richard R. Fagen, John O. Field, Frederick W. Frey, and Merilee Grindle. Portions of this paper were presented at the Annual Meeting of the American Political Science Association, New Orleans, September 1973.

viduals or groups as specific claims upon the government. The term "demand" implies nothing about the way in which such claims are asserted. Particularly among low-income city dwellers in Latin America, demand articulation need not, and usually does not, involve table pounding, protest demonstrations, or other aggressive forms of behavior.

Demand making as a mode of political participation has several distinctive characteristics. Because the individual participant—or the leaders of the demand-making group of which he may be a member—determines the subject matter of contacts with public officials, the outcome of the participatory act is certain to be highly salient and important to the individual.[1] The linkage between the participatory act and its results is more direct and visible in this mode of political activity than in any other. Moreover, the motivations for participation in the demand-making mode are almost entirely instrumental rather than expressive or regime-supportive.[2] Although demand-making activity may sometimes have ritualistic, system-supportive overtones, it is nevertheless a form of behavior aimed primarily at manipulating the political system and influencing value allocations. Demand makers are persons who are "actively seeking explicit responses from the political system" and perceive their behavior as "a tactic to enhance their chances of eliciting those responses."[3] If political participation is conceived generally as citizen activity aimed at influencing governmental decision making, participation within the demand-making mode is the most direct and often the most effective strategy of goal attainment among the types of urban dwellers who are the focus of this study.

Urbanization and Political Demand Making

Theorists of political and social change, as well as government policy makers, have often posited a close relationship between increased rural-to-urban migration and changes in the types and frequency of demands made upon governments.[4] It has been argued that migration to the city tends to expand the range of human needs which are susceptible to satisfaction through governmental action. At the same time, exposure to the urban environment is thought to increase levels of political awareness among the migrant population. Finally, cityward migration places a larger proportion of the population in close proximity to government officials, facilitating citizen access to such officials and reducing the costs of articulating needs in terms of time, effort, and money. The result is assumed to be accelerated demand making; and it is further assumed that demands for public services and other governmental benefits will increase much faster than does the capability of the political system to respond to them. Applying this line of argument specifically to Latin America, Robert Scott has written that

> as the character of the general population alters from predominantly rural and isolated to partly integrated and urban, . . . shifts in the kinds, amounts, and intensity of demands on government occur. Both totally and proportionately more people leave subsistence agriculture and sharecropping to become part of the money economy in the city, or outside it as members of the rural proletariat and participants in commercial or market agriculture operations. To such persons, what happens in national politics is important, and the kinds of government programs they will condone change drastically. In the past . . . much of what government provided either was symbolic—a land-reform law that was not implemented, for instance—or not so much what the masses wanted as what government leaders felt they should have. Now, as the general populace becomes more knowledgeable, this sort of government activity does not satisfy them.[5]

[1] See the discussion of contacting of officials as a mode of political participation in Sidney Verba and Norman H. Nie, *Participation in America* (New York: Harper and Row, 1972), p. 52.

[2] On the distinction between "instrumental" and "expressive" political participation, see Lester Milbrath, *Political Participation* (Chicago: Rand McNally, 1965), p. 12. Verba and Nie (*Participation in America*, p. 2) also differentiate between instrumental participation and "ceremonial" or "support" participation, in which "citizens 'take part' by expressing support for the government, by marching in parades, by working hard in developmental projects, by participating in youth groups organized by the government, or by voting in ceremonial elections." See also the discussion of ritualistic and system-supportive participation in the Mexican political system, in Richard R. Fagen and William S. Tuohy, *Politics and Privilege in a Mexican City* (Stanford, Calif.: Stanford University Press, 1972), pp. 38–39.

[3] Peter K. Eisinger, "Protest Behavior and the Integration of Urban Political Systems," *Journal of Politics*, 33 (November, 1971), 984.

[4] See especially Karl W. Deutsch, "Social Mobilization and Political Development," *American Political Science Review*, 55 (September, 1961), 498–499; Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), p. 5; and W. Howard Wriggins and James F. Guyot, "Demographic Change and Politics," in *Population, Politics, and the Future of Southern Asia*, ed. W. H. Wriggins and J. F. Guyot (New York: Columbia University Press, 1973), pp. 5–16. See also, with specific reference to Latin America, Ernest A. Duff and John F. McCamant, "Measuring Social and Political Requirements for System Stability in Latin America," *American Political Science Review*, 62 (December, 1968), 1125–1129; and the studies cited in Wayne A. Cornelius, "The Political Sociology of Cityward Migration in Latin America: Toward Empirical Theory," in *Latin American Urban Research, Volume I*, ed. Francine F. Rabinovitz and F. M. Trueblood (Beverly Hills, Calif.: Sage Publications, 1971), pp. 100–101.

[5] Robert E. Scott, "National Integration Problems and Military Regimes in Latin America," in *Latin American Modernization Problems*, ed. R. E. Scott (Urbana, Ill.: University of Illinois Press, 1973), pp. 301–302.

Such arguments appear to be based upon rather mechanistic assumptions about the conversion of objectively defined needs into effective demands upon government. The demand for public services is viewed largely as a function of the number of potential clients, whose ranks are swelled not only by migration from the countryside but also by persistently high fertility rates among native-born residents of Third World cities. The evidence presented in this paper suggests that this kind of "one man, one demand" assumption may prove highly misleading in any attempt to estimate the potential impact of rapid population growth or redistribution upon the functioning of political systems. Particularly with regard to migrants from the countryside, the extent to which increased political awareness and objectively defined needs are actually translated into increased pressures for extension of public services and other types of benefits is an empirical question, and one which has rarely been addressed in previous research.

Research bearing on this question must go beyond measurement of the absolute level of frequency of political demand making among newly urbanized sectors of the population. It must seek answers to a number of questions relating to the *process* by which objective needs are converted into political demands. How do migrants to the city perceive or define their most acute needs and problems? To what extent is the government perceived by such people as the relevant problem-solving agency? What is the substantive nature of the demands presented? Are they concerned with individual needs, or collective needs of urban neighborhoods or other types of social units? Toward what kinds of officials or agencies are the demands directed? What strategies are used in demand-making attempts, and what factors determine the outcomes of such attempts? In sum, how do those seeking to influence the distribution of government benefits accomplish their goals?

This paper will present some research findings bearing on these questions, drawn from a larger comparative study of political attitudes and behavior among low-income migrants to the capital cities of Mexico, Peru, and Japan. In Mexico City, male heads of family residing in six predominantly low-income settlement zones or *colonias proletarias* comprise the universe of the study. These communities, all located on the periphery of the metropolitan area, were selected purposively so as to maximize the range of variation among them on three dimensions of particular revelance to political demand making: (1) the age of the community; (2) its type of origin (i.e., how the community was originally occupied or created); and (3) the overall developmental level of the community, defined in terms of access to basic urban services and improvements. Thus the research sites in Mexico City include three squatter settlements at different stages of evolution (designated here as Colonia Nueva, Colonia Periférico, and Colonia Militar), two different types of government-initiated settlements (Colonia Esfuerzo Propio and Unidad Popular), and a commercially developed subdivision (Colonia Texcoco).[6]

In each community a stratified probability sample of male heads of family was selected for structured, personal interviews. Approximately one third of the total sample consists of non-migrants, defined as individuals born or raised in the Mexico City metropolitan area. The remainder are migrants, defined as people born outside of Mexico City who spent most of their lives between ages 5 and 15 outside of the city. The data gathered through sample survey interviews were supplemented with ethnographic data gathered through several months of participant observation in each of the research communities. Unstructured interviews were also conducted with the leaders of each community, as well as with government officials who have had extended contact with the community.[7] Large portions of the basic survey instrument were also administered in an independent study conducted in four rural communities which have served as points of origin for migrants to Mexico City.[8] In the analysis below, some comparisons are made between migrants to Mexico City and residents of these rural communities. Contrasting findings from a parallel study of migrants to Lima, Peru, will also be reported.[9]

The Conversion of Needs into Political Demands

It is possible to identify several distinct stages in the conversion of objectively defined needs into

[6] Pseudonyms have been used to identify all of the research communities. The formation and subsequent development of each community are described in Wayne A. Cornelius, *Political Learning Among the Migrant Poor: The Impact of Residential Context* (Beverly Hills, Calif.: Sage Publications, 1973 [Sage Professional Papers in Comparative Politics, Vol. 4, No. 01-037]), pp. 10–15.

[7] Field work was conducted in Mexico City from January to December, 1970, with supplemental data gathering in 1971 and 1972. For a more detailed description of the design and methodology of the study, see Chapter 1 and Appendix "D" in Wayne A. Cornelius, "Poverty and Politics in Urban Mexico: Political Learning Among the Migrant Poor" (Ph.D. dissertation, Stanford University, 1973).

[8] F. Christopher Arterton, "Patterns of Political Participation in Four Rural Mexican Villages" (Ph.D. dissertation, Massachusetts Institute of Technology, 1973).

[9] See Henry A. Dietz, "Becoming a Poblador: Political Adjustment to the Lima Urban Environment" (Ph.D. dissertation, Stanford University, 1973). Additional data from the Lima survey are presented in Wayne A. Cornelius and Henry A. Dietz, "Urbanization, Demand-Making, and Political System 'Overload': Political Participation Among the Migrant Poor in Latin American Cities," paper presented at the Annual Meeting of the American Political Science Association, New Orleans, Louisiana, September, 1973.

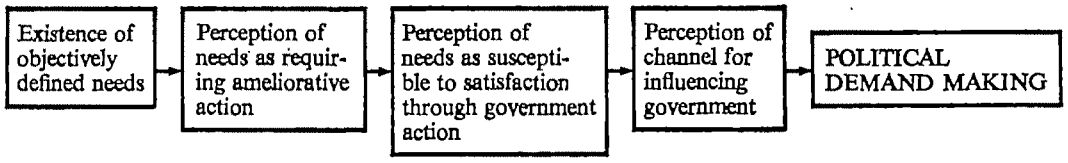


Figure 1. Basic Stages in the Process of Demand Creation

actual demands upon government. Objective deprivations or problems must first be perceived by the individual as requiring some kind of solution or ameliorative action. Secondly, these felt needs must be viewed as needs particularly susceptible to satisfaction through governmental action. Finally, the potential demand maker must be able to perceive a strategy or channel through which to articulate these "politicized" needs. The basic stages in this process of demand creation are summarized graphically in Figure 1.[10]

The assumption implicit in most discussions of the relationship of urbanization to demands on the political system is that the conversion of objective needs into political demands occurs almost automatically. It remains, however, a largely unproved assumption. In the analysis which follows I will attempt to demonstrate that there are often major lags in the process of demand creation among migrants to the city, and that, in fact, many kinds of felt needs are viewed by migrants as needs to be satisfied primarily through individual action. Such needs rarely become the objects of political demand-making attempts.

[10] For an alternative conceptualization of the process of demand creation at the level of the individual, see Jerry F. Medler, "Negative Sanctions: Their Perception and Effect in the Political System" (Ph.D. dissertation, University of Oregon, 1966), chapter 5. My model of the process differs from that of Medler primarily in the lesser importance which I attribute to cognitive involvement in politics and a sense of individual political efficacy as preconditions of the act of demand makng. See also George J. Graham, Jr. and Richard A. Pride, "Styles of Political Participation, Want Conversion, and Political Support among Adults and Students in a Metropolitan Community," paper presented at the Annual Meeting of the American Political Science Association, Chicago, September, 1971.

Table 1. Measures of Economic and Service Needs Among Migrants to Mexico City[a]

Have water piped into house	29.7%
Have connection with public sewerage system	30.0
Have paved street adjacent to house	24.8
Lack electricity for house	4.4
Obtain electricity illegally	48.3
Have regular electricity supply	47.4
Have one-room house	31.9
Have house with walls of non-permanent materials	23.3
Have formal, definitive title to land	4.0
Unemployed and seeking work	4.7
Have family income of minimum wage or less[b]	53.7
Have six or more dependents living at home	60.8
Have school-age children	90.0
Had children who died before age 6, while living in city	50.6

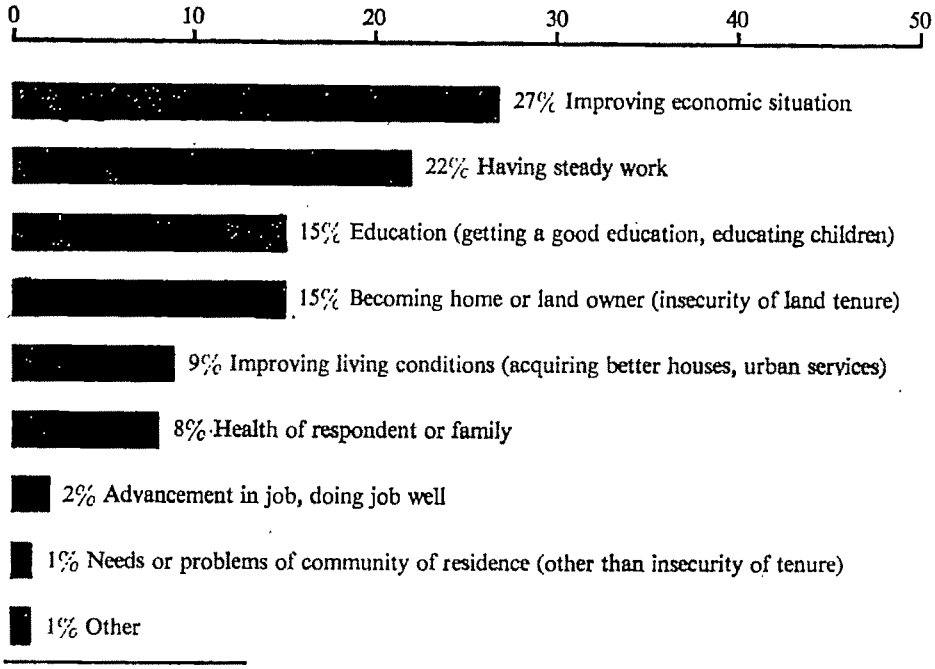
[a] Percentages are based on an average N of 618.

[b] At the time of the study, a minimum-wage income in Mexico City was equivalent to about $60.00 (U.S.) per month.

Objective and Perceived Needs. The objective needs of migrants residing in the six research communities are massively evident. As reported in Table 1, less than a third had piped water or connections with public sewerage systems for their houses at the time of the study. Less than a third of the migrants had access to paved streets or permanent sidewalks near their homes. Nearly half obtained their electricity for home use through illegal connections with public transmission lines in nearby urbanized areas. More than one in four of the migrant families still resided in one-room shacks made predominantly of laminated cardboard or other nonpermanent materials. Only 4 per cent had received formal title to the land which they occupied.[11] Less than 5 per cent of the migrant heads of family were unemployed and seeking work at the time of the study, but most held jobs which were poorly paid. Over half had family incomes of less than (U.S.) $60 per month. Migrant families were large (more than 60 per cent of the family heads had six or more dependents living at home) and 90 per cent had school-age children. Child mortality was high; more than half of the families reported having had children who died before reaching the age of six while residing in Mexico City.

What kinds of needs or problems are *perceived* as most important among this sector of the urban population? In the survey, respondents were asked to specify what they felt to be their most

[11] The extremely low proportion of migrants having formal, "definitive" title to their land reflects not only the illegal origins of three of the communities but a lack of money to pay the fees involved in obtaining an officially recognized property title, even after all necessary payments for the cost of the land have been made.



[a] Question: "What about your worries . . . what kinds of things do you worry about most?" $N=629$. "Don't know" and "Have no worries" responses (representing 7.3% of the sample) are excluded.

Figure 2. Greatest Personal Concern Among Migrants to Mexico City.[a]

pressing personal concern. The responses, presented in Figure 2, indicate that problems relating to socioeconomic mobility are uppermost in the concerns of the respondents. Nearly half of the sample report their greatest personal concern to be either improving their economic situation or having steady employment. Education—either for oneself or one's children—is another key area of concern, followed closely by the need to become a land or home owner. Fewer than one in ten migrants regard improvement of living conditions (through acquiring better housing or access to basic urban services) as their most important concern.

The migrants were also asked to identify the most serious problem or need relating to their community of residence. Their responses, classified by community, are reported in Table 2. Security of tenure is the overwhelmingly salient need in the two squatter settlements, Colonia Nueva and Colonia Periférico, where official recognition of individual occupancy rights had not yet been secured at the time of the survey. In the four officially recognized communities (Colonia Texcoco, Colonia Esfuerzo Propio, Unidad Popular, and Colonia Militar), virtually no one perceived security of tenure as a key need. In the latter communities, basic urban services and improvements —particularly piped water, sewerage systems, and paved streets—are regarded as the most serious deficiencies, followed by community facilities such as schools, public markets, and health care centers. Such problems as environmental pollution, lack of parks and other recreational facilities, and inadequate public transportation are salient only in the two most highly developed communities (Unidad Popular and Colonia Militar); and even in these communities, deficiencies in water supply and in community facilities such as schools and public markets are regarded as more serious. The overall frequency (across communities) with which needs for water and sewerage systems, paved streets, and security of tenure are mentioned as the most important community-related deficiencies corresponds closely with the findings of other studies of perceived needs among residents of low-income communities in Latin American cities.[13]

[13] Douglas Butterworth, "Squatters or Suburbanites? The Growth of Shantytowns in Oaxaca, Mexico," in Scott, *Latin American Modernization Problems*, p. 231; Frank M. Andrews and George W. Phillips, "The Squatters of Lima: Who They Are and What They Want," *Journal of Developing Areas*, 4 (Janu-

Table 2. Most Important Community Problem Perceived by Migrants to Mexico City, by Community[a]

Problem	Colonia Nueva	Colonia Periférico	Colonia Texcoco	Colonia E. Propio	Unidad Popular	Colonia Militar	ALL
Water supply	23.4%	12.6%	32.5%	12.0%	*35.6%*	7.9%	20.1%
Sewerage	11.0	28.7	*41.7*	14.5	0.0	0.9	16.9
Security of tenure	*46.7*	*45.8*	0.8	0.0	0.0	2.6	16.5
Medical care facilities	2.0	0.0	10.4	*20.5*	13.6	13.2	10.0
Street pavement	2.3	8.9	7.5	19.6	3.4	2.6	7.7
Public markets	0.0	0.0	0.0	12.9	0.0	*28.1*	7.2
Schools	9.1	1.1	0.0	0.0	1.7	25.3	6.4
Environmental[b]	0.0	0.0	0.0	4.3	23.7	0.0	3.6
Electricity	1.0	0.0	5.0	3.4	0.0	0.0	1.8
Police protection	0.0	1.1	2.1	0.8	3.4	0.9	1.2
Garbage collection	0.0	1.9	0.0	0.0	0.0	3.5	0.9
Other	4.4	0.0	0.0	11.9	13.6	7.2	6.8
None	0.0	0.0	0.0	0.0	5.1	7.9	1.9

[a] $N=663$. Inter-community differences are statistically significant, by Chi-square test, at the .001 level. Column percentages may not sum to 100.0 due to rounding. Don't know/no response cases excluded. Question: "Think now about the most important needs and problems of [this community]. In your opinion, what are the most urgent or serious problems and needs of this community? [Probe:] Of these problems or needs which you have mentioned, which do you think is the most important?"

[b] Includes air pollution, insects, unstable subsoil conditions, and overcrowding.

The degree of importance attached by residents of the research communities to three particular needs—security of tenure, schools, and housing—merits further discussion. The intensity with which migrants lacking legal title to their land feel a need to secure such titles is illustrated by the responses to three additional questions asked in the Mexico City study. When asked whether they had been personally concerned or worried about any particular community-related problem during the last year or so, a larger proportion of the total sample mentioned security of land tenure than any other single problem. In response to another question, 30 per cent of the nonrenting migrants expressed the belief that their claim to their house and land was not yet secure, and the proportions of respondents holding this belief were much higher in Colonia Nueva and Colonia Periférico, the two communities whose legalization by the government was not yet complete at the time of the survey. Migrants residing in the research communities were also asked whether they had ever worked or cooperated with other residents of their community to try to solve some community problem. A plurality (28 per cent) of those who had done so reported that they had been concerned with the security of tenure problem.

The overriding importance attached to this problem by so many migrants residing in illegally formed communities is not surprising, since the opportunity afforded by such communities to become owner of a homesite constitutes one of the primary motivations for moving there in the first place. Apart from the important psychological satisfactions deriving from land ownership, it assures at least a modest property inheritance (a "*patrimonio familiar*") for one's children. But short-term economic advantages for the migrant and his family are equally important in explaining the high salience of the security of tenure problem. Most migrants residing in the communities included in this study build their own homes, an activity which represents their principal form of capital investment. The rent-free dwelling provides a source of economic security during periods of unemployment or rampant inflation.[13] Protection of this source of security requires official recognition of land tenure rights. Moreover, land titles are often essential for obtaining loans for home improvements from banks and other private lending institutions.

With regard to the need for more—or better—schools, the reader may find it surprising that individuals who perceive education as a key personal concern (see Figure 2) generally do not place schools at the top of the improvement agenda for their community. The simplest and most adequate explanation for this finding lies in the fact that even in low-income communities suf-

ary, 1970), pp. 217–218; Cornelius and Dietz, "Urbanization, Demand-Making, and Political System Overload," p. 8, Table 2; John F. C. Turner, "Barriers and Channels for Housing Development in Modernizing Countries," in *Peasants in Cities*, ed. William Mangin (Boston: Houghton Mifflin, 1970), pp. 8–9.

[13] See Andrews and Phillips, "The Squatters of Lima," p. 216.

fering from extreme service deprivation and insecurity of tenure, residents are usually able to construct their own make-shift school buildings, often using the same temporary materials as in their individual dwellings. Teachers may be obtained by petitioning the Ministry of Education or the Popular Sector of the official party. The federal government's program of school construction is also well developed in the Mexico City metropolitan area. Despite extreme overcrowding in many primary and secondary schools, the regime's performance in providing educational opportunities is more impressive than in any other problem area affecting the city's low-income neighborhoods.

One of the most striking findings of this and other studies of perceived needs among the urban poor in Third World cities relates to the very low priority attached by such people to housing needs. Only 3 out of 678 migrants interviewed in Mexico City specified housing as the most important need affecting people in their community of residence. It is clear that most of these individuals regard basic services and improvements such as piped water, sewerage systems, and paved streets as of more immediate importance in the creation of an adequate dwelling environment than a house built of permanent materials. Permanent construction of the house proceeds, but only at a pace commensurate with a family's financial resources. Most families are extremely reluctant to assume the continuing financial burden that occupancy of government-provided housing would impose upon them. Instead they prefer to build and improve their own houses as time and money become available, even though the basic construction period alone may last for 10 to 15 years.[14]

Politicized Needs. What, then, is the "citizen agenda" for governmental action among the migrant population? What kinds of needs do they perceive as susceptible to satisfaction through government resources? The migrants interviewed in Mexico City were asked to give their opinion of how well the government was doing in a wide variety of specific problem areas. They were also asked to specify those kinds of problems or needs for which they thought the government had no responsibility. The responses showed that while the migrants were somewhat hesitant to attribute responsibility to the government for providing economic help to those in need, providing employment, and redistributing wealth ("trying to even out the differences between the rich and poor classes in Mexico"), they overwhelmingly perceived the government as responsible for providing most kinds of community-related services and improvements.

The degree to which community-related problems and needs have been politicized among migrants to Mexico City is further illustrated by their responses to several questions about the role of government in community development. In answer to the question, "Who or what is to blame for most of the problems" of their community, nearly two thirds of the respondents mentioned "the government" in general, or some specific government agency or official. Responding to another question, only 14 per cent of the sample felt that the most important problems of their community could be solved by the residents themselves, without external assistance. When asked, "Who is it from outside of this community who may be of help in solving the kinds of problems you have mentioned," more than 97 per cent of the respondents mentioned some government agency or official. Only 1 per cent mentioned private sources of aid (business firms, private land developers, the Church, etc.), and less than 1 per cent viewed the official party (P.R.I.) as a potential source of aid.

The perceived importance of government assistance versus self-help in community development is also revealed by the migrants' responses to a fixed alternative question which required them to identify the single most important factor for improving living conditions in their community. As shown in Table 3, most migrants to Mexico City viewed government help as most important, followed by self-help efforts by the residents. Analysis by community of residence shows that even among Mexican migrants residing in squatter settlements, where the ethic of self-help is more deeply ingrained than in other types of low-income communities, government assistance is still regarded as paramount in the community development process.[15]

Very significant differences emerge on the issue of self-help versus government assistance in community problem-solving when the responses of migrants residing in the Mexican research com-

[14] *Ibid.*, p. 220; and Turner "Barriers and Channels in Housing Development," pp. 10–15.

[15] Squatter settlement residents in Ecuador, Chile, and Venezuela have also tended to place a relatively low valuation on self-help efforts. See Thomas M. Lutz, "Self-Help Neighborhood Organizations, Political Socialization, and the Developing Political Orientations of Urban Squatters in Latin America: Contrasting Patterns from Case Studies in Panama City, Guayaquil, and Lima" (Ph.D. dissertation, Georgetown University, 1970), pp. 156–158; and Talton F. Ray, *The Politics of the Barrios of Venezuela* (Berkeley: University of California Press, 1969), p. 85. (But compare, with regard to Venezuela, Kenneth L. Karst, Murray L. Schwartz, and Audrey J. Schwartz, *The Evolution of Law in the Barrios of Caracas* [Los Angeles: Latin American Center, University of California, 1973], pp. 50–51, 76–77.)

Table 3. Perceived Importance of Alternative Sources of Help in Community Development, Among Migrants to Mexico City, Rural Community Residents in Mexico, and Migrants to Lima, Peru[a]

Source of Help	Mexico City (N=676)	Rural Mexico (N=474)	Lima, Peru (N=328)
Government.....	40.5%	57.6%	37.8%
Community residents	28.4	29.3	57.9
God.....	17.5	10.1	2.4
Good luck.....	13.5	2.1	1.8

[a] Differences between migrants to Mexico City and Lima are statistically significant, by Chi-square test, at the .001 level.

Question: "Generally speaking, which is the most important for improving the conditions of life in this community: The hard work of the residents, God's help, the government's help, or good luck?"

munities are compared with those of low-income settlement residents in Lima, Peru. More than 30 per cent of the migrants to Lima believed that the most important problems of their community could be solved by the residents alone, as compared with only 14 per cent of the Mexican migrants. As shown in Table 3, the Peruvian respondents regard self-help as considerably more important to community development than government aid. Although differences in the distribution of responses within other categories ("God's help," "good luck") are also substantial, the even larger cross-national difference in the *relative* importance attached to self-help and government assistance is particularly relevant to our concern with the politicization of needs among migrants.

The most adequate explanation for this difference may lie in variations in the content of directed political socialization from one country to another. In both Mexico and Peru, the government has strongly influenced mass perceptions of the appropriate scope of its activities, although in very different ways. In Peru, most administrations since the late 1950s have sought to stimulate and capitalize upon the self-help efforts of low-income city dwellers. The self-help orientation was also nurtured by U.S. Peace Corps volunteers in low-income urban communities. Moreover, until 1968, when a leftist military regime came to power, the strong governmental emphasis on the need for self-help among residents of such communities was accompanied by a relatively low level of official responsiveness to their needs. Partly in response to these conditions, large and effective self-help projects were launched in many of the squatter settlements ringing large cities.[16]

In Mexico, the low propensity of the poor for self-help in both urban and rural areas (see middle column, Table 3) reflects the efforts of successive governments since 1940 to encourage a sense of mass dependence upon the regime for community improvements and other types of social welfare benefits. In fact, the response of Mexican government officials to community development projects initiated without their knowledge or involvement has often been one of anger or embarrassment.[17] The leaders of low-income urban communities, many of whom are tied to government bureaucrats and PRI functionaries through patron-client relationships, have also encouraged a sense of dependence upon government assistance among their followers.[18]

The cross-national evidence thus suggests that the range of needs perceived by cityward migrants as requiring governmental action is partly a function of the presence or absence of overt governmental attempts to create feelings of dependence among the lower classes. Where the official incentives to self-help have been weak or lacking altogether, as in Mexico and Venezuela, the range of politicized needs will be broader. However, the conversion of diffuse claims upon the government into specific demands communicated to public officials will be retarded by an emphasis on paternalism and dependence in the government's relationship with the poor.

The responsiveness of the political system to petitioning by low-income migrants may also condition their attitudes toward reliance on government aid. In countries where competitive party systems have induced incumbent governments to be more responsive to petitions from low-income

[16] See Sandra Powell, "Political Participation in the Barriadas: A Case Study," *Comparative Political Studies*, 2 (July, 1969), 211; David Collier, "Squatter Settlement Formation and the Politics of Co-optation in Peru" (Ph.D. dissertation, University of Chicago, 1971), p. 76; Donald L. Maruska, "Government Policy and Neighborhood Organizations in the Squatter Settlements of Lima" (Honors Thesis, Department of Government, Harvard University, 1972), p. 6; Alfredo Rodríguez et al., "De invasores a invadidos," *Revista Latinoamericana de Estudios Urbano Regionales*, 2, No. 6 (November, 1972), 101–142; Henry A. Dietz, "The Office and the Poblador: Perceptions and Manipulations of Housing Authorities by the Lima Urban Poor," paper presented at the Meeting of the American Society for Public Administration, Los Angeles, Calif., April 3, 1973, pp. 3, 10–11; and Sara Michl, "Urban Squatter Organization as a National Government Tool: The Case of Lima, Peru," in *Latin American Urban Research, Volume III*, ed. Francine R. Rabinovitz and F. M. Trueblood (Beverly Hills, Calif.: Sage Publications, 1973), pp. 155–178.

[17] Similar responses have been observed among Venezuelan officials. See Ray, *Politics of the Barrios of Venezuela*, pp. 89–90.

[18] See Wayne A. Cornelius, "Contemporary Mexico: A Structural Analysis of Urban Caciquismo," in *The Caciques: Oligarchical Politics and the System of Caciquismo in the Luso-Hispanic World*, ed. Robert Kern (Albuquerque, N.M.: University of New Mexico Press, 1973), pp. 145–146.

urban communities, as in Chile since the early 1960s and Venezuela since 1958, residents of such communities have devoted most of their energies to petitioning rather than self-help efforts.[19] In the Mexican case, while competition for the votes of low-income city dwellers from opposition parties has been minimal, the regime's emphasis on support-building among the poor has nevertheless ensured a relatively high level of responsiveness to the needs of low-income urban communities.[20]

But even in communities where a strong emphasis on self-help has taken root, due either to government neglect or the attitudes of local leaders, the long-term trend appears to be toward increasing dependence upon governmental assistance. This trend is well illustrated by the data from the Mexico City research communities reported in Table 4. Attitudes and behavior supportive of self-help efforts are most prevalent in the most recently established and most poorly developed squatter settlements (Colonia Nueva and Colonia Periférico). In Colonia Militar, a squatter settlement established about 20 years ago, only a third of the respondents currently exhibit a propensity for self-help efforts.

As Lisa Peattie has observed on the basis of field work in a low-income urban neighborhood in Venezuela, such a community "has, of necessity, only limited power to solve its own problems," and "may in the end find itself powerless to solve what it sees as problems because the solutions lie at points which [it] cannot control."[21] Thus community residents may cooperate to install a provisional water system, but an adequate supply of potable water must depend upon permission to tap a large public water main in a nearby area. Permanent solutions to many such problems of community development must await governmental action. Particularly in older squatter settlements, the residents reach a point where they view their own efforts and resources as inadequate to satisfy their needs.

Perception of Channels for Demand Making. Whether politicized needs will be translated into actual demands upon government often depends on the ability of the potential demand maker to perceive an adequate channel for influencing government decisions. Though migrants to the city may take it upon themselves individually to contact government officials to present requests for aid, most data from Latin American cities (including my own) indicate that such individual demand-making activity is quite infrequent, and that the overwhelming majority of contacts with officials are made by the migrant as a member of a delegation of residents from his community. Since demand-making groups are typically recruited from among the members of community improvement associations, and are led by elected officials of such associations, the importance of community organizations and their leaders to demand-making activity among the migrant population becomes obvious. If no such leadership and organizational resources are available to the migrant, he is much less likely to engage in any form of political demand making.

[19] See Samuel P. Huntington and Joan M. Nelson, "Socio-Economic Change and Political Participation: Report to the Civic Participation Division of the Agency for International Development," Center for International Affairs, Harvard University, September, 1973, p. 5–33.

[20] For a fuller discussion of the dimensions of governmental performance affecting low-income residents of Mexico City, see Wayne A. Cornelius, "The Impact of Governmental Performance on Political Attitudes and Behavior: The Case of the Urban Poor in Mexico City," in *Latin American Urban Research, Volume III*, ed. Rabinovitz and Trueblood, pp. 213–251.

[21] Lisa R. Peattie, *The View from the Barrio* (Ann Arbor: University of Michigan Press, 1968), p. 67.

Table 4. Self-Help Orientation Among Migrants to Mexico City, by Community[a]

Index score[b]	Colonia Nueva	Colonia Periférico	Colonia Texcoco	Colonia E. Propio	Unidad Popular	Colonia Militar
0 or 1.....	32.4%	50.0%	64.3%	65.0%	84.7%	64.6%
2.....	67.6	50.0	35.7	35.0	15.3	35.4

[a] $N = 678$. Inter-community differences are statistically significant, by Chi-square test, at the .001 level.

[b] Self-help orientation is measured by a summative index constructed from responses to the following questions:

"When you think of problems of the sort you have mentioned . . . can they be solved by those living within the community, or would help be needed from outside the community?"

Scoring: *1 point* "Can be solved by those living within" *0 points* "Help needed from outside"

"Have you ever worked or cooperated with other residents of this community to try to solve some problem or meet some need of the community?"

Scoring: *1 point* "Yes" *0 points* "No"

Numerous studies of political participation in both Third World and advanced industrial nations have demonstrated that those who are involved in any type of formal voluntary organization participate in politics more frequently than citizens who are not so involved.[22] These studies have also shown that organizational involvement may lead to increased political participation *in the absence* of other personal attributes or attitudes such as high socioeconomic status, a sense of political efficacy, or a high level of political information. Moreover, there is specific evidence from studies of low-income communities in Santiago, Chile, and Lima, Peru, indicating that members of community improvement organizations are considerably more likely to engage in political demand making than are their neighbors who have not been involved in improvement organizations.[23] Among the migrants interviewed in Mexico City, those who had participated in community improvement organizations were *five times* more likely to have engaged in demand making than the nonparticipants.

Participant observation in the Mexican research communities suggests that community improvement organizations provide important psychological support for the low-income migrant seeking to make demands upon government. Participation in communal demand-making efforts greatly reduces the amount of individual initiative required, and provides the potential demand maker with a clear strategy of action for influencing government decisions.[24] Moreover, to the extent that community-based organizations are successful in securing benefits through collective demand-making attempts, they strengthen the migrant's perceptions of the political system as being subject to manipulation and increase his sense of political efficacy. Such attitudes and perceptions increase his propensity to engage in future demand-making attempts.

Participation in collective demand making channeled through community organizations may not, however, increase the migrant's confidence in dealing with government officials as an individual. Community improvement organizations in low-income urban neighborhoods seem to affect the attitudes of their members in much the same way that certain types of peasant unions do in rural areas:

> The syndicate is something concretely visible and understandable to peasants. No ability to imagine oneself individually trying to influence national government is required for a sense of efficacy. A peasant need only learn that the syndicate exists as broker between himself and the government—and this lesson is learned by observing and especially by participating in the brokerage activities of the union. . . . Consequently, if the union is adept at brokerage . . . there is a greater likelihood that its members will be taught that they are, in fact, efficacious.[25]

This helps to explain why so few of the migrants to Mexico City who have engaged in collective demand-making efforts have *also* attempted individually to contact government officials about personal or family-related problems (see next section). Individual demand making with a purely particularistic referent apparently requires a different set of perceptions of the political system and one's competence to influence its outputs—perceptions which do not necessarily result from collective demand-making experiences.

Political Demands. In most countries for which reasonably comparable data are available (see Table 5), demand making appears to be a relatively uncommon form of political participation among the general population. Even in the United States, only about a third of the population report that they have contacted a government official at least once. Among migrants interviewed in Mexico City, about one out of four has contacted a political or governmental official on at least one occasion.[26] The incidence of demand-making

[22] See especially Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963), pp. 262–265; William Erbe, "Social Involvement and Political Activity: A Replication and Elaboration," *American Sociological Review*, 29 (April, 1964), 198; Norman H. Nie, G. Bingham Powell, and Kenneth Prewitt, "Social Structure and Political Participation: Developmental Relationships," *American Political Science Review*, 63 (September, 1969), 813; William H. Form and Joan Huber, "Income, Race, and the Ideology of Political Efficacy," *Journal of Politics*, 33 (August, 1971), 688; Verba and Nie, *Participation in America*, pp. 174–208; and Paul Burstein, "Social Structure and Individual Political Participation in Five Countries," *American Journal of Sociology*, 77 (May, 1972), 1087.

[23] James W. McKenney, "Voluntary Associations and Political Integration; An Exploratory Study of the Role of Voluntary Association Membership in the Political Socialization of Urban Lower-Class Residents of Santiago, Chile, and Lima, Peru" (Ph.D. dissertation, University of Oregon, 1969), p. 184; Daniel M. Goldrich, "Political Organization and the Politicization of the Poblador," *Comparative Political Studies*, 3 (July, 1970), 192; and Raymond B. Pratt, "Community Political Organizations and Lower Class Politization in Two Latin American Cities," *Journal of Developing Nations*, 5 (July, 1971), 536–537.

[24] Cf. Goldrich, "Political Organization," p. 192; Pratt, "Community Political Organizations," p. 537.

[25] John R. Mathiason and John D. Powell, "Participation and Efficacy: Aspects of Peasant Involvement in Political Mobilization," *Comparative Politics*, 4 (April, 1972), 327.

[26] Due to the wording of the relevant survey questions, all of these reported contacts with officials involved a personal visit to the office of some functionary of the government or the official party. Thus the data do not reflect contacting through letters or

activity varies considerably from one community to another. The proportion of migrants who have engaged in some form of demand making varies from 45 per cent in Colonia Nueva (the most recently established squatter settlement) to 10 per cent in Colonia Texcoco (the commercial subdivision). These inter-community differences can be attributed partly to the nature of developmental problems which each community has confronted (e.g., demand making has been more frequent among migrants residing in illegally formed settlements, due to extended negotiations for government recognition of tenure rights); but differences in community leadership and organizational structure appear to be equally important determinants of demand-making propensities among the residents.

When migrants contact government officials, what is the substantive nature of their demands? The relevant data for the Mexico City sample are presented in Figure 3. Nearly two-thirds of the reported contacts with officials were concerned with the need for security of tenure. In the period immediately following a land invasion, demands relating to this need are defensive in nature—i.e., requests that the settlers be permitted to remain on the land without fear of eviction by the government. Subsequently the tenure-related demands take the form of petitions for legalization of the community and the granting of individual land titles to its residents. Petitions relating to other kinds of community needs account for most of the remaining demand-making attempts reported by the respondents.

In their study of a national sample of the U.S. population, Verba and Nie found that about one-third of all citizen-initiated contacts with government officials were concerned with particularistic problems relating only to the individual demand-maker or his family, while the remaining two-thirds had a broader social referent.[37] A similar pattern emerges from the data on demand making by migrants to Mexico City, although the frequency of contacting with regard to particularistic problems is even lower among this population than among U.S. citizens. Only about 3 per cent of the contacts with government officials reported by the migrants dealt with personal or family-

petitions signed by community residents and sent to government officials. The actual wording of the questions is as follows: "Have you ever gone to an office of the P.R.I.? [If Yes:] For What purpose?"; "Have you ever personally—either alone or with other residents of the community—gone to see some official of [in Colonia Texcoco: the Municipal Government; elsewhere: the Department of the Federal District], or some other person of influence in the government, about some problem or need?"

[37] Verba and Nie, *Participation in America*, p. 67.

Table 5. Frequency of Citizen-Initiated Contact With Government Officials in Mexico, Peru, Chile, Brazil, United States, and India

Country	%*	Sample Composition
Mexico[a]	25.8	Lower-class male, migrant heads of family in Mexico City (six low-income neighborhoods)
Mexico[b]	20.0	Lower-class adult residents of city of Jalapa
Mexico[c]	17.0	Male household heads in four rural communities, State of Michoacán
Peru[d]	32.3	Lower-class male, migrant heads of family in four squatter settlements of Lima
Peru[e]	5.9	Lower-class male household heads in Lima
Chile[f]	11.1	Lower-class male household heads in Santiago
Brazil[g]	42.0	Lower-class male residents of squatter settlements in Rio de Janeiro
U.S.[h]	33.0	National sample of adult population
U.S.[i]	27.0	Registered voters in New Haven, Connecticut
U.S.[j]	27.5	Adult residents of Nashville, Tennessee.
U.S.[k]	12.0	Adult residents of five low-income urban neighborhoods in several cities
India[l]	20.0	National sample of adult population

* Percent who have contacted a government official on one or more occasions.

[a] Cornelius, "Poverty and Politics in Urban Mexico."

[b] Fagen and Tuohy, *Politics and Privilege in a Mexican City*, p. 89.

[c] Arterton, "Patterns of Political Participation in Four Rural Mexican Villages."

[d] Dietz, "Becoming a Poblador."

[e] McKenney, "Voluntary Associations and Political Integration," p. 181.

[f] McKenney, "Voluntary Associations and Political Integration," p. 181.

[g] Janice E. Perlman, "The Fate of Migrants in Rio's Favelas" (Ph.D. dissertation, Massachusetts Institute of Technology, 1971), p. 317.

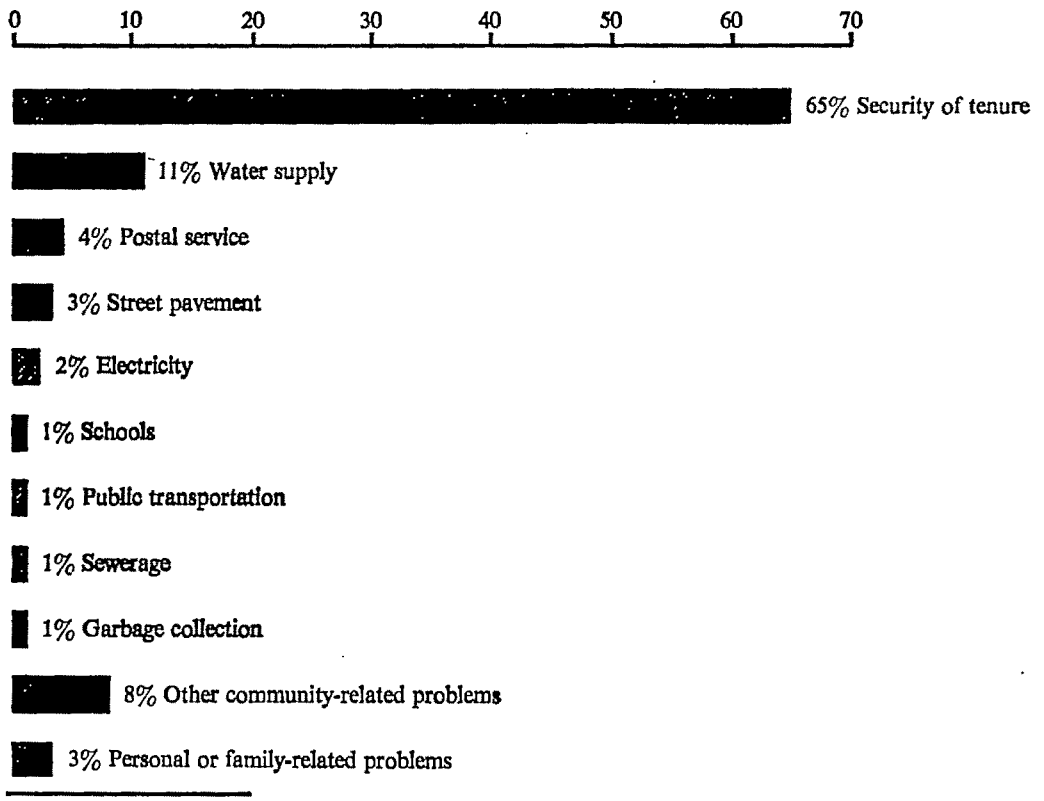
[h] Verba and Nie, *Participation in America*, p. 106.

[i] Robert Dahl, *Who Governs?* (New Haven: Yale University Press, 1961), p. 279.

[j] Graham and Pride, "Styles of Political Participation," p. 41.

[k] Curt Lamb, "Political Culture in Poor America," cited in Perlman, "The Fate of-Migrants in Rio's Favelas," p. 316.

[l] John O. Field, "Partisanship in India: A Survey Analysis" (Ph.D. dissertation, Stanford University, 1973), p. 355A.



[a] $N=112$. Don't know/no response cases excluded from table.

Figure 3. Subject Matter of Citizen-Initiated Contacts with Government Officials, Among Migrants to Mexico City.[a]

related needs.[28] Thus it is clear that the migrant's agenda for governmental action is strongly influenced by the priority he attaches to certain problems and needs which affect him most directly as a member of a given residential community. Most of the demands emanating from the migrant population are demands articulated in the name of an entire community, for collective goods benefitting all residents of the community. Highly particularistic requests for jobs or other special dispensations benefitting only the individual demand maker or his family constitute a very small proportion of migrant demands upon government.

This low frequency of particularistic contacting also reflects the migrant's perceptions of what types of needs are most amenable to satisfaction through governmental action. Perhaps the most important reason that individual needs for housing and employment are not frequent objects of demand making is that the migrant usually perceives the satisfaction of such needs as an *individual* responsibility.[29] As government-employed

[28] Among the reported contacts with functionaries of the official party, about 13 per cent had a particularistic (personal or family) referent; but contacts with P.R.I. officials represent only 9 per cent of the total number of demand-making attempts reported by the respondents.

[29] Fagen and Tuohy also found that among residents of the city of Jalapa, Mexico, "the major source of personal difficulties—the economic situation, and specifically the matter of jobs—is still widely perceived in the first instance as a private or individual domain. Individual mobility in the context of aggregate national growth is seen as the mechanism through which economic conditions will be ameliorated. A sense of personal blame and responsibility thus colors perceptions of these problems. . . . When Jalapeños were asked, 'If a man is unemployed, who has the chief responsibility for finding work for him?,' over 70 per cent said that the man himself had that responsibility. Five per cent said 'friends and relatives' and 23 per cent mentioned the government or some other organization. . . . Despite substantial diffuse reliance on government, economics has not been connected with politics in ways that lead the average citizen to seek political redress for economic grievances" (Fagen and Tuohy, *Politics and Privilege in a Mexican City*, pp. 142–143). Similarly privatized perceptions of the problem of unemployment have been observed among residents of squatter settlements in Lima, Peru (Rodríguez et al., "De invasores a invadidos," p. 131).

engineers supervising the construction of housing by residents of some low-income settlements in Mexico City have discovered, the migrant may actively resist governmental involvement in house construction, which he regards as interference in a personal matter. Alternatively, despite the urgency of such problems as unemployment, seeking government assistance may not be viewed as an effective way to deal with the problem, either because of the complexities and delays involved in obtaining government help or because of low governmental responsiveness to petitions concerned with individual needs. By contrast, securing land tenure rights and obtaining access to basic urban services are problems not viewed as being susceptible to "individual" solution. Even the most affluent residents of illegally formed communities in Mexico City have found it impossible to acquire title to their land through individual negotiations with government officials.[30] Security of tenure is achieved only through a government decision to recognize the land occupancy rights of all residents of a community; and such a decision is almost always the result of collective demand-making attempts by the residents.

Strategies of Demand Making

What methods are likely to be employed by migrants engaged in demand-making attempts? To help identify their preferences among a number of alternative demand-making strategies, migrants interviewed in Mexico City were presented with the following hypothetical situation: "Suppose a group of people in this city strongly feels that the government is treating them unfairly. I'm going to read a list of some of the things these people might do to try to get the government to change the situation. Which do you think would be the most effective way?" The responses, reported in Table 6, indicate a very marked preference for working through conventional bureaucratic channels (specifically, by sending a group representative to talk with a government official).

Table 6. Preferred Strategy of Demand Making Among Migrants to Mexico City[a]

Sending a representative to a government office	47.9%
Working through personal connections with public officials	25.2
Working through the official party (PRI)	11.5
Organizing a protest demonstration	9.5
Organizing public meetings or rallies to get other people interested in the problem	5.8

[a] $N = 545$. Don't know/no response cases excluded from table.

Nearly half of the respondents felt that this would be the most effective strategy for influencing the government. Since investigation of past and current demand-making efforts of residents of the research communities revealed that they often sought to circumvent regular bureaucratic processes by entering into patron-client relationships with key politicians or bureaucrats, this was also specified as an alternative in the survey question regarding demand-making strategies. About one in four of the respondents endorsed this type of approach, defined in the question as "working through personal connections with public officials." Much smaller proportions of respondents asserted the efficacy of working through the official party to influence the government's actions, organizing a protest demonstration, and trying to exert pressure on the government by mobilizing the support of people outside the demand-making group through public meetings or rallies.

Parties as Demand Articulators. Working with party politicians in order to influence decisions of the government bureaucracy is a strategy employed frequently among the poor in some Latin American cities, primarily those in which competitive multi-party systems are operative.[31] Yet data from several of these cities reveal considerable disillusionment with political parties as

Such perceptions and expectations may help to explain the rarity of collective political action organized around the issue of unemployment among low-income residents of Latin American cities. Cf. Peter C. W. Gutkind, "From the Energy of Despair to the Anger of Despair: The Transition from Social Circulation to Political Consciousness among the Urban Poor in Africa," *Canadian Journal of African Studies*, 7, No. 2 (1973), 179–198.

[30] Individual negotiations to secure land titles have apparently been feasible and successful in some Third World cities with large squatter populations. See, for example, Joan M. Nelson, "Migration, Integration of Migrants, and the Problem of Squatter Settlements in Seoul, Korea: Report on a Field Study for the Smithsonian Institution," mimeographed, Washington, D.C., July, 1972, p. 20.

[31] See Peattie, *The View from the Barrio*, p. 67; Anthony Leeds, "The Significant Variables Determining the Character of Squatter Settlements," *América Latina*, 12 (July-September, 1969), pp. 78–79; and David G. Epstein, *Brasilia: Plan and Reality* (Berkeley and Los Angeles: University of California Press, 1973), pp. 113, 130–131. Cf. Aprodicio A. Laquian, "Slums and Squatters in South and Southeast Asia," in *Urbanization and National Development: South and Southeast Asia Urban Affairs Annual, Vol. I*, ed. Leo Jakobson and Ved Prakash (Beverly Hills, Calif.: Sage Publications, 1972), p. 199; and Kemal H. Karpat, "The Politics of Transition: Political Attitudes and Party Affiliation in Turkish Gecekondu," unpublished paper, Department of History, University of Wisconsin, Madison, 1972, p. 26.

articulators of lower-class needs.[32] The findings from Mexico City are consistent with this general Latin American pattern. As shown in Table 6, the official party is not widely viewed by migrants residing in the research communities as an effective vehicle for demand making, especially when the matter at issue is some community problem or need. Informal interviews with community residents and their leaders further revealed that working with opposition political parties is not regarded as a rational strategy of goal attainment. These parties have no independent resources of utility to low-income communities; and their electoral weakness vis-à-vis the PRI insures that they have no chance of gaining control of the government agencies which do control the allocation of resources to such communities.

Despite the absence of a credible threat to PRI hegemony, the Mexican regime continues to place a high premium upon maintaining or even increasing the official party's already overwhelming margin of support at the polls. Yet low-income communities find it difficult or impossible to take advantage of this emphasis on voter mobilization to further their interests. Most *colonias proletarias* in Mexican cities are relatively small, by comparison with similar communities in other Latin American cities, and hence unable to deliver a substantial bloc of votes to the PRI. Moreover, the boundaries of electoral districts are rarely coterminous with the boundaries of specific *colonias;* hence electoral statistics are not gathered and reported in such a way as to accrue to the credit or discredit of a given *colonia.* In short, such communities typically have little or no bargaining power with government officials based on a capacity to grant or withhold electoral support from the regime.

In some provincial Mexican cities, the official party has been observed to function effectively as "a decentralized clearinghouse through which [lower-class] demands can be articulated and, at times, attended to."[33] There is, however, considerable variation in the performance of this function by the PRI, from city to city and even within single cities. For example, in some sections of the Mexico City metropolitan area, community improvement organizations are tied administratively into the local PRI structure, and direct their petitioning for community improvements toward the Party office. Where such direct ties do not exist, demand making is not usually filtered through the Party apparatus. In general, working exclusively through PRI functionaries is viewed by residents of the *colonias proletarias* as a highly indirect strategy of demand making. They believe that better results can be achieved through direct contacting of government officials, relying on official party contacts only in the event that negotiations with government decision-makers break down or produce an unfavorable outcome. Such beliefs reveal a highly realistic perception of the locus of decisional authority in the Mexican political system: It is government bureaucrats rather than party politicians who control the allocation of resources most relevant to the solution of community problems.[34]

The PRI, mainly through its Popular Sector, is the source of a limited amount of patronage for individual residents of low-income neighborhoods. It may, for example, provide medical assistance, assist in obtaining a license or permit for commercial activity, or help to enroll a child in an already overcrowded neighborhood school. But as we have already noted, demand making with an individualized referent is very infrequent among the people included in this study, regard-

[32] See Peattie, *The View from the Barrio,* pp. 69–70; Ray, *Politics of the Barrios of Venezuela,* pp. 80, 98–127; Powell, "Political Participation in the Barriadas"; Lutz, "Self-Help Neighborhood Organizations," pp. 194–195; Raymond B. Pratt, "Parties, Neighborhood Associations, and the Politicization of the Urban Poor in Latin America," *Midwest Journal of Political Science,* 15 (August, 1971), 509–510; Pratt, "Community Political Organizations," p. 538; John D. Uzzell, "Bound for Places I'm Not Known To: Adaptation of Migrants and Residence in Four Irregular Settlements in Lima, Peru" (Ph.D. dissertation, University of Texas, Austin, 1972), p. 240; Susan Eckstein, "The Poverty of Revolution: A Study of Social, Economic, and Political Inequality in a Central City Area, a Squatter Settlement, and a Low Cost Housing Project in Mexico City" (Ph.D. dissertation, Columbia University, 1972).

[33] Fagen and Tuohy, *Politics and Privilege in a Mexican City,* pp. 30–31. Cf. Antonio Ugalde, *Power and Conflict in a Mexican Community* (Albuquerque: University of New Mexico Press, 1970), pp. 139–149; and Antonio Ugalde, Leslie Olson, David Schers, and Miguel Von Hoegen, *The Urbanization Process of a Poor Mexican Neighborhood: The Case of San Felipe del Real Adicional, Judrez* (Austin, Texas: Institute of Latin American Studies, University of Texas at Austin, 1974).

[34] Drawing primarily upon research conducted in the provincial city of Jalapa, Mexico, one investigator has concluded that "Only persons and organizations which lack adequate political resources articulate demands to and through the Party. And since it is not a major decision-maker, those articulators can only hope for the Party's services as a broker or intermediary connected with the government." See William S. Tuohy, "Centralism and Political Elite Behavior in Mexico," in *Development Administration in Latin America,* ed. C. E. Thurber and Lawrence Graham (Durham, N.C.: Duke University 1973). See also David Schers, *The Popular Sector of the Partido Revolucionario Institucional in Mexico* (Tel Aviv: The David Horowitz Institute for the Research of Developing Countries, Tel Aviv University, Research Report No. 1, November, 1972), p. 68.

less of the type of official (PRI or government) toward which it is directed.

The Politics of Protest. The low degree of efficacy attached by migrants to protest tactics as a strategy of demand making is particularly significant, in view of the very extensive literature attributing to cityward migrants a high potential for protest behavior. In response to another interview question, almost half of the migrants interviewed in Mexico City expressed the belief that it would be wrong for a man to join in a protest march or demonstration "to get public officials to correct an unjust situation." The migrants were also quick to condemn, by a margin of 59 per cent to 15 per cent (with 26 per cent expressing no opinion), the protest activities of students who demonstrated against government violations of university autonomy and other grievances in Mexico City during the fall of 1968. Not surprisingly, levels of migrant involvement in overt protest activities were quite low. Only 3 per cent had ever gone with a group of people to protest or complain to a public official about some problem, and only about 2 per cent of the migrants had ever attended a protest meeting or participated in a protest demonstration. In my interviews with community leaders, several expressed the view that if a need of their community were particularly acute and the government had ignored community petitions regarding the need, they would try to organize a protest demonstration outside the office of some high-ranking government functionary; but none were able to document an instance in which this kind of action was actually taken.

It is also noteworthy that even in the context of conventional petitioning activity, communications by residents of the research communities were never formulated as protests, complaints, or expressions of anger at being deprived unjustly of some particular service or government benefit.[35] Both the written petitions and the language used in presenting them to government officials are replete with expressions of deference toward authority and expressions of hope that the benefits being sought will be granted by the government as a "special favor" to residents of the community.

The migrant's deferential, non-threatening approach to negotiating with public officials, as well as his avoidance of protest tactics in general, are rooted in a world view which emphasizes "conformity to the rules rather than manipulation of them."[36] Lower-class migrants to Latin American cities, like 19th-century immigrants to the United States, tend to view themselves as

> foreigners to the city. As such, they are more prone, at least initially, to question the legitimacy of their own presence as audacious newcomers than the general urban order. The city which offers the opportunity of upward [socioeconomic] ascent also sets its own hard rules. The migrant's decision to come to the city, like the immigrant's choice of settling in a new country, implies a tacit acceptance of these rules.[37]

Thus the migrant's preference for demand making through conventional bureaucratic channels or informal clientage relationships can be viewed as a reflection of his commitment to abiding by the political rules of the game in his new environment. Clearly illegal or violent forms of political action are never considered as a means of bringing pressure to bear on the authorities, because they represent a fundamental violation of these rules. Participation in a land invasion is usually the only instance in which the migrant may be willing to deliberately contravene social and political norms. And since such invasions are often carried out with the covert support of politicians or government officials, it is unlikely that they are even construed by many of the participants as a form of anti-system political action.[38]

The migrant's low tolerance for the risks and uncertainties of unconventional political action also reflects his perceptions of what kinds of demand-making strategies are most *instrumental* in gaining tangible benefits for himself and his community. He sees little or nothing to be gained by offending the authorities through protest activities, and perhaps a great deal to lose by doing so.

[35] The reluctance of residents of low-income neighborhoods to formally protest or complain about deficiencies in public services is strikingly reflected in a recent compilation of data on citizen complaints received by the city government and Mexico City newspapers and television stations. During the period from January through June, 1973, more than 61 per cent of the complaints received were concerned with trash collection (a problem with very low priority in most low-income neighborhoods); and more than three-quarters of all complaints came from people residing in predominantly upper or middle-income sections of the city. See *El Día* (Mexico City), July 8, 1973, p. 13.

[36] Douglas Butterworth, "Two Small Groups: A Comparison of Migrants and Non-Migrants in Mexico City," *Urban Anthropology*, 1 (Spring, 1972), 41.

[37] Alejandro Portes, "Rationality in the Slum," *Comparative Studies in Society and History*, 14 (June, 1972), 284.

[38] On the involvement of government bureaucrats and politicians in urban land invasions in Chile, Peru, and Venezuela, see Franz Vanderschueren, "Political Significance of Neighborhood Committees in the Settlements of Santiago," in *The Chilean Road to Socialism*, ed. Dale L. Johnson (Garden City, N.Y.: Doubleday-Anchor Books, 1973), pp. 267–268; Collier, "Squatter Settlement Formation and the Politics of Co-optation in Peru," *passim;* and Ray, *Politics of the Barrios of Venezuela*, pp. 33–37, 42–43.

Examples of both the potential costs and the futility of protest activity as a strategy of goal attainment abound in recent Mexican history. In the countryside, open protest by peasants has been systematically and ruthlessly suppressed; and the 1968 protest demonstrations in Mexico City were violently repressed by police and military forces, resulting in the deaths of up to 300 persons and the sentencing of students accused of organizing the demonstrations to 15-to-20-year prison terms. (Most of them had been released by 1971.) More lives were lost in June, 1971, when a group of high-level government and PRI officials sponsored an attack by a terrorist street gang upon student demonstrators in the capital. Since 1968 perhaps a dozen more student protesters have been killed by riot police in Puebla, Culiacán, and other provincial cities.[39] These events, extensively reported in the mass media, have greatly increased popular awareness of the formidable government coercive apparatus which can be activated in response to overt protest. As in some contemporary U.S. cities where protest has been met with violent actions by the authorities, such actions are likely to reinforce tendencies toward less aggressive strategies of political demand making among low-income sectors of the population.[40]

Moreover, from the standpoint of the community leaders who typically organize demand-making efforts, protest-oriented strategies are risky and unproductive because they may damage the clientage relationships with government bureaucrats or politicians which they have labored so long to establish. Overt protest actions threaten the reputations of such officials as "capable managers" and hence, their prospects for future advancement within the regime.[41] When confronted with such a threat, an official is more likely to respond with retaliatory measures than with positive action on the offending community's requests for assistance. Henceforth the community's leaders are perceived as troublemakers and are likely to be refused access to the offended official if further contact is attempted.

Finally, the demonstrated efficacy of *conventional* demand-making strategies in securing community improvements serves to reduce the attractiveness of protest tactics among both community leaders and their followers. In Mexico City, the authorities have worked diligently to cultivate a popular image of accessibility and responsiveness to lower-class needs, even though their responses to petitions from low-income groups more often take the form of symbolic reassurances (e.g., making inspection tours, committing the problem at hand to technical study) than allocations of substantive benefits. Petitioners from low-income neighborhoods are seldom refused access to lower or middle-echelon government officials; and those who have visited a government or PRI office overwhelmingly report that they were treated courteously and fairly by the officials they contacted.[42] Furthermore, more than two-thirds of the migrants interviewed in Mexico City who had participated in demand-making attempts report that such attempts were successful in the sense of having contributed to the solution to some community problem. Table 7 summarizes the outcomes of demand-making attempts by residents of the six research communities over the period since settlement of the communities began, as reported in interviews with community leaders. Although these leaders are more likely to view any benefit received by their communities as the outcome of demand-making attempts rather than through governmental initiative, detailed investigation—including interviews with the relevant government decision makers—suggests that most of the benefits cited by community leaders would not have been provided in the absence of overt demand-making efforts.[43]

[39] In a recent analysis of historical trends in the use of violence as a repressive tool by Mexican authorities, Evelyn P. Stevens notes that such practices are still fairly common at the local level in rural areas and even at the level of state governments in some parts of the country. See E. P. Stevens, *Protest and Response in Mexico* (Cambridge, Mass.: M.I.T. Press, 1974), chap. 9. The use of small-scale violence against peasants in rural areas is discussed further in James D. Cockcroft, "Coercion and Ideology in Mexican Politics," in *Dependence and Underdevelopment: Latin America's Political Economy*, ed. J. D. Cockcroft, Dale L. Johnson, and Andre Gunder Frank (Garden City, N.Y.: Doubleday-Anchor Books, 1972), p. 254. Cf. Fagen and Tuohy, *Politics and Privilege in a Mexican City*, p. 27.

[40] See Peter K. Eisinger, "The Conditions of Protest Behavior in American Cities," *American Political Science Review*, 67 (March, 1973), 27. Fear of physical and economic coercion has also depressed political participation among minority groups in the U.S. South. See the empirical analysis reported in Lester M. Salamon and Steven van Evera, "Fear, Apathy, and Discrimination: A Test of Three Explanations of Political Participation," *American Political Science Review*, 67 (December, 1973).

[41] Fagen and Tuohy, *Politics and Privilege in a Mexican City*, p. 71.

[42] See Cornelius, "Impact of Governmental Performance on Political Attitudes and Behavior," p. 224.

[43] Lest it be assumed that the research communities in Mexico City have benefitted from an atypically high level of governmental responsiveness, it should be noted that most of the benefits which they have received are the outcomes of difficult and protracted demand-making experiences, often extending over a period of five to ten years. Moreover, many serious needs of the communities have gone unmet. Even today, functioning sewerage systems, schools, public markets, and health care facilities are inadequate or non-existent in three of the six communities. In some cases these unmet needs reflect

Table 7. Outcomes of Demand-Making Efforts, by Community

Colonia Nueva	Colonia Periférico	Colonia Texcoco	Colonia E. Propio	Unidad Popular	Colonia Militar
Destruction of stone wall erected by former landowner to restrict access to community	Introduction of regular bus service	Installation of preliminary electricity supply system	(None—water and sewerage systems, electricity, and other services and improvements installed at government initiative)	Installation of water purification plant	Government recognition of land tenure rights
Reconstruction of houses destroyed by fire	Installation of provisional water system	Construction of secondary school		Postponement of property tax payments	Installation of electricity for individual dwellings and street lighting
Expropriation of land occupied by community (first step toward legalization of community)	Construction of primary school	Installation of provisional water supply system		Construction of 2 primary schools, 1 secondary school	Installation of sewerage system
Construction of primary school	Installation of regular electricity supply system and street lighting	Construction of child-care center		Installation of several public telephones	Construction of primary school
Installation of regular electricity supply system	Pavement of main street			Paving of some streets	Paving of streets
	Installation of 1 public telephone				Installation of public and private telephones
	Expropriation of land occupied by community				

Even the residents of communities whose petitions for recognition of tenure rights or urban improvements have not yet elicited positive responses from the government are aware of nearby communities which *have* been so favored. These examples foster perceptions of the political opportunity structure as being relatively open, thus eliminating the necessity for resort to protest tactics. Migrants residing in the city's low-income neighborhoods are thus likely to feel that by working together they are capable of influencing at least some of the decisions of government affecting the development of their community; and most believe that they can do so through conventional, nonconflictual means.[44] If his community's petitions are consistently denied by the government, and deeply felt needs seem likely to remain unsatisfied in the foreseeable future, the migrant is far more likely to respond to the situation by moving to another community with better developmental prospects than by engaging in some form of protest activity.[45]

Conventional Demand-Making Strategies. The most commonly used strategy among the migrant poor in Mexico City is that of forming a committee or delegation of community residents to visit the office of a government official and present a formal request for assistance. What kinds of officials are the primary targets of such petitioning? In nearly half of the demand-making attempts reported by migrants residing in the six research communities, some high-ranking official within the Department of the Federal District (D.D.F.) had been contacted. (The D.D.F. serves as the principal organ of local government for Mexico City and its immediate metropolitan area.) Another 31 per cent of the demand-making attempts had been directed toward the head of the Office of Colonias, an appendage of the D.D.F. having primary responsibility for the problems of low-income neighborhoods of the city. Only 9 per cent of the reported contacts with officials involved a functionary of the official party, and only 6 per cent involved a *delegado*

an absence or suspension of demand-making efforts by residents of the community; in others, a lack of skill or commitment among community leaders has led to unsuccessful negotiations with the authorities. Still other unmet needs reflect a sheer lack of responsiveness by government officials to persistent demand-making attempts.

[44] Cf. Alejandro Portes, "Cuatro poblaciones: informe preliminar sobre situación y aspiraciones de grupos marginados en el gran Santiago," Centro de Estudios Socio-económicos de la Universidad de Chile, August, 1969, pp. 39–39A.

[45] Of course, numerous factors beyond governmental neglect may contribute to a decision to abandon a community suffering from insecurity of tenure or service deprivation. A useful conceptualization of the cost/benefit calculations performed by low-income migrants to Lima, Peru, with regard to intracity residential mobility is provided in Douglas Uzzell, "The Interaction of Population and Locality in the Development of Squatter Settlements in Lima," in *Anthropological Perspectives on Latin American Urbanization: Latin American Urban Research, Volume IV*, ed. Wayne A. Cornelius and Felicity M. Trueblood (Beverly Hills, Calif.: Sage Publications, 1974). Much of Uzzell's analysis seems applicable to the population under study in this paper. Cf. John M. Orbell and Toro Uno, "A Theory of Neighborhood Problem-Solving: Political Action vs. Residential Mobility," *American Political Science Review*, 66 (June, 1972), 471–489.

(a lower-echelon official of the D.D.F., who is nominally responsible for all residential zones within one of the major political subdivisions of the Federal District).

The migrant's choice of demand-making targets exhibits an accurate understanding of the highly centralized nature of the government decision-making process in Mexico. In attempting to influence the outcomes of this process, "both efficiency and security argue that it is wise to seek support and decisions as close to the center as possible lest time be wasted with those who don't count and whose decisions may subsequently be overturned."[46] Thus residents of a community seeking a water supply system may send copies of their petition to a *delegado*, to the head of the city's water department, and to other middle-echelon officials, but will seek to negotiate directly with the Governor of the Federal District or one of his closest deputies. Direct approaches are also made occasionally to the First Lady or an assistant to the President of the Republic, despite the rarity with which audiences are granted by these individuals. Residents of one of the six research communities, located just outside the Federal District in the State of Mexico, often took advantage of the public audiences held each Saturday by the second-highest official of the state government to present petitions for assistance, rather than channeling them through subordinate officials of the municipal government. Of course, access to officials with personal political power and decisional autonomy is more difficult to obtain; yet the potential rewards flowing from contacts with such officials are likely to be far more substantial and immediate than any benefits provided by their subordinates.

In presenting a request for government assistance, community leaders take pains to emphasize the willingness of the residents to pay for any services extended to them ("We don't want you to 'give' us anything . . . "). Since residents of low-income communities in Mexico City are in fact required by the government to repay the full cost of installing basic urban services and improvements (usually through individual resident assessments, to be paid over a period of five to ten years), evidence of previous, successful fund-raising campaigns to finance community improvements may be presented to help assure officials of the community's credit-worthiness. Offers of free manual labor to help in the installation of improvements are also made routinely. Occasionally, gifts or outright cash bribes are presented to an official with whom negotiations are being conducted.

If these and other inducements fail to produce the desired results, the demand makers may attempt to exert indirect pressure on the government by publicizing their needs through the mass media.[47] One major newspaper in Mexico City maintains a section for "complaints and petitions" directed at government officials by residents of predominantly low-income neighborhoods; and delegations of residents from such communities frequently visit the newspaper's offices to present copies of their petitions to the government or request news coverage of the situation in their community. The resulting stories may be highly embarrassing to unresponsive bureaucrats. Another indirect tactic involves asking official party functionaries to intercede with government officials responsible for acting on their petitions.

Negotiations for official recognition of land tenure rights and installation of basic urban services are usually quite complex and require frequent attention by the demand makers over a long period of time. A government decision to "regularize" an illegally formed community must be followed by a number of additional governmental acts before individual tenure rights can be considered secure. These actions include expropriation of the land, if privately owned before it was invaded; initiation of technical studies to determine the feasibility of installing certain types of services and improvements; completion of a census of current occupants of land within the community; subdivision of the land into parcels assigned to individual residents; and determination of land prices, service assessments, and terms of payment. At any of these stages in the "regularization" of the community, serious difficulties and delays may develop; hence the necessity for numerous follow-up visits to check on the processing of the community's petitions and cultivate personal relationships with the officials in charge.[48]

[46] Fagen and Tuohy, *Politics and Privilege in a Mexican City*, p. 23. For more detailed discussions of the mechanisms and consequences of political centralism in Mexico, see Tuohy, "Centralism and Political Elite Behavior in Mexico"; William L. Furlong, "Obstacles to Political Development; Case Studies of Center and Periphery in Northern Mexico," paper presented at the Annual Meeting of the American Political Science Association, Washington, D.C., September, 1972; and Susan K. Purcell and John F. H. Purcell, "Community Power and Benefits from the Nation: The Case of Mexico," in Rabinovitz and Trueblood, eds., *Latin American Urban Research, Volume III*, pp. 49–76.

[47] Cf. Peattie, *The View from the Barrio*, p. 75; Ray, *Politics of the Barrios of Venezuela*, pp. 96–97.

[48] Repeated contacts with the same official also serve importantly to fix in his mind the history and problems of the demand-making community, thus enabling him to distinguish more readily the petitions of that specific community from those of numerous similar communities competing for his attention. See Dietz, "The Office and the Poblador," p. 20.

During this follow-up period, the demand makers take advantage of every possible opportunity to renew their claims. Officials making inspection tours, social service workers of the ruling party, or politicians on electoral campaign visits may be confronted by a delegation of residents from the demand-making community to draw attention to their petitions. A disaster of some sort —particularly if on a scale sufficient to warrant coverage by the mass media—provides additional opportunities to dramatize the demand makers' plight and bring pressure to bear on the authorities to grant their requests. For example, a fire set by the original owner of the land occupied by Colonia Nueva squatters, resulting in the death of seven people, greatly facilitated that community's negotiations for official recognition of tenure rights.

The competition for government attention from many other communities whose needs are equally acute is often fierce, but persistence and skill in negotiating with officials can often make a great difference, particularly in a city where the allocation of government benefits to low-income areas is not governed by routinized, rationalistic developmental criteria. In their study of politics in the city of Jalapa, Mexico, Fagan and Tuohy noted a total absence of developmental planning of the sort that might provide criteria for bringing together and judging conflicting claims on scarce resources.[49] Much the same situation prevails in Mexico City, where the idea of comprehensive physical planning, even as an eventual goal, was discarded shortly after World War II and never revived.[50] Neither has there been any government office in the capital having responsibility for overall planning and resource allocation for low-income areas of the city. Many government agencies and individual politicians and bureaucrats act, or claim to act, in behalf of the *colonias proletarias*.[51]

These conditions are clearly reflected in the highly erratic, idiosyncratic responses made by officials to petitions from low-income neighborhoods within the city. Official behavior toward such communities has fluctuated sharply from overt hostility to benevolent permissiveness with top-level changes in city government personnel. Stubborn refusals to urbanize a squatter settlement on technical grounds that topography or subsoil conditions would make the introduction of basic services prohibitively expensive have given way virtually overnight to a decision to provide all necessary services.

While affording certain opportunities to lower-class petitioners, the absence of formally established guidelines by which needs are evaluated and the availability of multiple access points within the government bureaucracy may create other kinds of obstacles, especially for low-income communities whose leaders have failed to develop connections with high-ranking officials. The particularism of bureaucratic behavior "makes it hard for the [petitioner] to get a firm commitment on the part of any agency. He may even find it hard to find out what agency policy is, because agency policy may not exist; it may be that various bureaucrats each have their own mutually conflicting policies. . . . In such a system, it is quite true that an agreement with one official one day does not commit the agency" to any given course of action.[52]

Conclusion: Anticipating Demands and System "Overload" Due to Migration

We have observed that most demands articulated by migrants to the city are concerned with very concrete problems, close to the individual's own life space. But what happens after the most immediate, parochial, and acutely felt needs of migrants for such things as land titles and basic urban services are satisfied? Should we expect some sort of "spill-over" of demand making into other issue areas of broader social and economic import, such as inflation, unemployment, or inequalities in the country's distribution of wealth?

One prominent student of the urban poor in Asian countries has hypothesized that as economic and social conditions in low-income neighborhoods improve, there will be a shift in the role

[49] Fagen and Tuohy, *Politics and Privilege in a Mexican City*, pp. 28–29.

[50] See Robert C. Fried, "Mexico City," in William A. Robson and D. E. Regan, eds., *Great Cities of the World*, 2 vols., 3rd ed. (Beverly Hills, Calif.: Sage Publications, 1972), p. 680; Francine F. Rabinovitz, "Urban Development Decision-Making in the Mexican Federal District," in *Programs for Urban Development in Latin America* (Washington, D.C.: U.S. Agency for International Development, 1965); and Louis Harris, "Government for the People of Mexico City" (Ph.D., dissertation, University of California, Los Angeles, 1956), chap. 5.

[51] One investigator has noted that functionaries of the official party are aware that neighborhood leaders "know very well how to make use of this situation [to] 'get things done'" (Schers, *The Popular Sector of the Partido Revolucionario Institucional*, pp. 71–72). Other observers have also documented the advantages offered to petitioners from low-income neighborhoods in Lima, Peru, and Guatemala City, by ambiguities in the jurisdictions of various government officials and agencies. See Dietz, "The Office and the Poblador"; Douglas Uzzell, "Bureaus and the Urban Poor in Lima," paper presented at the Annual Meeting of the American Anthropological Association, New Orleans, November-December 1973; and Bryan R. Roberts, "The Interrelationships of City and Provinces in Peru and Guatemala," in *Anthropological Perspectives on Latin American Urbanization: Latin American Urban Research, Volume IV*, ed. Cornelius and Trueblood.

[52] Peattie, *The View from the Barrio*, p. 89.

of community improvement associations from "selfishly particularistic pressure groups to more general welfare-oriented lobbies." He notes, however, that this may require "a generation of educated, upwardly mobile slum children" whose concerns extend beyond the developmental problems of their neighborhoods.[53]

The demand-making activity observed among migrants to Mexico City and other major Latin American metropolitan areas lends little support to such expectations. After several decades of large-scale migration to these cities, demand making by residents of communities populated mostly by low-income migrants or their offspring remains confined to highly parochial, small-scale needs.[54] Moreover, demand making tends to fall off sharply within these communities once security of land tenure has been assured and basic urban services have been introduced.[55] In many cases, community improvement organizations have been created solely for the purpose of negotiating for land titles or some other specific benefit. When this purpose was fulfilled, the organizations lost their *raison d'être* and quickly disintegrated. Examples of community organizations which originally had a narrowly-defined function and subsequently evolved into more general-purpose lobbying groups are extremely rare.[56]

In assessing the potential impact of cityward migration on demand making aimed at local and national governments, one must also take into account the limited *scope* of the demand-making activity in which migrants are likely to engage. Attempts to aggregate demands across communities in which migrants take up residence are almost nonexistent, as are attempts by such communities to influence the content or scale of government policies and programs. For example, community-based organizations often negotiate for government recognition of the land tenure rights of their constituents, but almost never seek to alter government policies regarding land use or the legalization of squatter settlements in general.[57] Thus most demand making by low-income migrants to the city represents an attempt to influence the government decision-making process on the *output* (policy implementation) side.[58]

These observations are quite consistent with those of students of the urban poor in the United States, who often stress the difficulties of organizing the poor around larger policy issues.[59] Yet

[53] Laquian, "Slums and Squatters in South and Southeast Asia," pp. 199–200.

[54] Data from the Mexico City study also indicate that the adult sons of migrants to the city are less likely than their parents to engage in demand making and other forms of political participation. See Wayne A. Cornelius, "The Cityward Movement: Some Political Implications," in *Changing Latin America: New Interpretations of Its Politics and Society*, ed. Douglas A. Chalmers (New York: Academy of Political Science, Columbia University, 1972), pp. 38–39.

[55] See, for example, Goldrich, "Political Organization," pp. 186–187.

[56] Cf. Marc H. Ross, "Community Formation in an Urban Squatter Settlement," *Comparative Political Studies*, 6, No. 3 (October, 1973), 312–313.

[57] This pattern is often reinforced by private and public development agencies operating in low-income communities. Such agencies encourage a perception of the community as an entity isolated from the rest of the city in which it is located, with problems and solutions particular to that community. Commenting on the activities of "community development" and social service workers in squatter settlements of Lima, Peru, one investigator notes that by relating the settlements to a larger social reality, such workers might enable the residents to understand that the problems experienced by their community (e.g., insecurity of land tenure) are caused by basic problems at the city or national level (e.g., inequities in the land tenure system). Instead, they tend to define the problems confronting a settlement in highly parochial terms, thus "insuring . . . that the residents' organization will be limited to demand making focused only on immediate [community-specific] interests" (Rodríguez, "De invasores a invadidos," pp. 113, 130–131).

[58] Although we lack specific data on this point, it could be argued that their behavior is conditioned importantly by recognition of the low receptivity of Mexican political leaders to influence attempts on the "input" side of the decision-making process. As Robert Scott has recently observed, "Mexico's present political system is based on a petitioning pattern and not a bargaining process. Most citizens still see government (or more precisely, some individual, particularly the President) as dispensing favors to a petitioner on a personal and paternalistic basis rather than as a mechanism that implements policy decisions reached by a ritualized legal process of give and take among competing interests. So do most party and government functionaries." See Robert E. Scott, "Politics in Mexico," in *Comparative Politics Today: A World View*, ed. Gabriel A. Almond (Boston: Little, Brown, 1974).

[59] See, for example, Arthur Hillman and Frank Seever, "Elements of Community Organization," in Fred M. Cox et al., eds., *Strategies of Community Organization* (Itasca, Ill.: Peacock, 1970), pp. 284–285; Ralph M. Kramer, *Participation of the Poor* (Englewood Cliffs, N.J.: Prentice-Hall, 1969), pp. 235–237; and John H. Mollenkopf, "On the Causes and Consequences of Neighborhood Political Mobilization," paper presented at the Annual Meeting of the American Political Science Association, New Orleans, La., September, 1973. The present study does indicate, however, that low-income migrants in Mexico City possess a greater capability for organizing themselves for demand making with regard to *community-specific* needs and problems than has often been observed among the urban poor in the United States. For example, contrast the preceding discussion of demand-making skills and experience among residents of the communities included in this study with the following description, drawn from a study of neighborhood self-improvement associations in U.S. cities during the 1960s: "They had not developed clear goals. Interviews with club members, as well as those with the community organizers, indicated that *most of the club members had no con-*

there are a few Latin American cases of low-income urban groups which represent important exceptions to this general pattern, to the extent that they have addressed themselves from time to time to concerns transcending their immediate, parochial needs for security of tenure and basic services. These include a substantial number of squatter settlements ("*campamentos*") in Chilean cities organized in the early 1970s by members of the extreme-left MIR (Movement of the Revolutionary Left), MAPU (a party originating from a 1969 split in the Christian Democratic Party), and some elements of the Socialist and Communist parties, as well as some urban neighborhoods in Cuba in which residents were organized into Committees for the Defense of the Revolution.[60] While the Cuban committees have often served primarily as instruments of political control or "popular mobilization" in support of the regime, the squatter *campamento* organizations in Chile were ostensibly aimed at stimulating demand making based on perceptions of class interests and class solidarity. They were viewed by their organizers as "instruments of political education" which would stimulate demands for government benefits going far beyond land titles and community services.

During the administration of President Salvador Allende, delegations of residents from the *campamento* settlements in Santiago and other Chilean cities carried out numerous *tomas* (temporary seizures or occupations) of government buildings (including ministries), streets, and other public facilities to force officials to receive and act upon demands for assistance.[61] While many—perhaps a majority—of these instances of direct action were motivated by the needs of the settlements for water and sewerage systems, schools, health care facilities, and other local improvements, other *tomas* were staged to protest high prices, shortages of food and other commodities, and unemployment. Some *campamentos* in Santiago also organized committees to seek jobs for unemployed residents through conventional contacting of politicians and government bureaucrats. In sum, the organizers of these communities drew the attention of the residents to a variety of large-scale problems which were manifested concretely at the community level, while encouraging them to view these problems as national in scope, origin, and import. Perhaps even more importantly, the Allende regime took a generally permissive stance toward demand making by the poor, regardless of the subject matter of the demands or the tactics employed in articulating them. Occasionally local authorities retaliated against squatters involved in a *toma* with tear gas and arrests, but in general this type of demand-making activity was tolerated under the Unidad Popular regime, and the resulting negotiations between squatters and government officials often produced prompt and acceptable solutions to the problems at issue, or at least their local manifestations.

The case of Chile under Allende suggests that if migrants and other elements of the urban poor in Latin American cities are to be involved substantially in any kind of demand-making activity

ception of the type of activities necessary to put pressure upon governmental or commercial agencies, to say nothing of knowing how to organize and implement such activities. . . . They apparently lack knowledge of how to deal effectively with the sources of dissatisfaction. Even very small and limited goals are rarely accomplished. . . . Drop-outs are frequent. . . . [The attempt] to develop local neighborhood self-improvement associations through the efforts of an indigenous community organizer . . . was a failure." See Walter Gove and Herbert Costner, "Organizing the Poor: An Evaluation of a Strategy," *Social Science Quarterly*, 50 (December, 1969), 643–656.

[60] On the Chilean *campamentos*, see James F. Petras, "Chile: Nationalization, Socio-Economic Change, and Popular Participation," in *Latin America: From Dependence to Revolution*, ed. J. F. Petras (New York: Wiley, 1973), pp. 58–60; Vanderschueren, "Political Significance of Neighborhood Committees," pp. 281–282; Joaquin Duque and Ernesto Pastrana, "La movilización reivindicativa de los sectores populares en Chile, 1964–1972," *Revista Uruguaya de Ciencias Sociales*, 1, No. 2 (August-October, 1972); Equipo de Estudios Poblacionales del CIDU, "Pobladores y administración de justicia," *Revista Latinoamericana de Estudios Urbano Regionales*, 3, No. 5 (July, 1972), 135–148; Equipo de Estudios Poblacionales del CIDU, "Reivindicación urbana y lucha política: Los campamentos de pobladores en Santiago de Chile;" *Revista Latinoamericana de Estudios Urbano Regionales*, 3, No. 6 (November, 1972), 55–81; Manuel Castells, Luis Alvarado, Santiago Quevedo, Eder Sader, Jorge Fiori, and Ignacio Santa María, *Revista Latinoamericana de Estudios Urbano Regionales*, 3, No. 7 (April, 1973), 9–112 (special issue on low-income urban settlements in Chile). The following discussion of demand making by *campamento* residents is based primarily on these sources and supplemental information provided by Richard R. Fagen. For a detailed analysis of demand making by squatter settlement residents in Santiago during the Frei administration (1965–1970), which stresses the nearly total preoccupation of the demand makers with land tenure problems, see Peter S. Cleaves, "Bureaucratic Politics and Administration in Chile" (Ph.D. dissertation, University of California, Berkeley, 1972), pp. 548–606. On political organization of low-income neighborhoods in Havana, Cuba, see Douglas Butterworth, "Grass Roots Political Organizations in Cuba: The Case of the Committees for the Defense of the Revolution," in Cornelius and Trueblood, eds., *Latin American Urban Research, Volume IV*, chap. 8.

[61] In Chile the word "*toma*" is also used to refer to illegal seizures of land for permanent occupation by urban squatters. These *tomas* do not represent political demand making as defined in this paper, although they may *lead* to such behavior, with the object of securing land titles, basic urban services, and other improvements for the invaded land.

beyond that related to immediate, community-specific problems, it will come only as a result of intensive, sustained mobilization by leaders of political groups external to the low-income communities themselves. Moreover, this mobilization must occur in the context of a national political environment which is supportive of aggressive demand making by low-income groups, in a wide range of issue areas. Demands concerning non-parochial needs are likely to be made by residents of low-income communities only when they feel that the risk of violent repression by the authorities is minimal and that the likelihood of getting some redress of grievances is quite high. The amount of time and energy involved in petitioning public officials is too great to be squandered upon matters which are not regarded by the government as legitimate objects of demand making by the poor. In the absence of both external mobilization efforts and perceptions of governmental receptivity to non-parochial, "class-based" or cross-community demand making, the potential for such activity among the migrant poor will remain very low.[62]

The evidence presented above indicates that low-income migrants to the city should not be viewed as a "non-demanding" sector of the population, even though the demand making in which they engage is considerably less frequent than has often been assumed. However, the demands which they make upon the political system remain highly parochial and limited in scope. They are *the kinds of demands which can be satisfied most easily by incumbent authorities* without fundamental changes in government priorities or patterns of resource allocation. A political system in which most participation by the urban poor is confined to contacting officials about community-related needs is one which tends toward preservation of the basic sociopolitical order:

> Pressures which might otherwise take collective form, and be directed to earlier stages of policy formation or to the composition of the government itself, are diverted into discrete, separable, and small-scale demands which can be met in full or in part or rejected one by one. . . . Even where the incidence of neighborhood organization is high, . . . the sporadic nature of small-scale independent collective action means that only a fraction of all associations are likely to press for assistance at one time.[63]

Rapid urbanization has undoubtedly increased the number of demands made upon governments in Latin American and other Third World countries. But apocalyptic visions of mushrooming popular demands threatening to overwhelm local or national political systems as urbanization proceeds are clearly premature, if not altogether unfounded. Most of the demands on Latin American governments stemming from large-scale redistribution of population from rural to urban areas are, regrettably, those which are least likely to threaten an inequitable distribution of power and privilege in a society.

[62] The obstacles to demand making by low-income migrants based on perceived class interests are legion, but perhaps the most important derive from the extreme occupational heterogeneity of the migrant population and their concentration within the tertiary sector of the urban economy. For discussions of these and other factors inhibiting class-based demand making by the poor in contemporary Latin American cities, see Daniel Goldrich, "Toward the Comparative Study of Politicization in Latin America," in *Contemporary Cultures and Societies of Latin America*, ed. Dwight B. Heath and Richard N. Adams (New York: Random House, 1965), pp. 368–371; Alaine Touraine and Daniel Pécaut, "Working-Class Consciousness and Economic Development in Latin America," *Studies in Comparative International Development*, 3, No. 4 (1967–68), 71–84; Pablo González Casanova, "Dynamics of the Class Structure," in *Comparative Perspectives on Stratification*, ed. Joseph A. Kahl (Boston: Little, Brown, 1968), pp. 76–80; James F. Petras, *Politics and Social Structure in Latin America* (New York: Monthly Review Press, 1970), pp. 13–23; Roger Hamburg, "Urbanization, Industrialization, and Modernization in Latin America: Soviet Views," *Studies in Comparative Communism*, 5 (Spring, 1972), 18–19; Huntington and Nelson, "Socio-Economic Change and Political Participation," pp. 5–21—5–29; and Richard Sandbrook, "The Working Class in the Future of the Third World," *World Politics*, 25 (July, 1973), pp. 469–478.

[63] Huntington and Nelson, "Socio-Economic Change and Political Participation," p. 5—51. Bryan Roberts, on the basis of field work in Guatemala City and Lima, Peru, concludes that even though residents of low-income neighborhoods in these cities are often able to take advantage of ambiguities in the urban power structure to obtain needed services, these manipulative strategies "have little lasting impact on the existing structure of power or on the distribution of resources. . . . The major impact of these local-level political activities . . . is that of creating a state of political uncertainty for those who control political and economic resources." See Roberts, "The Interrelationships of City and Provinces in Peru and Guatemala," and *idem, Organizing Strangers: Poor Families in Guatemala City* (Austin: University of Texas Press, 1973), pp. 307–337.

Clientelist Politics in the Philippines: Integration or Instability?*

THOMAS C. NOWAK AND KAY A. SNYDER
Michigan State University

Introduction

The central question about political instability is not so much whether it occurs, but how it occurs. While the causes of political instability have been widely discussed,[1] it is less clear how these causes interact in particular contexts. Nor is it always clear what forms such instability will take. In this paper we examine the causes and consequences of political change in the Philippines, and suggest that certain nonviolent forms of competition may reflect conditions conducive to political instability.

Changes in nonviolent political competition (e.g., more competitive elections) are seldom viewed as an erosion of legitimacy in cases such as the Philippines, where elections were regularly conducted within a stable party system.[2] Yet whenever political competition reflects intra-elite rivalries within a clientelist structure of political mobilization,[3] it is crucial to determine whether or not changes in the pattern of competition are indicative of changes in the integrative capacity of clientelist structures.

Electoral institutions, when imposed on a feudal social structure like that found in the Philippines, relied on pre-existing patron-client bonds and incorporated them into a party organization. Under such conditions the legitimacy of the government derives in large part from the legitimacy accorded patrons by clients. Whether the disruption of traditional relationships reflects a decline in the legitimacy of government depends in large part on how rapidly and effectively alternative structures are provided for channeling sufficient resources to clients.

A deterioration in conditions supporting clients' claims for economic and social security is usefully analyzed from a class perspective in societies such as the Philippines. In the Philippines, the emergence of a cohesive landed class set the basis for further accumulation by such a class in industry and commerce.[4] To accumulate surplus more effectively and shift capital into these increasingly profitable activities, national elites desire to structure exchange relationships in specialized contractual ways.[5] In so doing national elites become increasingly freed from the informal distributive pressures common in more diffuse forms of patron-client relationships.

The free enterprise ideology absorbed by the Filipino elite through the American colonial experience assigns a prominent role in economic development to the private sector, both local and foreign. When a free enterprise system is imposed on a feudal social structure and bonds between patron and clients weaken, inequality is likely to increase since the restraints on the increased expropriation of surplus by patrons are lessened. Because the Philippines has a labor surplus economy, allowing relatively free play of supply and demand keeps wages low and allows capital to

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[1] See, for example, Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), pp. 32–92, and Ted Robert Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970).

[2] Increased support for extremist parties is often acknowledged as a sign of discontent and potential erosion of legitimacy. Other than the Democratic Alliance (a coalition of peasant groups in central Luzon, urban middle-class elements and some Communists, whose representatives were barred from holding office after the 1946 election), there have been no extremist parties participating in Philippine elections.

[3] There is a rapidly growing body of literature dealing with patron-client relationships. The literature most applicable to our analysis includes: Carl Landé, "Networks and Groups in Southeast Asia," *American Political Science Review*, 67 (March, 1973), 103–127; James C. Scott, "Patron-Client Politics and Political Change in Southeast Asia," *American Political Science Review*, 66 (March, 1972), 91–113; and by the same author, "Corruption, Machine Politics, and Political Change," *American Political Science Review*, 63 (December, 1969), 1142–1158.

[4] Harry Benda, "Political Elites in Colonial Southeast Asia: An Historical analysis," *Comparative Studies in Society and History*, 7 (April, 1965), 233–251. See also Robert Reed, "Hispanic Urbanization in the Philippines," *Journal of East Asiatic Studies* (March, 1967), 1–213.

[5] See Mary Hollnsteiner, "Reciprocity in the Lowland Philippines," in *Four Readings on Philippine Values*, ed. Frank Lynch and Alfonso De Guzman (Quezon City: Ateneo de Manila Press, 1970), p. 85.

accumulate. If such capital accumulation is part of a dynamic, self-sustaining pattern of economic growth, market forces will ultimately raise the cost of labor. If on the other hand such capital accumulation is not part of a dynamic process of growth, deteriorating "terms of trade" between patron and client are likely to persist and to continue favoring the patron.

Compensatory mechanisms can alleviate the deterioration in relative position of clients which frequently occurs during early industrialization. As the economy becomes differentiated and national political and economic power is centralized, integration of local clientelist systems into the national political system occurs through a pyramiding of clientelist structures. Such a vertical integration makes possible the provision of national resources to clients. In electoral clientelist systems such as existed in the Philippines, an extension of the exchange network provided the conduit through which national patronage resources could be bartered for electoral support.[6]

Whether or not the change in patron-client structures to a more specialized political machine proceeds smoothly depends in large part on the capacity of the government to extract and distribute resources. If sufficient resources are allowed to move from the center to local areas, political machines should prove capable of integrating and mobilizing large groups of people.[7] We shall argue that such a pattern of resource capability and distribution does not exist in the Philippines. In so doing we shall examine the nature of capital concentration, economic change, and the capacity of the public sector to extract and distribute resources. To further explore the conditions leading to the declining strength of political machines, we offer hypotheses which focus on: (1) the relationship of capital concentration to economic diversification and industrialization; (2) the relationship of economic structure and sociodemographic structure; and (3) the relationship of economic and socio-demographic structure to the strength of political machines.

Social Structure and Capital Concentration

The historic role of the bourgeoisie in Western Europe as a capitalist class antagonistic to old feudal elites has no counterpart in the Philippines. While the process of industrialization in Western Europe and the United States marked the triumph of industrial elites over feudal agrarian ones,[8] no such conflicts among elites appear forthcoming in the Philippines. Philippine industrial entrepreneurs had not developed into a distinct group, but rather tend to come from families of the older agrarian and commercial elites who increasingly diversify wealth into a variety of advanced as well as traditional enterprises.[9] While there is occasional mobility into the national elite by persons of more modest means, the basic trend in the Philippine elite has been toward consolidation and concentration. That is, a dispersed tribal nobility evolved into a largely Spanish and Chinese Mestizo landed and commercial elite, joined after the turn of the century (once political power became available and educational opportunities expanded) by some indigenous Malay elements. A basic homogeneity of interest among this emergent national elite has been facilitated by increased intermarriage among the different ethnic strands (e.g., Chinese-Malay and Spanish-Malay).[10]

By stimulating a particular pattern of economic diversification through easy credit and import controls, monetary and fiscal policy in recent years has further increased the economic power of the national elite. It was feared that the devaluation and abandonment of controls in 1962 would threaten the existence of Filipino enterprises long nurtured by protection and subsidization, yet most Filipino enterprises managed to survive.[11] Diversification of familial economic power decreases susceptibility to fluctuations in world prices, potential loss of privileges in the U.S. market, and local policy changes such as devalua-

[6] See Carl H. Landé, *Leaders, Factions, and Parties: The Structure of Philippine Politics*, Yale University Southeast Asia Studies Monograph Series No. 6 (New Haven: Yale University, Southeast Asia Studies, 1966).

[7] See René Lemarchand, "Political Clientelism and Ethnicity in Tropical Africa: Competing Solidarities in Nation Building," *American Political Science Review*, 66 (March, 1972), 86.

[8] Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy* (Boston: Beacon, 1966), chapter 3.

[9] John Carroll's data on the socioeconomic origins of Filipino entrepreneurs leads him to conclude that "the old elite did not precisely 'withdraw . . .' it remains, in proportion to its size, the most fruitful single source of manufacturing entrepreneurs." See John J. Carroll, *The Filipino Manufacturing Entrepreneur* (Ithaca: Cornell University Press, 1965), p. 100. Laurence Davis Stifle, *The Textile Industry —A Case Study of Industrial Development in the Philippines, Data Paper No. 49* (Ithaca: Cornell Southeast Asia Program, 1963), p. 93, notes that for textile entrepreneurs ". . . entrepreneurship has been a means of preserving or consolidating a realized upper stratum position." Dante C. Simbulan, *A Study of the Socio-Economic Elite in Philippine Politics and Government 1946–1963*, (Ph.D. dissertation, Australia National University, 1965) notes the fused nature of the Philippine social, political and economic elite.

[10] See Simbulan, pp. 107–110.

[11] See Frank Golay, "The Philippines," in *Underdevelopment and Economic Nationalism in Southeast Asia*, ed. Frank H. Golay, Ralph Anspach, M. Ruth Pfanner, and Eliezer B. Ayal (Ithaca: Cornell University Press, 1969), p. 103.

tion, which hurt some sectors more than others. We therefore hypothesize that:

H1: Capital concentration increases with economic diversification and industrialization.

Economic Stagnation and Government Capacity

It is important to point out not only the existence but also the nature of capital concentration. Filipino entrepreneurs continue to invest heavily in the profitable primary sector and also in urban real estate and services—investments which do not generate extensive "backward" linkages and employment.[12] While reliance on the protected American sugar market is no longer as crucial a source of elite surplus as in previous years, it continues to be highly lucrative. Additionally, although certain government policies encourage economic diversification, the most extensive foreign and local credit continues to be available to dollar- or yen-earning export industries such as lumber and mining. The removal of subsidies for capital equipment in the early 1960s raised the cost of capital, yet did not significantly stimulate the growth of an indigenous capital-producing sector. Consequently, in recent years the growth in manufacturing has slowed as investors turn to more profitable extractive activities such as logging. Hicks and McNicoll have found that a significant proportion of recent economic growth derives from rapid expansion of activity in the primary sector (e.g., mining and lumber), which has limited long-term growth potential as natural resources are increasingly depleted.[13]

Since a dynamic pattern of economic growth has not occurred in the Philippines, considerable government intervention is necessary to attenuate an increasing loss of security for broad groups of people.[14] Yet it is questionable whether the Philippine government is capable of such intervention. The public sector's share in the total supply of final goods and services has remained virtually constant since 1950 and is low among Southeast Asian countries.[15] One reason is that the government is heavily dependent on indirect excise taxes which are relatively regressive and inelastic.[16] Patronage resources have become increasingly scarce in relation to the obligations incurred. In the last twenty years, for example, the proportion of public works appropriations which were actually released to agencies has fallen approximately 15 per cent.[17] The negative correlation between the rate of population growth from 1960 to 1970 in chartered cities and the rate of growth in total government expenditures per capita from 1961 to 1969 ($r = -.36$) suggest that local government capabilities are not expanding rapidly enough. Time lag correlations, while producing slightly more optimistic results, are not significant. The rate of population growth from 1948 to 1960 correlates +.15 with the rate of growth in total government expenditures per capita from 1961 to 1969.[18]

The inevitable recourse to deficit financing characteristic of Philippine election years raises further questions about the capability of the government. In each of the six election years between 1957 and 1968, the level of net government receipts declined.[19] This pattern of deficit financing has both aggravated inflationary pressures and diminished net international reserves. Between 1956 and 1962 net reserves fluctuated but were

[12] The concept "backward linkages" is explicated in Albert O. Hirschman, *The Strategy of Economic Development* (New Haven: Yale University Press, 1958).

[13] George L. Hicks and Geoffrey McNicoll, *Trade and Growth in the Philippines* (Ithaca: Cornell University Press, 1971), p. 105.

[14] The real wage of wage earners in selected non-agricultural industries in the Philippines dropped 1.1 per cent from 1957 to 1969. Calculated from Central Bank of the Philippines, *The Statistical Bulletin*, 21 (December, 1969), Tables 24 and 127. Between 1956 and 1971 the income concentration ratio in the Philippines increased from .452 to .455. Calculated from Bureau of Census and Statistics, *Survey of Households, Family Income and Expenditures: 1957* and Bureau of Census and Statistics, *Survey of Households, Family Income and Expenditures: 1971*. The concentration ratio is obtained by dividing the area bounded by the line of equal distribution and the Lorenz curve of income distribution by the total area under the line of equal distribution. The ratio therefore varies from 0 to 1, and the higher the value the greater will be the inequality in the distribution of income.

[15] See United Nations, Economic Commission for Asia and The Far East, *Economic Survey of Asia and the Far East 1961* (Bangkok: United Nations, 1962), p. 77 and *Economic Survey of Asia and The Far East 1966*, p. 24.

[16] Jesus P. Estanislao, *Economics of a Nation* (Manila: Center for Research and Communication, 1970), p. 122 and 151.

[17] Calculated from the Philippine Department of Public Works and Communications, *Annual Report, 1969–1970*, p. 38. Included in the early years (1954–1958) are Public Works Acts Nos. 1200, 1411, 1613 and 1900. Included in the later years (1959–1965) are Public Works Act Nos. 2093, 2301, 2701 and 3101. To include more recent acts would artificially accentuate these differences since releases for such acts are still occurring.

[18] The rate of population growth between 1960 and 1970, and between 1948 and 1960, is computed from the Republic of the Philippines Bureau of Census and Statistics, *Census of the Philippines, Population and Housing* for 1948, 1960 and 1970. The rate of growth in total government expenditures per capita between 1961 and 1969 is computed from statements on expenditures at the General Auditing Office of the Philippines.

[19] Harvey A. Averch, Frank Denton, and John Koehler, *The Matrix of Policy in the Philippines* (Princeton: Princeton University Press, 1971), p. 101.

always positive. By contrast, net reserves from 1963 to 1968 were negative for three of the six years under consideration.[20]

Economic Structure and Sociodemographic Structure

The relationships of economic complexity and limited forms of industrialization to increased social mobilization and urbanization in the Philippines differ in important respects from the relationships among such variables in the West. In the Western model urbanization is strongly related to industrialization—although it is recognized that in some instances manufacturing may follow as well as "pull" workers, and urban areas may develop around commerce and service as well as manufacturing.[21] Certain forms of urbanization in non-Western societies are more likely to occur separated from industrialization.[22] That is, major sources of elite surplus are derived from investment in primary industries in the more rural areas, while cities serve as major commercial and administrative centers (and also the primary places of residence for the national elite). The rural brokers for such an increasingly absentee national elite (e.g., *hacienda* managers) enable the elite to retain their control over rural surplus and facilitate a net transfer of resources from rural into urban areas through a variety of mechanisms.[23] Such a transfer in turn encourages further urban migration in excess of the cities' real industrial base—for example, an oversized service sector develops which caters largely to the indigenous elite and middle classes, and foreign businessmen.

Increased population density and the emergence of groups facilitating the economic activities of the national elite in more urbanized areas of the Philippines (e.g., professionals and middle level bureaucrats) create a domestic market which, under particular conditions such as import controls or tariffs, encourages foreigners and the indigenous elite to invest for this market. Still, it is clear that strong incentives remain to invest in the rural primary sector.[24]

The emergence of many types of petty establishments in the Philippines provides an indication of the vast supply of under- and unemployed laborers who must resort to marginal employment for their livelihood. Such forms of economic activity —coexisting side by side with large commercial establishments (and to a lesser extent, large manufacturing concerns)—constitute part of a dual socioeconomic system in urban areas involving pronounced class and cultural cleavages between an ever proliferating lower class population and the elite, middle-level professionals and bureaucrats living in the suburbs. Another manifestation of the dual economy occurs in the more rural areas, where large agro-business corporations such as sugar centrals are accompanied by little other industrial or large commercial activity. While such agro-businesses tend to be fairly traditional in terms of the methods of social control they employ and in not being very disruptive of traditional social structure, they do tend to be *in* but not *of* the rural areas in which they are found. That is, they are geared to serving the national elite in the primate city and a largely foreign market.

Even though urban affluence may be functionally related to the extraction of rural surplus, the benefits of this surplus are less apparent in the rural areas. That is, there is a net transfer of resources from rural to urban areas. It is not clear, however, whether the existence of an urban market (e.g., arising from population density or high levels of social mobilization) is related in linear fashion to investment in manufacturing for such a market. The process by which cities with a largely commercial and administrative base acquire some degree of industrialization is likely to result from certain economies of scale which concentrate manufacturing for the internal market in large cities. While detached in spatial terms from the extraction of rural surplus, we anticipate that urban wealth is related to measures of service-commercial development which cater largely to the upper and middle classes (e.g., banks, supermarkets, nightclubs).

Sociodemographic Structure and Political Structure

Changes in traditional Philippine status and economic arrangements have increased the separation between private and public wealth. As

[20] *Ibid.*, p. 97.

[21] See Gideon Sjoberg, "Cities in Developing and in Industrial Societies: A Cross Cultural Analysis" and Nathan Keyfitz, "Political-Economic Aspects of Urbanization in South and Southeast Asia," in *The Study of Urbanization*, ed. Philip Moris Hauser and Leo F. Schnore (New York: Wiley, 1965). See also Simon Kuznets, *Economic Growth of Nations* (Cambridge: Harvard University Press, 1971), pp. 80 ff.

[22] Sjoberg. See also T. G. McGee, *The Southeast Asian City* (New York: Praeger, 1967).

[23] Absente landlords in the Philippines invariably bank in major urban areas and often pay the majority of their taxes there. Sugar centrals and other large corporations in the more rural cities have offices in the dominant cities and often pay their corporate income taxes through these main offices. Since chartered cities which collect greater amounts of national revenues than they did in the past are apportioned a certain amount of the excess, the location in which corporations and individuals pay their taxes is important.

[24] When the Philippines did not receive its anticipated share of the U.S. sugar quota in 1970, six new sugar centrals were being constructed and eight more had been built since 1965. Information compiled from *Atlantic Gulf and Pacific Progress, Second Quarter Report*, Vol. 11, No. 2 (Manila, 1970), p. 5.

private wealth continues to be concentrated within an economy unable to absorb the growing labor surplus, public income is increasingly scarce relative to what is needed to provide economic security for broad groups of people. The impact of such scarcity upon the strength and structure of patron-client ties faced with increased urbanization and social mobilization can be observed through a number of processes, as follows: (a) differentiation among the local elite increases the probability that any local area will have more potential centers for patron-client alliances; (b) growing proportions of economically insecure clients and also middle class groups decrease the coverage of patrons unable to procure sufficient relevant public resources; (c) increased specialization in the structure of patron-client exchange decreases the reliability of political mobilization and increases the demand for public patronage resources suitable for such exchange (e.g., money).

Proliferation of Candidates

A Philippine middle class which performs important administrative and political functions for the national elite emerged as economic differentiation and centralization of government increased. At the local political level such individuals are brokers for an elite increasingly distant from the masses, yet seeking to extend national administrative and economic control. These brokers occasionally expand their influence beyond the local level, yet their bases of power are limited by the increasing concentration of private capital and the growing importance of national public resources.[25] To maximize control over local brokers, it is in the interest of the national elite to make such brokers as dependent upon them as possible. Since the capitalization required for business enterprises has risen, local elites with limited capital are less able to concentrate wealth.[26] The growing penetration of national bureaucratic institutions into local areas heightens the dependence of the local elite upon office-based resources. Results of an Ateneo de Manila survey are indicative of such differential control over the means of production by the national as compared with the local elite—that is, higher proportions of the national elite are reputed to have primary influence in the areas of business and economics than do local elites (41 per cent of the national elite as contrasted with 18 per cent of the local elite).[27]

While the particular pattern of Philippine economic development gave rise to a largely service and commercial middle class, the mobility opportunities for such a class have been much smaller than its aspirations. Such a middle class has been heavily exposed to Western consumerism and is vulnerable to inflation, and thus is particularly eager to increase its income when possible (e.g., through extensive moonlighting). Under these circumstances, the growing absolute size of the middle class has increased the number of individuals interested in holding a potentially lucrative local office. Additionally, the existence of a middle class has provided a social base for the emergence of occasional "reform" candidates more sensitive to its needs.

The specialization of clientelist structures that accompanies economic and social differentiation, together with the weakness of any single structure, provides particularly strong incentives for patrons to expand the range of clientelist groupings under their domination. This is likely since the appropriate boundaries of influence among institutions are not clearly defined, yet sizable rewards may be derived from any one of them. Labor union bosses may find themselves competing with political bosses, or vice versa.[28]

Increased ethnic diversity further expands the social bases necessary to support multiple local candidacies. This should be particularly true in ethnically plural areas of the Philippines (such as Cotabato City) which are not developed enough to sustain an urban culture that might weaken the strength of ethnicity. While ethnicity in the Philippines is not generally reinforced by religious cleavages (the primary exception being the Mindanao and Sulu Moslem groups), it does remain an important basis for political participation.

Since the number of political positions in any given Philippine city remains fairly constant over time, an expanded number of office seekers will simply increase the number of persons competing for a particular office. Such an inflationary cycle

[25] In her study of the municipality of Hulo, Hollnsteiner notes that the increasing incorporation of Hulo into the national-metropolitan arena has led to a proliferation of and differentiation among the local elite. Whereas previously the traditional land- and fish pond-owning elite had been strong enough to carry completely the dominant political faction and its auxiliaries, at present "non-elite professionals were trying to gain mastery over the faction but though fairly rich, they did not have the seemingly inexhaustible funds which their elite predecessors had had." Mary R. Hollnsteiner, *The Dynamics of Power in a Philippine Municipality* (Quezon City: Community Development Research Council, University of the Philippines, 1963), p. 51.

[26] From 1958 to 1961 the real paid-up mean capital investment per newly registered business was 214,900 pesos, and by 1961 to 1964 it had increased to 233,800 pesos. Calculated from Bureau of Census and Statistics, *Yearbook of Philippine Statistics, 1966* (Manila, 1966), Table 18, p. 117. The wholesale price index used as deflater appears on page 99 of the same source.

[27] Perla Makil, *IPC/PAASCU Study of Schools and Influentials* (Manila: Institute of Philippine Culture, 1971), Table A-3.

[28] For examples of this phenomenon in the Philippines, see Thomas Nowak and Kay Snyder, "Urbanization and Clientelist Systems in the Philippines," *Philippine Journal of Public Administration*, 14 (July, 1970), 267.

might be offset by a number of different conditions, but none of these seems evident in the Philippines presently. Rapid economic growth of the sort that occurred in the United States at the turn of the century, for example, could result in sufficient mobility through other channels so that political office would be less sought after as an avenue to success. Second, the emergence of strong political machines might restrict the number of local political aspirants in any given area. To understand why such strong machines have not developed in more urbanized areas of the Philippines, we must note the important differences between the Philippine machine and its turn-of-the-century American counterpart.

American machines evolved in response to some of the same conditions which render Philippine machines functional—the presence of a large, economically insecure population[29] within an electoral system in which votes could be traded for jobs and patronages. The rapid economic growth occurring in the United States at the turn of the century provided important resources for machine politicians. In addition to the exchange of corporate and finance capital by businessmen for local "advantages," the expansion of skilled and unskilled wage employment in the private sector offered the well-connected political boss the opportunity to procure employment in the private sector for his clients. Further patronage resources resulted from the rapid growth in local government services, particularly before the passage of the Civil Service Act.[30]

Resources for urban machines in the Philippines have not been as plentiful. A variety of factors contribute to this scarcity: wage employment opportunities are limited; systems of local taxation are inefficient; the national government preempts major sources of excise revenues; national patronage resources are insufficient. The limited patronage available may suffice for those poor first-generation migrants whose relative standard of living has increased, but is unlikely to satisfy later generations of the lower class whose expectations are rising[31] or to be particularly relevant for middle-class groups.

Since social mobility has not been extensive in the Philippines and since strong urban machines have failed to emerge, various processes of social change (population diversity, economic complexity, social differentiation, and ethnic diversity) should increase the numbers of individuals seeking access to scarce resources such as local political office. National political parties are too weak to limit the rise of local candidacies. Philippine local elections often have several candidates competing for the same office under the same party label.

H2: Population density, economic complexity, social differentiation, and ethnic diversity increase local elite differentiation, which increases the number of candidates competing for local office.

Specialization in Clientelist Structures

In the Philippines a feudal elite with origins in colonial centers of commerce and estate agriculture has gradually transformed itself into a national elite as political and economic power became more centralized. With the diversification of its power the Philippine national elite appeared increasingly unwilling to play the part of the traditional patron, both in the home areas of such elites and in the frontier areas opened to various forms of investment. Such elites sought to put their relations with clients on a specialized, commercial footing which could produce both surplus and political support but would otherwise free the elites from traditional obligations which could hinder their ability to pursue an increasingly national set of interests. Hollnsteiner notes that those with extra-local interests or those who reside in urban areas find that entanglement in diffuse forms of creditor-debtor relationships (*utang-na-loob*) places severe constraints on their ability to accumulate surplus. Such individuals prefer more explicit contractual or quasi-contractual forms of exchange.[32]

As the economy became differentiated, specialized patron-client relationships have emerged in more functionally specific forms such as political machines and boss-dominated labor unions. Specialized patron-client clusters are likely to be unstable, with a small core and large unreliable periphery.[33] The exchange between patron and client becomes both more instrumental and intermittent. Factionalism is intensified by the greater ease with which clients can attach themselves to

[29] The mean per cent of individuals with an elementary education or less in Philippine chartered cities in 1960 was 71 per cent. Calculated from the Bureau of Census and Statistics, *Census of the Philippines, 1960, Population and Housing*. In Chicago first generation immigrants comprised 52 per cent of the population by 1850. See Donald S. Bradley and Mayer N. Zald, "From Commercial Elite to Political Administrator: The Recruitment of the Mayors of Chicago," *The American Journal of Sociology*, 71 (September, 1965), 153–167.

[30] Between 1849 and 1868 municipal expenditures in Chicago increased from approximately $45,000 to more than $6,000,000. See Bradley and Zald, pp. 156–159.

[31] See the discussion by William Mangin, "Introduction," in *Peasants in Cities*, ed. William Mangin (Boston: Houghton Mifflin, 1970), pp. xxvii ff.

[32] Mary Hollnsteiner, "Reciprocity in the Lowland Philippines," p. 85.

[33] James Scott, "Patron-Client Politics and Political Change in Southeast Asia," pp. 91–113, does an extensive analysis of patron-client structures in which he suggests a number of dimensions along which such structures can be contrasted.

and break attachments with patrons. Since local political or union leadership provides local patrons with one attractive avenue of mobility, competition among elites for such positions has been intensive. This competition helped prevent the emergence of a more stable clientelist network by offering clients some choice between patrons. Increased lateral mobility by clients does not, however, represent any increase in their net resource position. To the contrary, the leverage which clients gained with competing patrons through the vote appears offset by the weakening of distributive pressures which were inherent in more diffuse forms of patron-client exchange.

Specialization in patron-client structures means that there is an increase in the number and type of such structures in any given area. This further complicates the coalition-building process since these specialized structures are more likely to have autonomous institutional bases of power. Additionally, the growth of middle-class groups located within specialized clientelist structures marked the emergence of clients whose interests could not be met within the local political arena.

Increasing reliance on monetary forms of exchange by political machines facing a growing shortage of patronage resources decreases the reliability of support for any one faction. Since both the economic and psychic sanctions which patrons can apply to the urban lower classes are more limited than those available to a landlord with his tenants, clients have more incentive to maximize their resources by hedging on commitments—e.g., collecting money from or being courted by more than one faction.

The deteriorating resource position of clients in more urbanized areas is underscored by the fact that the electorate increasingly demands explicit and immediate resources for their votes. If the resources "lost" from decreased services provided by patrons were compensated through adequate wage labor or provision of economic security by the government, voters would be unlikely to have such a short-range mercenary attitude toward the worth of a vote. As economic insecurity increases, however, the vote comes more and more to represent a resource exchanged for ready money to the highest bidder. To summarize:

H3: The strength of political machines weakens as general sociodemographic processes such as social mobilization and urbanization result in specialized clientelist forms of exchange.

H4: The strength of political machines weakens as intra-elite competition intensifies (evident in the proliferation of local political candidates).

Electoral Participation

In the Philippines limited forms of political mobilization may occur more effectively in areas where traditional forms of clientelist bonds are more fully intact. Mobilization theory, in assuming that the power of the local elite which benefits from parochial community loyalties must be broken before regional and national elites can gain support, ignores the often strong incentives for local elites to deliver exactly such loyalties to higher levels in exchange for resources which help them maintain their position in the community.[34] The increasing access of local elites to a variety of external resources (e.g., financial credit and the coercive power of the state to back up domination of local resources) allows escape from the distributive pressures normally existing in closed community systems.[35] At the same time such access increases the brokerage functions of local elites, which mediate between demands of the national government for resources such as votes and those of local clients for services and security. As long as the clients' positions do not substantially deteriorate, the patron remains able to increase political participation either for himself or on behalf of regional and national allies given the strength of obligations owed him for past and present services rendered.

Greater electoral participation in less urbanized areas should result not only from the previously noted characteristics of such areas, but also from circumstances in more urbanized cities which militate against participation. Nie et al. suggest that while city residents are more likely to have the status and organizational involvement which leads to political participation, the facilitating effect of small-town life is lost with increased urbanization and social mobilization.[36] Local political participation appears particularly likely to deteriorate as government becomes more complex. Whether such a decline in participation will be reflected in voting is not clear, however, since voting appears as one of the most simple and effortless acts of political participation.

[34] This is both cogently argued and demonstrated by Steven Dodge Norris, *Political Behavior and Social Change: Causes of the Growth of the Indian Electorate in the Last Half Century* (Ph.D. dissertation, Cornell University, 1971). What mobilization theory does alert us to is that in systems such as the Philippines which have literacy requirements for voting (albeit lax), social mobilization increases the proportion of adults registered to vote. Our data show that the level of social mobilization correlates +.46 with the level of voter registration. The concept of social mobilization is described in Karl W. Deutsch, "Social Mobilization and Political Development," *American Political Science Review*, 55 (September, 1961), 498–499.

[35] Lenski, for example, argues that in the simplest societies, or those which are technologically most primitive, the goods and services available will be largely distributed on the basis of need. See Gerhard E. Lenski, *Power and Privilege* (New York: McGraw-Hill, 1966).

[36] Norman H. Nie, G. B. Powell, Jr., and Kenneth Prewitt, "Social Structure and Political Participation: Developmental Relationships, I," *American Political Science Review*, 63 (June, 1969), 366.

While decreased local political participation might be attributed to the nationalization of politics, reduced national participation requires other explanations—e.g., the lessened effectiveness of clientelist machines. Such a reduction in effectiveness does not necessarily imply a more underlying process of institutional deterioration, however. A changing class structure could alter the arenas through which individuals press demands. In the United States at the turn of the century, for example, declining participation signified the weakening of machines as middle-class reformers mounted political and legislative attacks against such machines.[37] To argue then that decreased participation measures a decline in the effectiveness of clientelist machines for *all* sectors of the population requires some demonstration that general processes such as social mobilization explain more variance in turnout or decline in turnout than does the size of the middle class.

The institutionalization of particular patterns of competition may also lead to declining turnout. Gordon suggests that in the United States the struggle by ethnic groups to gain access to the political system was characterized both by intense competition and high turnout. Once ethnic conflict is built into a partisan party framework, both political competition and turnout may decline.[38] Whether this might be the case in the Philippines depends on the extent to which factional conflict has been stabilized.

Institutionalization of clientelist forms of mobilization could prevent turnout from declining, however, if important welfare needs of lower-class groups were met. The integration of peasants into national clientelist parties in Venezuela and in Italy was prompted by the need of such parties to increase their electoral support among peasants. By providing peasants with agrarian reform and other types of patronage, the clientelist system in fact stabilized electoral participation.[39] What did diminish was the recourse of such groups to violent means of expressing demands. Improving the quality of the urban environment has higher costs, and thus the state or party would seem less able to stabilize clientelist participation in more urbanized areas.

Since the Philippine government lacks the resources to finance reform and patronage available in a more industrialized country such as Italy or an oil-rich developing country such as Venezuela, lack of political participation could be linked to the general resource scarcity of machines. As the resources available to local brokers decline relative to the needs of lower-class individuals, incentives decrease for both elites and masses to participate in electoral politics. Individuals who expect to be paid off or otherwise rewarded for voting have little incentive to vote unless local political brokers reach them with the actual payoff. If the content of the payoff is increasingly less able to provide the lower-class voter with a margin of economic security, the potential increases for a shift to other arenas through which demands are pressed (for example, the streets).

The general scarcity of resources should be particularly pronounced in areas which have a minimum amount of access to national patronage. The relative frequency and the ease with which local officials in the Philippines switch to the party of the president indicate both the weakness of parties in the Philippines and the importance of access to national patronage resources. For a variety of reasons (such as long-term personal feuds) there are some consistently oppositionist areas which would be likely to receive few patronage resources.

H5: The effectiveness of machines in mobilizing broad groups of people declines with greater social mobilization, ethnic diversity, urbanization and other factors increasing the scarcity of patronage resources.

Data and Measures

The degree of indigenous capital concentration among the economic elite is measured by a Gini Index. This index measures the degree of inequality in familial control over corporate assets in each chartered city.[40] Construction of the index involved a number of steps. Following compilation of a master list of all large corporations filing with the Philippines Securities and Exchange Commission, stockholders were included for each corporation until the pool of the largest corporate stock-

[37] William Nisbet Chambers, "Party Development and the American Mainstream," in *The American Party Systems*, ed. William Nisbet Chambers and Walter Dean Burnham (New York: Oxford University Press, 1967), p. 14. See also Walter Dean Burnham, *Critical Elections and the Mainstream of American Politics* (New York: W. W. Norton, 1970), pp. 21 and 71–90. In Mexico, the access of the middle and upper classes to resources and policy makers through nonelectoral channels helps explain lower turnout among these sectors. See Jose Luis Reyna, *An Empirical Analysis of Political Mobilization: The Case of Mexico*, Latin American Studies Program Dissertation Series No. 26 (Ithaca: Cornell University Press, 1971), chapters 5 and 6.

[38] Daniel N. Gordon, "Immigrants and Municipal Voting Turnout: Implications for the Changing Ethnic Impact on Urban Politics," *American Sociological Review*, 35 (August, 1970), 665–681.

[39] John Duncan Powell, "Peasant Society and Clientelist Politics," *American Political Science Review*, 64 (June, 1970), 420.

[40] Our unit of analysis is the chartered city. Chartered cities are legal entities which may or may not be true cities, since they display considerable variance in density, industrialization and the level of social mobilization. Such cities either alone or in clusters, as in the Greater Manila complex, form labor market areas, which makes them a useful unit in analyzing relationships between socioeconomic and political structure.

holders owned at least 70 per cent of the corporate stock in that corporation. Subsequently, stockholders were organized by family or by company (in cases where companies owned stock) and the percentage of stock out of total paid-up capital was computed for each family or company.[41] Since individual corporations vary in size and therefore in their importance to an area, family corporate control is standardized by multiplying the percentage of stockholdings owned by one family by the total assets of the corporation.[42] Following computation of total corporate assets in a given chartered city, each family's share in this total was computed. The Gini Index, used to measure the degree of indigenous capital concentration, is constructed as follows:

$$\text{Gini Index} = 2\sum_{i=1}^{N}(X_i - Y_i)\Delta X_i$$

$$X = \frac{i}{N}$$

$$Y_i = \sum v_k / V$$

$$k \geqq i$$

N = Total number of families in a given chartered city
V = Total amount of corporate assets in a given city
v = Each family's share of city corporate assets
i = The i'th family in assets, from lowest to highest

The Gini Index sums for each family in the population the difference between its location on a Lorenz Curve and its expected location in the case of perfect equality in the ownership of corporate assets.[43]

Since patterns of industrialization and economic growth in countries such as the Philippines differ in important respects from Western models, we cannot assume that commonly used measures of industrialization are equivalent across systems. It is unlikely that variables such as the amount of value-added in manufacturing per capita and the mean size of manufacturing firms cluster together in economies where investment in giant, capital-intensive agro-business is such an important source of elite surplus. Generally, large agro-businesses such as sugar centrals and copra processing plants do not create as much value-added as do secondary basic industries like steel production.

Under the assumption that the meaning of specific indicators is best interpreted in the context of the particular system in which they are observed, we predict that indicators of various aspects of economic structure in the Philippines do not cluster together in the same manner as in the West.[44] Therefore measures of economic structure should distinguish, for example, between manufacturing investment for the largely internal, urban market and investment in the primary or rural sectors. They also should illustrate the commercial-service character of many Third World urban areas, as well as distinguish between petty forms of economic activity that absorb surplus urban labor and larger manufacturing firms which provide more stable employment.

Two sets of factor analyses were employed to extract the hypothesized dimensions.[45] Table 1 indicates the types of measures which tap agro-business forms of rural investment as against those measuring more urban-linked manufacturing investment for the internal market. High loadings on the factor labeled urban industrialization were noted for the following variables: percentage of total employment represented by mining and manufacturing; number of manufacturing production employees, number of manufacturing employees (as measured by two different sources); and per capita manufacturing fixed assets. Mining is included with manufacturing in the first measure for two reasons: (1) mines as well as agro-business forms of manufacturing are important sources of foreign exchange and investment in less urban areas; (2) since there are relatively few mines as compared with manufacturing firms, analyzing mines alone creates cumbersome distributions among the chartered cities.

[41] The information on stockholders and their stockholdings was obtained from records of the Philippine Securities and Exchange Commission. Only 16 per cent of the corporations were controlled by other corporations rather than individual families ($N = 1511$).

[42] Since the total corporate assets of a city accounted for under the 70 per cent rule varied from 77 per cent to 92 per cent, each family's control over corporate assets is further standardized by dividing it by the total assets accounted for by all the families enumerated under the 70 per cent rule for a city.

[43] See Hayward R. Alker, Jr., *Mathematics and Politics* (New York: Macmillan, 1965), pp. 30–42. A Lorenz Curve is a plot of values held by cumulative proportions of a population.

[44] Richard I. Hofferbert, "Socioeconomic Dimensions of the American States: 1890–1960," *Midwest Journal of Political Science*, 12 (August, 1968), 401–418, finds measures of urbanization and industrialization loading on the same factor which he calls "industrialization."

[45] In order to derive factors uncorrelated with each other, we used orthogonal rotation. While oblique factor solutions are often a closer approximation to reality in producing intercorrelated factor solutions, use of factor scores in regression equations influenced our decision to derive (when necessary) uncorrelated ecological variables. Entering highly intercorrelated variables into a regression equation may produce unreliable results. For a discussion of the assumptions behind different forms of rotation see Norman Nie, Dale H. Bent, and C. Hadlai Hull, *SPSS: Statistical Package for the Social Sciences* (New York: McGraw-Hill, 1970), chapter 17.

Table 1. Factor Analysis of Industrial Structure[a]

	Factor 1 Loadings (Urban Industrialization)	Factor 2 Loadings (Agro-Business)	Communality
Mean Employment			
Mining-Mfg.	.07	.82	.67
Mining, Utilities, Const. & Mfg.	.23	.87	.81
% Total Employment			
Mining-Mfg.	.92	.24	.90
Mfg. Production Employees	.87	−.05	.76
Mfg.	.92	−.01	.84
Mfg. 2	.90	.16	.84
Per Capita			
Mining-Mfg. Assets	.44	.55	.49
Mfg. Fixed Assets	.59	.41	.51
Mean Fixed Assets			
Mining-Mfg.	.01	.68	.46
Mfg.	.08	.67	.76
Variance Explained	65.7	34.3	

[a] Data are from the Bureau of Census and Statistics, *Economic Census of the Philippines: 1967*, except for statistics on mfg. 2, which are taken from the Bureau of Census and Statistics, *Directory of Key Establishments of the Philippines, 1967*. Since both sources had yet to be published, the variables are computed from raw data. There were fewer firms in the Economic Census than the Directory because the former requires quite detailed and extensive information from firms and thus received a lower response rate.

Table 2 illustrates clearly why common measures cannot be assumed equivalent across systems. The degree of urbanization appears to be part of a dimension depicting the commercial-service nature of Philippine urban areas, and is not related to industrialization, as Hofferbert and others have found it to be in the United States.[46]

The fact that per capita small firms in manufacturing appear in a dimension measuring small-scale economic activity rather than one measuring urban industrialization again should caution us against assuming common measures equivalent. The relatively high intercorrelations shown in Table 3 among small and medium-sized establishments per capita, regardless of economic classification, suggest in part that establishments employing less than twenty persons do not so much "pull" employment into urban areas as represent the effort by labor to survive given the scarcity of stable employment in the cities. Chinese participation in the petty economic sector in the Philippines may also help account for the intercorrelations among different types of small-scale firms.[47] In both cases petty economic activity appears to be a response by class or ethnic groups to a lack of opportunity in other economic sectors.

Factor-analytic data-reduction techniques are also used to create an index of social mobilization. As defined by Deutsch, social mobilization is a general process "in which major clusters of old social, economic and psychological commitments are eroded or broken and people become available for new patterns of socialization and behavior."[48] Deutsch suggests that a number of indicators measure the mobilization process representing the general availability of persons for nonagricultural occupations, the percentage of the population residing in urban areas, and the proportion of the population which is literate.[49] To ascertain whether such indicators measure an equivalent process across systems, an orthogonal factor analysis was performed on a group of nine variables; the analysis produced two dimensions—(1) social mobilization and (2) city size and diversity (see Table 4). A number of common social mobilization indicators indeed appear to be equivalent across systems. Some of the variables loading highly on the mobilization factor are: percentage of the population engaged in farming, literate, urban; and the percentage of households with electricity. Economic complexity, population density, and population size combine to form the

[46] Hofferbert.

[47] Economic census figures (while probably understating actual Chinese control) indicate, however, that alien control is limited in the smallest firms. Among establishments employing 10 or fewer individuals, Filipinos owned 90.0 per cent of those in commerce, 92.1 per cent of those in manufacturing, and 92.2 per cent of those in services. Bureau of Census and Statistics, *Economic Census of the Philippines: 1961*, Vols. 3, 6, 8 (Manila, 1965).

[48] Deutsch, p. 494.

[49] *Ibid.*, pp. 501–503.

Table 2. Factor Analysis of Economic Structure[a]

	Factor 1 Loadings (Small-Scale Economic Activity)	Factor 2 Loadings (Development of Commerce and Service)	Factor 3 Loadings (Urban Industrialization)	Communality
Degree of Urbanization	.29	.54	−.12	.40
Economic Complexity	.19	.87	.40	.95
Per Capita Small Firms				
Mfg.	.85	.40	−.08	.89
Commerce	.94	.26	.06	.96
Service	.84	.32	.04	.82
Mean Employment				
Service	.20	.36	−.18	.20
Commerce	.22	.74	.21	.64
% Total Employment				
Commerce	.42	.73	.23	.76
Service	.43	.54	−.24	.54
Mining-Mfg.	.01	.27	.80	.71
Per Capita				
Mining-Mfg. Assets	−.17	−.18	.65	.49
Value-Added Mfg.	.10	.06	.61	.39
Variance Explained	64.8	24.6	10.6	

[a] Sources: Degree of Urbanization (per cent of the population that is urban)—advanced reports for the Bureau of Census and Statistics, *Census of the Philippines, 1970, Population and Housing*, economic complexity (number of different industry types in a given city, utilizing a 4-digit classification)—from data collected for the Bureau of Census and Statistics, *Directory of Key Establishments of the Philippines, 1967;* per capita small firms (5 to 9 employees)—Department of Labor, *Directory of Key Establishments in the Philippines, 1959–1960;* other variables—data collected for the Bureau of Census and Statistics, *Economic Census of the Philippines: 1967.*

second dimension. The two factors intercorrelate +.53 after oblique rotation.[50]

Ethnic uniformity is measured by the standard deviation from the mean of the percentage voting-age population that is in each of the eight major language groups within a given chartered city. Cities with the greatest ethnic uniformity (a high concentration of persons in one language group such as Tagalog) have large variances, while those with more diversity and a more even distribution of language groups have smaller variances.

Measures of the strength of political machines utilize local rather than regional or national electoral statistics. As Landé has argued, Philippine political parties

> emerged from an upward rather than downward organization of forces . . . interests of modern Filipino voters . . . are primarily local interests and their first loyalties are to their local leaders.[51]

Parties are little more than agglomerations of vertically linked patron-client sets of alliances, segments of which may switch parties when expedient. Thus, local machine strength is viewed as the degree of control which influential patrons have in an area over time, irrespective of the party affiliation of the patron. Such control is measured by the mean plurality achieved by the winning mayor(s) in the three local elections held between 1959 and 1967.[52]

To confirm the validity of using local rather than national electoral statistics for measures of machine strength, we predict that the degree of machine strength at the local level is more stable for purely local elections than for national ones. Delivering support to national and regional allies is more complex than generating local support for oneself. Patron-client tiers extending from the *barrio* level through the city level and on up to the national level are more complex than tiers extending only from the *barrio* to the city level, and therefore one would expect greater "leakage" to occur in alliance structures leading to the national level than in those leading to the city level. Multiple regression analyses confirm the greater stability of local than of national electoral support

[50] Since oblique rotation does not necessarily produce factors uncorrelated with each other, its factors are often a closer approximation of reality than orthogonal solutions. In this case, however, the factors derived from oblique rotation were virtually identical to those derived from the orthogonal solution.

[51] Carl Landé, *Leaders, Factions and Parties: The Structure of Philippine Politics*, p. 4.

[52] While some cities had elections for certain local offices before 1959, all mayoral and city councilor positions became elective for all cities with the passage of the Local Autonomy Act in 1959 (R.A. 2264). City residents participated in national elections, however, even before Philippine independence. The earliest national election data available to us for all chartered cities was for the 1957 election.

Table 3. Intercorrelations of Different Forms of Petty Economic Activity[a]

Item	No. of Establishments Per Capita					
No. of Establishments Per Capita	Small Mfg.	Med. Mfg.	Small Service	Med. Service	Small Commerce	Med. Commerce
Small Mfg.	1.00	.67	.87	.56	.91	.80
Med. Mfg.		1.00	.58	.50	.59	.72
Small Service			1.00	.56	.87	.76
Med. Service				1.00	.57	.58
Small Commerce					1.00	.87
Med. Commerce						1.00

[a] All coefficients are significant beyond the .01 level. Small establishments employ 5–9 people, and medium establishments 10–19 people. Source: Department of Labor, *Directory of Key Establishments in the Philippines, 1959–1960.*

over time. There are only two previous local elections to enter into the equation to predict the level of support for the winning mayor in 1967, as compared with three previous national elections to predict the level of support for the strongest congressional candidate or the strongest presidential candidate in 1969. In spite of the inclusion of fewer variables, slightly more variance is explained in the level of mayoralty support than in the level of either congressional or presidential support (.50 as compared with .48 for both of the latter).

The validity of mean plurality of votes for the winning mayor as a measure of local machine strength is further confirmed by its high intercorrelations with other measures of local dominance. Such measures include the mean percentage of city councilors belonging to the same party as the mayor, and turnover among all elected city officials.[53] While parties have scant existence apart from cliques and factions, a councilor affiliated with the same party as the mayor is more likely to be an ally or perhaps a client of the mayor than a councilor belonging to the opposing party.

The number of local political factions is measured by the mean number of "excess" candidates running for local political office. Since the norm for local political competition in a two-party system such as exists in the Philippines is that for each local office there will be two candidates on the slate, any deviation from this norm is an ex-

[53] Mean vote for winning mayor is strongly correlated with the mean percentage of city councilors belonging to the same party as the mayor (+.62) and with turnover among all elected city officials over the three terms of office for which election data are included (−.59). With an N of 46 both correlation coefficients are significant beyond the .001 level.

Table 4. Factors Utilized to Construct Social Mobilization Scale and City Size and Diversity Scale[a]

Item	Factor 1 Loadings (Social Mobilization)	Factor 2 Loadings (City Size & Diversity)	Communality
% Population[b]			
In Farming	−.66	−.38	.69
Literate	.83	.11	.85
High School Ed.	.89	.17	.88
Degree of Urbanization	.73	.23	.65
% Households with			
Electricity	.85	.43	.96
No Radio	−.79	−.46	.95
Economic Complexity	.33	.90	.87
Population			
Density	.23	.69	.60
Log to Base 10	.16	.85	.83
% Variance Explained	81.8	18.2	

[a] Sources: Degree of Urbanization, economic complexity—see Table 2; all other variables are from the Bureau of Census and Statistics, *Census of the Philippines, Population and Housing;* population density is from the 1970 census, others from the 1960 census.

[b] Population bases for these variables are as follows: farm population (economically active population), literacy (population over 10 years of age), high school educated (population over 6 years of age).

cess or deficit. The number of local political factions is a mean of the number of excess or deficit candidates in three years for which local election data were available.

Voting turnout is used to measure the strength of political mobilization. A grand mean as well as separate means for both local and national elections are computed.[54] While Philippine cities do not as a group experience a change in level of turnout, individual cities do have increases and decreases.[55]

Change in national turnout is measured by the deviation of mean turnout in the 1965 and 1969 elections from the regression line computed by the regression of mean late turnout (1965 and 1969) on mean early turnout (1957 and 1961). This procedure controls for any variation among initial levels of turnout which might affect changes in future levels of turnout.[56] Negative residuals imply then that turnout has decreased while positive residuals suggest an increased turnout. Change in local turnout is computed in a similar fashion, with the exception that early and late turnout are measured by single years (1959 and 1967).

[54] The mean begins with the 1957 election and includes elections for 2 year intervals through the 1969 election. Turnout is the proportion of registrants voting.

[55] Mean voting turnout in these national elections fluctuates between 75 per cent and 77 per cent for the chartered cities as a group, and in local elections it fluctuates between 76 per cent and 79 per cent.

[56] See George W. Bohrnstedt, "Observations of The Measurement of Change," in *Sociological Methodology*, ed. Edgar F. Borgatta and George W. Bohrnstedt (San Francisco: Jossey-Bass, 1969). See also Norris, *Political Behavior*, pp. 22–24. For our data the use of residuals produces a solution similar to that obtained by subtracting turnout in the early years from turnout in the late years. The intercorrelations between the two alternative measures of change in local and national turnout are all over +.98.

Instability in pluralities is measured by the standard deviation of pluralities in the local and national elections between 1957 and 1969. Cities with fluctuating pluralities will have large standard deviations, while those with stable pluralities will have small standard deviations.

Stable party affiliation is measured by a dummy variable. Cities with either a Liberal mayor over all election years or a Nacionalista mayor over all election years are scored 1, while those with both Nacionalista and Liberal Party mayors are scored 0. Stable opposition is measured in a similar way. Since Nacionalista presidents were elected for three of the four national election periods, opposition is defined by Liberal Party membership. Cities with Liberal mayors over all election periods are scored 1, while other cities are scored 0.

Capital Concentration and Economic Diversification

As shown in Table 5, the degree of urbanization appears to be the most important determinant of economic complexity. Both the percentage of foreign ownership and the indigenous capital concentration are also significant elements in the equation.

As evident in Table 6, the indigenous capital concentration and, to a lesser extent, the degree of urbanization are determinants of urban industrialization. The relationship between urban industrialization and the percentage of manufacturing that is foreign is considerably weaker. This beta coefficient rises, however, when the percentage of manufacturing that is American is substituted for the percentage that is foreign.

The pattern and scope of foreign investment in the Philippines provides clues as to why the percentage of manufacturing that is American explains more variance in urban industrialization than does the percentage of manufacturing that is

Table 5. Multiple Regression Equations for Determinants of Economic Complexity[a]

Dependent Variable	Independent Variables	*B*	Beta	Standard Error of *B*
Economic Complexity[b]	% Foreign Ownership	2.6527	.26*	1.2846
	Indigenous Capital Concentration	0.0035	.45**	0.0010
	Degree of Urbanization	0.0221	.71**	0.0041
	(Constant) −0.672	$F = 12.6$ with 3,25 d.f.	$R^2 = .60$**	

[a] Sources: % Foreign ownership (percentage of total stockholdings that are controlled by foreigners)—data collected for the Bureau of Census and Statistics, *Economic Census of the Philippines: 1967;* indigenous capital concentration—see pp. 1154–1155; degree of urbanization, economic complexity—see Table 2.

[b] The natural log of economic complexity is utilized in order to lessen the skew of this variable (2.501) given the great differences between cities in the Greater Manila Area and smaller ones in the number of different types of economic establishments found in each.

* $p \leq .10$ on a one-tailed test. Significance tests are reported for the interested reader even though we are analyzing the universe of Philippine chartered cities rather than a sample.

** $p \leq .05$ on a one-tailed test.

Table 6. Multiple Regression Equations for Determinants of Urban Industrialization[a]

Dependent Variable	Independent Variables	*B*	Beta	Standard Error of *B*
	Equation 1			
Urban Industrialization	Indigenous Capital Concentration	.00369	.56**	.00107
	Degree of Urbanization	.00775	.29	.00430
	(Constant) −2.9361	$F=6.5$ with 2,27 d.f.	$R^2=.32$*	
	Variables with F less than 1.0 not in the equation			
	Variable		Partial	
	% Mfg. Foreign		.04	
	Equation 2			
Urban Industrialization	Indigenous Capital Concentration	.00343	.52**	.00104
	Degree of Urbanization	.00913	.34*	.00420
	% Mfg. American	1.14007	.29	.62567
	(Constant) −2.96248	$F=5.8$ with 3,26 d.f.	$R^2=.40$**	

[a] Sources: Indigenous capital concentration—see pp. 1154–1155; degree of urbanization—see Table 2; % mfg. foreign and % mfg. American—from data collected for the Bureau of Census and Statistics, *Economic Census of the Philippines: 1967;* urban industrialization—factor score derived from Table 1.

* $p \leq .10$ on a one-tailed test.

** $p \leq .05$ on a one-tailed test.

foreign. The largest manufacturing firms are American- rather than Chinese- or European-controlled. American investments constitute the biggest block (about 83.7 per cent) of the 67 foreign-owned firms among the top two hundred corporations in the Philippines.[57] American investment is more balanced between heavy and light manufacturing than is investment by the second largest foreign group, the Chinese. Approximately 47 per cent of American-owned manufacturing firms are classified as engaging in heavy manufacturing (e.g., refining of petroleum products) rather than light manufacturing (e.g., textiles and food processing), while heavy manufacturing accounts for only 14 per cent of Chinese-controlled firms.[58]

Greater diversification of American than Chinese investment in manufacturing does not mean extensive American participation in building up basic Philippine industries. On the contrary, realms of American domination suggest a high degree of dependence on capital imports—e.g., in petroleum refining and distribution (Caltex, Shell, Esso, and Mobil Oil), assembly and manufacture of appliances (USIPHIL, Aircon), assembly of transport equipment (Ford Philippines, Chrysler Philippines), assembly of office equipment (IBM), and manufacture of tire and rubber products (B. F. Goodrich and Goodyear).[59] Much recent growth of manufacturing in the Philippines is largely a function of growth in capital goods imports, with only a small growth in domestic output of capital equipment.[60]

The commercial nature of much American investment in the Philippines is illustrated by comparing Table 7 with Table 6. There is a stronger relationship between the percentage of commerce that is American and commercial development (as measured by both the mean employment in commercial establishments and the percentage employed in commercial establishments) than be-

[57] "The Top Two Hundred Foreign Firms," *Manila Chronicle Magazine Supplement,* July 7, 1971, p. 1.

[58] Computed from Bureau of Census and Statistics, *Economic Census of the Philippines, 1961,* Vol. III, Table 25.

[59] See "The Top Two Hundred Foreign Firms."

[60] See Hicks and McNicoll, *Trade and Growth in the Philippines,* p. 72. While the commercial, processing and assembly nature of much American investment in the Philippines helps perpetuate dependence on capital imports, equally significant for the pattern of economic growth in the Philippines is the amount of capital outflow. For the six years following 1957, earnings accruing to American foreign investors were more than twice the amount of direct foreign investment in the Philippines. See Frank H. Golay, "Economic Collaboration: The Role of American Investment," in *The United States and the Philippines,* ed. Frank H. Golay (Englewood Cliffs: Prentice-Hall, 1966), p. 110. From 1965 to 1968, new American investment amounted to $30.2 million while withdrawals were more than $59 million. A. B. Santos, "The Philippines—Country with an Image Problem," *Manila Chronicle,* January 6, 1971, p. 9. These figures overstate the amount of net capital inflow since many foreign firms raise considerable amounts of their capital requirements from Philippine banks. In 1968 the National Economic Council found that 108 American firms over the period from 1956 to 1965 raised 84 per cent of their capital requirements from Philippine Banks, and only 16 per cent from foreign sources. Cited in the *Manila Chronicle,* May 4, 1971, p. 9.

Table 7. Multiple Regressions for Determinants of Commercial Development[a]

Dependent Variable	Independent Variables	*B*	Beta	Standard Error of *B*
	Equation 1			
Mean Employment Commercial Est.	% Commerce American	86.4342	.51**	23.9531
	Indigenous Capital Concentration	0.0261	.42**	0.0083
	Degree of Urbanization	0.1220	.49**	0.0357
	% Commercial Chinese	8.9524	.21	5.8854
	(Constant) −11.076	$F=8.8$ with 4,24 d.f.	$R^2=.58$**	
	Equation 2			
% Employed Commercial Est.	% Commerce American	952.3636	.58**	200.3931
	Indigenous Capital Concentration	0.2840	.48**	0.0715
	Degree of Urbanization	1.0123	.43**	0.3119
	% Commercial Chinese	31.7998	.08	51.3823
	(Constant) 192.1564	$F=11.72$ with 4,24 d.f.	$R^2=.65$**	

[a] Sources: Indigenous capital concentration—see pp. 1154–1155; degree of urbanization—see Table 2; other variables—data collected for the Bureau of Census and Statistics, *Economic Census of the Philippines: 1967.*

** $p \leq .05$ on a one-tailed test.

tween the percentage of manufacturing that is American and urban industrialization. While American investment in the Philippines has been slightly greater in manufacturing than in large-scale commerce, American-controlled manufacturing is dispersed over a wider geographic base. Manufacturing investment in food processing linked to the primary sector (e.g., Dole Philippines and Franklin Baker) is found in certain less developed cities. Such a dispersion helps account for the weaker relationship between the percentage of manufacturing that is American and urban industrialization.

Economic and Sociodemographic Structure

As anticipated, agro-business is not positively correlated with social mobilization or the degree of urbanization (see Table 8). Urban industrialization (investment in manufacturing for the internal market) is positively correlated with both population density and economic complexity. While investment in various types of manufacturing increases the level of economic complexity in a city, such investment is in turn stimulated by the existence of sufficient numbers of people to constitute a market (as measured by population density). The spatial separation which we argued exists between the level of community wealth supportive of higher levels of social mobilization and one important source of this wealth in rural areas is also apparent in the more urbanized areas. That is, urban industrialization is only weakly related to social mobilization. For many of the more urbanized areas in the Philippines, colonial heritage dictated a substantial commercial and administrative base.[61] Additionally, Southeast Asian cities have tended to develop distinguishable patterns of land use. While the commercial-business district often constitutes the center of the larger Philippine urban areas, factory zones appear in outlying areas with the creation of new industrial estates (or appeared adjacent to the Port as a result of the emergence of processing industries during the colonial era).[62]

The absence of clearly demarcated city types of the kind found in the West is suggested by the positive relationships between commercial and service development (mean employed in commerce, in service; per cent employed in commerce, service), and forms of petty commerce and industry (per capita small manufacturing, commerce). As McGee notes in discussing land-use patterns in Southeast Asian cities:

> The mixed economic structure of the cities in which highly-developed forms of Western capitalism, such as banks and trading firms, coexist with pre-industrial and semi-capitalistic forms of economic organization like the Chinese loan association and the mobile street markets of the indigenous populations, inhibits large areas of homogeneous land use from emerging.[63]

While residentially exclusive areas in the suburbs with their large banks and supermarkets resist both the intrusion of squatters and open-air markets whenever possible, squatters still move into these areas—often because of the under-utilization of land and the desire to be as close as

[61] See McGee, *The Southeast Asian City*, chapter 3.
[62] *Ibid.*, p. 133.
[63] *Ibid.*, p. 126.

Table 8. Intercorrelations of Measures of Economic and Sociodemographic Structure[a]

	Agro-Busin.	Urban Indus.	Soc. Mobil.	Per Cap. Small		Mean Employed		% Employed		Degree of Urbaniz.	Pop. Density	Econ. Complexity
				Mfg.	Commerce	Commerce	Service	Commerce	Service			
Agro-Business	1.00	.26	−.25	−.32	−.39	−.23	−.24	−.16	−.26	−.20	−.00	−.13
Urban Indust.		1.00	.12	.20	.32	.41	.10	.48	.14	.16	.32	.64
Soc. Mobil.			1.00	.49	.37	.46	.29	.29	.59	.75	.23	.35
Per Cap. Small Mfg.				1.00	.91	.48	.38	.49	.61	.48	.23	.48
Per Cap. Small Commerce					1.00	.39	.29	.63	.55	.40	.21	.46
Mean Employed Commerce						1.00	.31	.73	.40	.45	.58	.77
Mean Employed Service							1.00	.22	.52	.15	.03	.28
% Employed Commerce								1.00	.49	.47	.73	.86
% Employed Service									1.00	.53	.20	.40
Degree of Urbanization										1.00	.31	.47
Population Density											1.00	.72
Economic Complexity												1.00

[a] Sources: Agro-business and urban industrialization—factor scores generated in Table 1; social mobilization—factor score generated in Table 4; population density—see Table 4; other variables—see Table 2. Correlations of .24 or above are significant beyond the .05 level.

possible to their place of work. In the Greater Manila area, squatters have moved both into the inner city (in areas such as Intramuros which suffered extensive war damage) and into encampments near suburban villages for the middle and upper class in some of the more outlying districts in Quezon City or Pasay City.

The Strength of Machines

As evident in Table 9, both ethnically defined social bases (a lack of ethnic uniformity) and the size of the class from which local candidates tend to come (the percentage of the population that is college-educated) are determinants of the number of local political factions. Since the percentage of the population that is college-educated is highly correlated with social mobilization ($r = +.80$), it is also likely that that percentage reflects the extent of social differentiation. In order for increased factionalism to be meaningful, there must be both an increase in the number of competing patrons and in the diverse groups of clients willing to support such patrons. The strong impact of the number of local factions on local machine strength (Table 10) suggests that the proliferation of political factions is indicative of a real diffusion of power rather than a proliferation of totally powerless local candidates.

The second major determinant of local machine strength in Table 10 is the prevalence of agro-business. Company towns arising from the presence of large sugar centrals or mines are conducive to maximum political control by a local elite with close ties to managers or owners of the corporation. While such settings do imply the commercialization of exchange relationships, any loss of control incurred from the increasingly monetary nature of exchange is more than compensated for by the corporation's monopolization of employment opportunities and sources of credit. Workers in sugar centrals and mines tend to be chronically in debt to the company store, for example, and extremely vulnerable to manipulation for the employer's or other purposes.[64] Additionally, since agro-business so dominates the economy of the area in which it is located, it can provide an important source of revenue to lubricate local machines—through such formal channels as taxes and also more informal payoffs or contributions.

City size and diversity is a weaker but positive determinant of local machine strength. This relationship may be partially explained by the high correlations between city size and diversity, on the one hand, and urban industrialization ($r = .61$) and the percentage employed in large commercial

[64] See for example Nanette Garcia Duñgo, *A Southern Industrial Complex* (Quezon City: Community Development Research Council, University of the Philippines, 1969).

Table 9. Multiple Regression Equations for Determinants of Number of Local Political Factions[a]

Dependent Variable	Independent Variable	B	Beta	Standard Error of B
No. of Local Political Factions	Ethnic Uniformity	−.0440	−.27*	.02395
	% Population College Educated	.0399	.27	.02211
	(Constant) 3.3515	$F=5.23$ with 2,43 d.f.	$R^2=.20$*	
	Variables with F less than 1.0 not in the equation			
	Variable	Partial		
	Urban Industrialization	.11		
	Agro-Business	.14		
	City Size and Diversity	.12		

[a] Sources: Ethnic uniformity—from a 1960 Philippine Census Sample obtained through John Koehler of the Rand Corporation; % population college educated from the Bureau of Census and Statistics, *Census of the Philippines, 1960, Population and Housing;* number of local political factions—data from the Philippine Commission on Elections (the natural log is utilized since the number of local political factions increases exponentially in primate area cities); other variables—see Tables 1 and 4.

* $p \leq .10$ on a one-tailed test.

establishments ($r=.80$), on the other. Machines able to tap local sources of finance and industrial capital in exchange for favors are likely to be stronger than those lacking access to such wealth.

While the simple correlation between social mobilization and local machine strength ($r=-.37$) becomes weaker in the regression equation, the correlation is in the anticipated direction. This finding supports our argument that alliance structures become more unstable as the form of exchange between patron and client becomes more specialized. The process may not be a perfectly linear one, however, and it may be more complex than we had anticipated. Our findings linking social mobilization to weakness in machines appear contradicted by those of Machado, who, in his comparative study of municipal mayors from Batangas and Capiz provinces, finds less turnover among mayors from municipalities with higher levels of social mobilization and with less farm tenancy (where mayors less often tend to be upper class) than from those less disturbed by social changes.[65]

While Machado cites longer tenure in office by those mayors from more humble class backgrounds as evidence for the increased professionalization of politics, there is no indication that such professionalization fundamentally changes the bases upon which political factions are organized. The movement away from diffuse landlord-tenant control to more specialized patterns of exchange suggests that political alliances become more unstable, *unless* the local politician

[65] Kit G. Machado, "Changing Patterns of Leadership Recruitment and the Emergence of the Professional Politician in Philippine Local Politics," *Philippine Journal of Public Administration,* 16 (April, 1972), 147–169.

Table 10. Multiple Regression Equations for Determinants of Local Machine Strength[a]

Dependent Variables	Independent Variables	B	Beta	Standard Error of B
Local Machine Strength	No. of Local Political Factions	−6.4549	−.62**	1.3024
	Agro-Business	3.0106	.29**	1.2288
	Social Mobilization	−1.5878	−.14	1.1385
	City Size and Diversity	1.4949	.16	1.3699
	(Constant) 69.4344	$F=9.70$ with 4,41 d.f.	$R^2=.49$**	
	Variables with F less than 1.0 not in the equation			
	Variable	Partial		
	Urban Industrialization	−.09		
	Ethnic Uniformity	.11		

[a] Sources: Local machine strength—from data of the Philippine Commission on Elections; other variables—see Table 9.

** $p \leq .05$ on a one-tailed test.

Table 11. Level of City Development By Number of Times Mayor Re-elected[a]

	Number of Times Mayor Re-elected			
	1	2	3	*N*
Level of City Development				
Most	7.1%	71.4%	21.4%	14
Medium	9.1%	50.0%	40.9%	22
Least	20.0%	20.0%	60.0%	10
Chi Square = 6.35 with 4 d.f., $p \leq .17$.				
Medium Level Cities (High vs. Low)				
	1	2	3	*N*
Medium High	8.3%	50.0%	41.7%	12
Medium Low	10.0%	50.0%	40.0%	10
Chi Square = .03 with 2 d.f., *N.S.*				

[a] Sources: Number of times mayor re-elected—from data of the Philippine Commission of Elections; level of city development—classifications developed by the Philippine Department of Finance, as discussed in Republic Act 4477, based on a city's average annual income for four years preceding their classification (Class Special, 1A and 1 are most developed; Class 2 and 3 are medium developed; and Class 4 through 8 are least developed. Medium high cities are Class 2 and medium low cities are Class 3.)

has access to sufficient resources to "buy off" enough support over long periods of time.[66] Since public resources are most scarce in highly urbanized areas, there may be certain thresholds that affect the relationship between the length of mayoral incumbency (which is one measure of local machine strength) and the level of city development. That is, in areas with intermediate levels of development, the professional politician may compensate for resources he has "lost" in his not coming from a prominent local family by cultivating (or being cultivated by) national sources of patronage. Since such politicians assume important brokerage roles between local clients and the national bureaucracy or national politicians, local political professionals may be able to mobilize sufficient office-based and patronage resources to enable them to stay in power. At even greater levels of urbanization (which are not included in Machado's sample, but are in ours) the effectiveness of local political machines decreases as it becomes impossible to marshall sufficient resources to meet the rising demands of increasingly diverse social groupings. Table 11 indicates that while the level of city development is inversely related to the number of times the mayor has been re-elected, there is no relationship between the two variables for cities with medium-high and medium-low levels of city development.

The weakness of machines in more developed cities suggests that there are determinate limits to exchanging local political support for national patronage resources without both the emergence of a more cohesive set of political structures such as a single or mass party and an increased flow of patronage resources to broad groups of clients. The important brokerage functions which local machines perform for national elites and clients is confirmed by the correlation between the strength of pluralities gained by the strongest presidential candidate in a city and the strength of local machines ($r = .63$). The instability of nationally linked coalitions in more developed cities is especially apparent in the intercorrelations between local and national pluralities over time. Areas in which local machines have considerable control should show greater stability in the pluralities such elites achieve for themselves and are able to deliver to national allies. Tables 12 and 13 indicate that pluralities for mayor tend to be most highly intercorrelated with pluralities for president in the least developed cities and in cities with the strongest local machines.

The limited effectiveness of patronage resources for building coalitions is apparent in the difference between the mean pluralities generated for local as compared with national candidates. It is anticipated that the strongest voting coalitions will be built at the time of national elections (since the national government has more patronage resources and national candidates run at large); this means that local candidates are motivated to deliver maximum pluralities to national candidates with whom they are affiliated. As shown in Table 14, national candidates receive higher mean electoral pluralities than local candidates do regardless of the level of city development. The smaller difference between national and local pluralities in more developed cities indicates limits to expanding coalitions in such areas during national elections. These differential pluralities cannot be attributed to any bias toward allocating patronage resources to less urban areas. If anything, the data indicate that more urban areas receive more patronage. The amount of large public works expenditures per capita from 1966–1969 is weakly but positively correlated with the level of urban industrialization ($r = .27$) and city size and diversity ($r = .25$). The amount of small public works expenditures per capita is not correlated with urban industrialization, but it is negatively correlated with city size and diversity ($r = -.29$). Correlations between the level of social mobiliza-

[66] In his study of local leaders in Camarines Norte, Benson notes that "jobs, projects and money were shown to be the most important considerations for a *lider* to support a candidate for governor or congressman." See Lewis Benson, *Political Leadership Through Political Liders: A New Approach for the Analysis of Philippine Provincial Leadership Positions* (Ph.D. dissertation, University of Hawaii, 1970), pp. V–VI.

Table 12. Intercorrelations Between Local and National Pluralities Over Time, by Level of City Development[a]

	Plurality for Presidential Candidate				
	1957	1961	1965	1969	
Most Developed Cities					
Plurality for Mayor					
1959	.10	.77*	−.07	−.20	
1963	.14	−.14	−.05	.11	
1967	−.04	.28	−.19	.44	(N=14)
Moderately Developed Cities					
Plurality for Mayor					
1959	.65*	.26	.52	.42	
1963	.50*	.03	.60*	.03	
1967	.36*	−.15	.62*	.41*	(N=22)
Least Developed Cities					
Plurality for Mayor					
1959	.42	.59*	.52	.42	
1963	.35	.80*	.60*	.66*	
1967	.63*	.77*	.57*	.57*	(N=10)

[a] Sources: Electoral pluralities—from data of the Philippine Commission on Elections; level of city development—see Table 11.
* $p \leq .05$ on a one-tailed test.

tion and either small or large public works expenditures per capita are all under .24, and therefore not significant at the .05 level.

The Stability of Local Machine Strength

The weakening of clientelist alliance structures can be further illustrated by the inability of dominant patrons to create organizations able to mobilize stable support over time. A decline in the integrative capacity of political machines requires more than a change from a pattern of competition in which a single machine has a wide range of power to one in which two machines have stable clienteles. Since we argue that our measure of machine strength does *not* measure a simple transition from a stable unifactional system in which pluralities are high to a stable bifactional system in which pluralities would be lower, we should be able to demonstrate that the strength of pluralities achieved by winning mayors in the more developed areas is not stable over time. By contrast, a stable bifactional system with a small

Table 13. Intercorrelations Between Local and National Pluralities Over Time, by Local Machine Strength[a]

	Plurality for Presidential Candidate				
	1957	1961	1965	1969	
Least Strong Local Machines					
Plurality for Mayor					
1959	.24	.17	.19	−.00	
1963	.20	−.37	.34	.00	
1967	.36	.11	−.21	.07	(N=16)
Moderately Strong Local Machines					
Plurality for Mayor					
1959	.20	−.45*	−.15	−.27	
1963	−.30	.02	.19	.16	
1967	−.05	.57*	−.05	.19	(N=16)
Most Strong Local Machines					
Plurality for Mayor					
1959	.68*	.50*	.57*	.62*	
1963	.60*	.55*	.50*	.31	
1967	.40	.21	.81*	.74*	(N=14)

[a] Sources: see Tables 10 and 12. Local machine strength is measured by mean mayoralty pluralities—least strong local machines have pluralities of 50% or less; moderately strong have pluralities between 50.1% and 55.0%; and most strong have pluralities greater than 55.1%.
* $p \leq .05$ on a one-tailed test.

Table 14. Level of City Development by Mean Pluralities in Local and National Elections[a]

	Mean Electoral Plurality (%)		
	Local	National	*N*
Level of City Development			
Most	50.4	53.1	14
Medium	53.2	56.9	22
Least	54.2	64.5	10

[a] Sources: See Tables 11 and 13.

swing vote should produce relatively equal pluralities for winners over time. Electoral data indicate that stable bifactionalism has not emerged in the most developed areas. For the most developed cities, the strength of early pluralities for mayor (1959 and 1963) predict very little variance in the strength of the plurality in the 1967 mayoral election ($R^2 = .01$). The amount of variance which early pluralities predict for the later one is greater in moderately developed cities ($R^2 = .65$) and is highest for the least developed cities ($R^2 = .70$). The plurality for mayor in 1967 is better predicted by earlier pluralities in cities with strong local machines than in cities with weak machines ($R^2 = .45$ as compared with $R^2 = .02$).[67]

Political Participation

Central to our argument that machines become weaker with urbanization and social mobilization is the notion that lower- as well as middle-class groups become less firmly integrated into local clientelist structures. For middle-class groups, greater exposure to national media and the emergence of class interests sensitive to national government policies on such issues as inflation decrease interest in local issues.[68] If the weakening of machines demonstrates a broader process of change apart from the growth of the size of the middle class in urban areas, then a decrease in coverage provided by machines cannot be specific to a particular class.

Table 15 shows that the determinants of both local and national turnout are ethnic uniformity and, to a lesser extent, city size and diversity. Ethnic appeals are powerful stimuli for mobilization in societies such as the Philippines where primordial loyalities remain salient and ethnic cleavages exist among the national elite seeking support. For example, the mean percentage vote in the 1965 and 1969 elections for President Marcos, an Ilocano, is positively correlated with the percentage of the voting population that is Ilocano (+.44). In these two elections, one of Marcos's opponents was a Pampangan and the other Visayan. Since Marcos had a Pampangan opponent in the 1965 election, it is hardly surprising that the vote for Marcos is negatively correlated with the percentage of the population that is Pampangan ($r = -.38$). For the 1957 election the simple correlation between the percentage voting for President Garcia (a Visayan) and the percentage of the voting population that is Visayan is +.66.

To the degree that a lack of ethnic uniformity implies at the same time less salient ethnic group identification, lower community cohesiveness makes it more difficult for political brokers to mobilize people to vote. Such a situation is particularly likely when sufficient cross-cutting cleavages exist to counter potential ethnic conflict and competition—for example, when exposure to the national Tagalog culture gradually weakens ethnic bonds. While ethnic diversification may under certain conditions intensify and politicize primordial loyalties (e.g., when it is reinforced by religious cleavages as in Mindanao), it may also, if initial cleavages are not too deep, speed the process of accommodation if patronage rewards result from such accommodations and various ethnic leaders have incentives to create interethnic coalitions. The innate instability of factions in systems such as the Philippines, combined with the often intense competition between rivals from the same ethnic group, would seem conducive to the creation of interethnic coalitions. Only 10.9 per cent of our chartered cities have ethnic cleavages reinforced by religious ones, and most of these cities have clear Christian majorities which either force Moslem leaders into electoral coalitions with Christians or encourage insurgency.[69]

Machine strength is another determinant of mean national turnout. Strong machines can mobilize higher proportions of the electorate for presidential candidates with which they are affiliated than can weak machines. The low community cohesiveness common to large cities may make the mobilization of votes more difficult, and helps account for the negative Beta between city size and both national and local turnout (Table 15).

[67] High levels of machine strength are measured by electoral pluralities of 52.1 per cent or higher, and low levels of machine strength by a mean plurality of 52.0 per cent or lower.

[68] See the discussion by Nie et al., "Social Structure and Political Participation," pp. 366–368.

[69] Basilan and Marawi are the only two of our cities with Moslem majorities (73 per cent and 86 per cent respectively). In the former the cleavages between the hill dwelling, nominally Moslem Yakan peoples and the coastal Tausog peoples are probably as intensive as those between Christian settlers and Tausogs. Such cleavages, combined with the high illiteracy among both Tausogs and Yakans, have made possible the election of a Christian mayor.

Table 15. Multiple Regression Equations for Determinants of Voting Turnout[a]

Dependent Variable	Independent Variables	*B*	Beta	Standard Error of *B*
	Equation 1			
Mean National Turnout	Ethnic Uniformity	.6711	.68**	.1004
	City Size and Diversity	−.8543	−.16*	.5576
	Local Machine Strength	.1853	.23**	.0834
	Social Mobilization	.8839	.13	.6659
	(Constant) 47.733	$F=19.7$ with 4,40 d.f.	$R^2=.66$**	
	Variables with F less than 1.0 not in the equation			
	Variable	Partial		
	Urban Industrialization	−.00		
	Agro-Business	−.06		
	Number of Local Political Factions	−.05		
	Equation 2			
Mean Local Turnout	Ethnic Uniformity	.6168	.65**	.0984
	City Size and Diversity	−1.5372	−.30**	.5292
	Agro-Business	−.6537	−.11	.5664
	(Constant) 62.025	$F=23.3$ with 3,41 d.f.	$R^2=.63$**	
	Variables with F less than 1.0 not in the equation			
	Variable	Partial		
	Social Mobilization	−.03		
	Urban Industrialization	−.04		
	Local Machine Strength	−.08		
	Number of Local Political Factions	−.02		

[a] Sources: Mean national and local turnout—data from the Philippine Commission on Elections; ethnic uniformity—see Table 9, local machine strength—see Table 10; other variables—from factor scores derived from Tables 1 and 4.

* $p \leq .10$ on a one-tailed test.

** $p \leq .05$ on a one-tailed test.

The weak positive Beta ($p > .10$) between mean national turnout and social mobilization makes it difficult to interpret the unexpected direction of the sign with any confidence.

For local turnout, machine strength is not a determinant. The incentive for strong local machines to maximize their potential to mobilize support is less strong in local than in national elections. It was mentioned previously that presidential candidates run at large and need a simple plurality to win, and consequently it is to their advantage to induce strong local machines to deliver a maximum of support to them. A local boss, by contrast, can win with a simple plurality in his city. Machines certain of victory can achieve savings by mobilizing only a portion of their clientele. The savings possible for elites with strong local power bases also helps explain the negative relationship between agro-business (often indicative of company towns) and local turnout.

Using a measure which approximates socioeconomic class (the level of education), we examined the differential influence of level of education on mean national and local voting turnout. Controlling for the three major determinants of turnout (ethnic uniformity, machine strength and city size and diversity) results in the following partial correlations with turnout: the proportion of the population with no education (−.15), with an elementary school education (+.23), with a high school education (+.02), with a college education (+.07). The fact that the crucial threshold occurs between "no education" and "elementary education" suggests that in a system of free mass elementary education a minimum amount of social mobilization is a much more important determinant of voting turnout than is middle class status.

Clientelist political mobilization means that mayoralty aspirants or local brokers have the loyalty of *barrio* leaders who can apply both the sanctions and rewards necessary to get voters to the polls. Areas with high proportions of less educated individuals only marginally participative in community economic life (e.g., agricultural workers or the unemployed) are likely to have low levels of mobilization because local leaders lack contact with and control over those only partially integrated into community activities. Anderson—

Table 16. Multiple Regression Equations for Determinants of Change in National and Local Turnout[a]

Dependent Variable	Independent Variables	*B*	Beta	Standard Error of *B*
	Equation 1			
Change in National Turnout	Social Mobilization	−1.0631	−.25*	.5735
	Ethnic Uniformity	.1856	.29**	.0823
	Stable Opposition	−3.4141	−.29**	1.5325
	Agro-Business	1.0980	.28*	.5203
	(Constant) −4.3588	F=7.5 with 4,40 d.f.	R^2=43**	
	Variables with F less than 1.5 not in the equation			
	Variable	Partial		
	Instability in Pluralities	−.17		
	City Size and Diversity	−.18		
	Urban Industrialization	.00		
	Local Machine Strength	.13		
	Equation 2			
Change in Local Turnout	Social Mobilization	−1.9868	−.26*	1.0560
	City Size and Diversity	−2.2553	−.35**	.8698
	Stable Opposition	−5.2144	−.24*	2.9134
	(Constant) .6793	F=6.52 with 3,42 d.f.	R^2=32**	
	Variables with F less than 1.5 not in the equation			
	Variable	Partial		
	Urban Industrialization	.03		
	Agro-Business	−.04		
	Local Machine Strength	−.06		
	Ethnic Uniformity	.05		
	Instability in Pluralities	−.07		

[a] Sources: stable opposition, instability in pluralities—from data of the Philippine Commission on Elections; other variables—same as Table 15.

* $p \leq .10$ on a one-tailed test.

** $p \leq .05$ on a one-tailed test.

in analyzing the situation of laborers, agricultural workers and the unemployed in the province of Pangasinan—notes that such individuals are

> economically very insecure . . . they participate to a more limited degree than any other category in the social, political, and religious affairs of the barrio.[70]

Similarly, Marxists have long been skeptical about the organizational potential of the lumpen-proletariat.[71] The weakness of social mobilization in explaining variance in voting turnout thus appears to be due to the curvilinear relationship between the two variables.

In Table 16 social mobilization is a determinant of change in both national and local turnout. To examine whether such drops in turnout are linked more specifically to the size of the middle and upper middle class, we recalculated the regression equations by substituting a more class-specific measure (the level of education) for social mobilization.

For local elections, substituting class-specific measures for social mobilization strengthens the Betas and weakens the influence of city size and diversity as another determinant of change in local turnout.[72] It appears then that declining local turnout can in part be explained by the lower interest in local affairs likely among the middle class. Since they are integrated into more specialized clientelist or nonclientelist structures such as the national bureaucracy or corporations, the middle sectors can only intermittently be served by local political machines.

[70] James N. Anderson, "Land and Society in a Pangasinan Community," in *Social Foundations of Community Development*, ed. Socorro C. Espiritu and Chester L. Hunt (Manila: R. M. Garcia, 1964), p. 179.

[71] See the discussion by David Matza, "The Disreputable Poor," in *Class, Status and Power*, 2nd ed., ed. Seymour Martin Lipset and Reinhard Bendix (New York: The Free Press, 1966), pp. 289–302.

[72] For change in local turnout, substituting the percentage of the population that is either high-school or college-educated for social mobilization raised the absolute value of the Beta slightly to −.29. When only the percentage of the population college-educated alone is substituted, the absolute value of the Beta is raised even further to −.33.

More class-specific measures are weaker determinants of change in national turnout than is the general measure of social mobilization, however.[73] For drops in national turnout, our argument that relates measures such as social mobilization to the general deterioration in clientelist structures for mobilizing broad groups of people appears plausible.

While changes in national and in local turnout are intercorrelated ($r=.52$), the change in national turnout appears to be much more than solely a consequence of changes in local turnout. That is, there are significant partial correlations between changes in national turnout and the following variables, controlling for changes in local turnout: social mobilization ($r=-.41$); ethnic uniformity ($r=.40$); local machine strength ($r=.31$). More class-specific measures such as the percentage of the population that is college-educated, by contrast, are only weakly correlated with changes in national turnout ($r=-.19$). Both ethnic uniformity and local machine strength appear to support continued clientelist mobilization, as evident in their positive correlations with changes in turnout.

Stable opposition is a significant determinant of change in national and, to a lesser extent, in local turnout. Cities with Liberal Party mayors, given their opposition to Nacionalista presidents (elected in 1957, 1965, and 1969), are more likely to be deprived of important national patronage resources needed to mobilize clients. Since the cost of elections has been rising, declining turnout in Liberal Party areas may result from the economics necessary to achieve victory at the lowest expense. Declining turnout in Liberal Party areas does not appear to be also a function of the institutionalization of competition in such areas. Assuming that institutionalized competition is reflected in stable pluralities over time, the low correlation between stable opposition and the instability of pluralities ($r=.09$) suggests that oppositionist areas do not have particularly stable voting blocs. Furthermore, if stable party affiliation of mayors over time were a good indicator of institutionalized competition, one would expect some correlation between stable party affiliation (regardless of the party) and declining turnout. Change in national turnout is insignificantly correlated with stable party affiliation ($r=-.19$) and also with stable Nacionalista Party affiliation ($r=.02$).

[73] For change in national turnout, substituting the percentage of the population either high-school or college-educated for social mobilization decreased the absolute value of the Beta to $-.21$. When the percentage of the population college-educated alone is substituted, the absolute value of the Beta is decreased even further to $-.18$.

The negative partial correlation in Table 16 between change in turnout and instability in local and national pluralities suggests, if anything, that factional instability over time is related to declining turnout. Our data provide no evidence that declining turnout is the result of a stabilization in factional competition.

A number of conditions help explain why agro-business is a determinant of increased turnout in national but not in local elections. We have already suggested that in areas where local elite power is strong, machines have more incentive to mobilize all their clientele in national as against local elections. The dependence of mines and agro-business on national sources of capital and credit as well as on favorable export-import policies provides further incentives for managerial personnel to collaborate with local politicians to deliver a maximum of support to favorably disposed national candidates.

Many of our previous results have indicated that deterioration in the integrative ability of political machines appears in a number of instances to be both a cause and a consequence of conditions which have decreased the proportion of income received by the bottom 40 per cent of the population between 1956 and 1971.[74] While such a deterioration is either irrelevant to or enhances the economic position of the middle and upper class, it means that urban workers and peasants are more fully exposed to market forces increasing the concentration of wealth. The dramatic rise in violent and mass urban activities, precisely during the period in which turnout declined, suggests that alternative structures capable of integrating and providing security for broad groups of people have yet to emerge on the Philippine scene.[75]

Much of the recent Moslem insurgency in the South of the Philippines can be traced to a similar failure of clientelist structures to provide economic security. The tendency to examine the insurgency in the South as an encroachment by a Christian majority on the land and customs of Moslems ignores the role of the old Moslem feudal elite in the process of economic deterioration. Co-opted into the national political and

[74] Figures on economic concentration are from the Bureau of Census and Statistics, *Survey of Households, Family Income and Expenditures: 1957* and Bureau of Census and Statistics, *Survey of Households, Family Income and Expenditures: 1971*.

[75] Clarence Abercrombie III (counting front page stories, letters to the editors and political cartoons dealing with the subject of revolution in a sample of *Manila Times* in 1951, 1960, and 1970) found a decline in the mean items per day dealing with revolution from 1951 to 1960 (from 2.62 to .31). Between 1960 and 1970 the mean rose to 7.03. Clarence Abercrombie III, "Vietnam and the Philippines: A Comparative Study of Revolution in Southeast Asia" (unpublished manuscript, Yale University, 1970).

economic arena, this elite lost touch with its clientele in the pursuit of its increasingly national and personal interests. Rebel leaders have charged Moslem leaders with corruption and selling communal land for personal gain, while ignoring the needs of the poor Moslems.[76] The class as well as ethnic basis of this insurgency should not be ignored. While interethnic conflict suggests very little about a change in the viability of clientelist structures as such, emerging class conflict between lower- and upper-class Moslems emphasizes the susceptibility of insecure clients to the more explicit class appeals utilized by the Rebel forces.

Conclusion

Capital accumulation by the indigenous elite and foreign investment help cause sociodemographic changes, which in turn encourage rising political factionalism and declining voting participation. As national elites become more powerful through the accumulation of capital and expropriation of increasingly large portions of social income, local political machines are confronted by structural changes that lessen their integrative capacity. The weakening of machines is not, as in the United States, the result of anti-machine activities by a rapidly growing middle class; consequently, a significant proportion of the Philippine population (especially the lower classes) becomes more vulnerable to market forces which weak machines cannot assuage. Since alternative integrative mechanisms such as a mass-based party or a dominant, partially clientelist party (e.g., the PRI in Mexico) have yet to emerge on the Philippine scene, the increasing weakness of machines intensifies the potential for more violent forms of mass activity.

The widening gap between the masses and the national elite that arises from the commercialization of clientelist exchange relationships may ultimately provide the stimulus for the reintegration and participation of individuals through class- or mass-based parties. In the short run, however, the response to increased factionalism and social unrest appears more a recognition by sectors of the national elite that control through increasingly expensive elections is both unwieldly and inefficient. The declaration of martial law, representing a shift to more bureaucratic and coercive control, will no doubt provide short-run stability.[77]

There is certainly no guarantee, however, that the change to a more hierarchical clientelist structure will provide long-run political stability. While the curtailment of public elections places fewer strains on public resources, it is unclear whether the savings inherent in such a curtailment either are sufficient or will be used to provide security for broad masses of people. The vote provided a minimal amount of leverage, however small, which forced the distribution of limited resources to the masses. If such individuals are further ignored, the concentration of income will accelerate. Both Marcos's renewed encouragement of foreign investment and his effort to curb inflationary pressures by curtailing expensive elections suggest a posture particularly responsive to much of the middle and upper class.

Both continued capital concentration by the elite and encouragement of foreign investment are unlikely to rectify certain of the structural imbalances in the Philippine economy. The consequences of such imbalances include an increased gap in the standard of living of different population sectors and an increase in unemployment. The government's move to more direct methods of bureaucratic control further limits the ability of lower class groups to press demands. Neither the provision of economic security nor the generation of adequate wage employment through government efforts appears probable in the near future.

[76] James P. Sterba, "Filipino Rebels Urge Ouster of Chiefs," *New York Times*, March 24, 1973, p. 3.

[77] Under martial law declared on September 23, 1972, the military and executive sectors are given extraordinary power.

An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems*

KENNETH JOWITT
University of California, Berkeley

Considerations of a Theoretical and Methodological Order

For at least some students of Marxist-Leninist regimes, political culture is becoming a salient analytic and research perspective.[1] There are several reasons for this development. First, there is the question of intellectual and theoretical stance. Those students of revolutionary change who define revolution as involving decisive but not absolute change tend to be sensitive to the role of cultural elements in contributing to the character of political systems. This understanding of revolutionary change is one which accepts the assertion that revolution involves "fundamental and even abrupt ruptures with the past in many areas of life . . . ," but that "for good or ill, revolutions never shut the door to the past."[2] Political culture also recommends itself to those who approach the phenomenon of social change by focusing on personality factors.

Second, recognition of diversity seems to be closely related to an appreciation of political culture as a research focus. It is interesting to note that during the period of Stalinism and the hegemony of the totalitarian thesis in the West, the major analytic approach to Marxist-Leninist regimes was a rather crude form of social-systems analysis.[3] Approaches of this order tended to discount or neglect the role of culture, largely because the relationship between regime and society was viewed simply as a pattern of domination-subordination. Similarly, during the reign of the "end of ideology" thesis in the West, structural-functional models dominated the analytic field. Only with the recognition of diversity does the analytic focus tend to expand and include cultural considerations.[4] The particular expression of diversity which has prompted consideration of foci that are complementary to a systems-approach has to do with the visible and systematic impact society has on the character, quality, and style of political life. One expression of this impact is the systematic discrepancy that occurs between statements about the ideal operation of a political system and its actual operation.

Third, political culture assumes greater salience as an analytic and research perspective as: (a) Marxist-Leninist regimes themselves redefine their task priorities from transformation and consolidation to modernization, (b) the inter-regime community of ruling parties loses its cohesiveness and power as a membership group and identity referent,[5] (c) the social composition of the parties

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[1] I have in mind Frederick Barghoorn's *Politics in the USSR* (Boston: Little, Brown & Co., 1966); Zbigniew Brzezinski and Samuel Huntington's chapter on "The Political System and the Individual," in *Political Power: USA/USSR* (New York: Viking Press, 1963); Richard Fagen's, *The Transformation of Political Culture in Cuba* (Stanford: Stanford University Press, 1969); Richard Solomon's, *Mao's Revolution and the Chinese Political Culture* (Berkeley: University of California Press, 1971); Lucian Pye's, *The Spirit of Chinese Politics* (Cambridge, Mass.: The M.I.T. Press, 1968); and Robert Tucker's "Culture, Political Culture, Communism" published in the *Newsletter on Comparative Studies of Communism*, 4 (May, 1971), 3–12.

[2] Benjamin I. Schwartz, "Continuity and Discontinuity in Contemporary China: Some Methodological Questions," *Bucknell Review*, 19 (Spring, 1971), 115–124, at p. 121.

[3] An exception to this was the study by Bauer, Inkeles, and Kluckhohn, *How the Soviet System Works* (New York: Vintage Books, 1956). In certain respects the argument to be presented in this article consciously builds on the work initiated in that volume.

[4] It is not surprising that the first formulations of the political culture approach preserved the existing and rather crude paradigmatic distinctions made between totalitarian and liberal-democratic regimes. Just as the "system theorists" responded to the changes after Stalin with the fallback concept of "rational totalitarianism," so students of political culture came up with the distinction of *pragmatic and ideological cultures*. (See Brzezinski and Huntington's, contrast of Soviet *ideology* and American *political beliefs;* and S. Verba, "Conclusion: Comparative Political Culture," in *Political Culture and Political Development*, ed., Lucian W. Pye and Sydney Verba (Princeton: Princeton University Press, 1965), pp. 544–550. There are a number of problems connected with this type of distinction: (a) it is ethnocentric and obstructs through definition a concern with the complexities of all cultures; (b) it is unnecessarily static (it might for example prove more useful to consider both the United States in 1776 and the Soviet Union in 1917 as ideological and both in 1972 as pragmatic); (c) it fails to sensitize the analyst adequately to comparison of different ideological cultures, i.e., the U.S. in 1776 and in 1948, the Soviet Union in 1917, 1928, and 1957.

[5] On this point see Kenneth Jowitt, "Political Integration and Political Identity in Eastern Europe," in *East Central Europe in the Seventies*, ed. Sylvia Sinanian, Istvan Deak, and Peter C. Ludz (New York: Praeger Publishers, 1972), pp. 180–184.

themselves changes, and (d) their posture towards the non-Marxist-Leninist world changes.

For a variety of reasons, then, political culture is currently becoming a salient analytic-research focus for certain students and a research-policy focus for certain Marxist-Leninist regimes.[6]

What is Political Culture and How to Study It. As conceived in this study political culture is a complementary perspective, not a substitute for other approaches or a single-factor explanation. This understanding of its role is related to the assumption that social reality is complex and that the function of theory is not to deny this complexity but to make it more intelligible. The definition of political culture used here ideally sensitizes the observer to certain relationships between regime and society and allows him to approach these relationships in a way that contributes to our understanding of the multifaceted reality characterizing these social systems.

Drawing an analogy with organizational theories, one can note at least three ways of studying Marxist-Leninist social systems. Both in the study of American factories and in the study of the Soviet Union the first major analytic statements were primarily structural. In the first case this took the form of Taylorism and in the second totalitarianism. In each instance the range of phenomena generated by structural relationships but not clearly consistent with them was perceived on the one hand by executives of Western business organizations as "anomalous," and on the other by Leninist elites as "bourgeois remnants." Analysts viewed such phenomena as either inconsequential and humorous or, when it came to Soviet type regimes as indicative that the efforts to change these societies had been superficial. Whether in an American factory or a Soviet-type regime, the informal, cultural, and covert aspects of the system were inadequately integrated into the theory that was used to explain the unit's character.

In organization theory the reaction against this structural, formal organization bias came with Mayo's human relations school. Mayo shifted the emphasis to an appreciation of human sentiment, of the factory culture, and of the significance of informal groupings. In recognizing the significance of the human factor, Mayo failed, however, to attend adequately to critical considerations such as the power structure of the factory. I would argue that within the field of comparative communism a development that parallels the Mayo school in organization theory is Solomon's work on China. Solomon also emphasizes the cultural dimension at the expense of the structural dimension. At least two major costs are associated with this failure. First, the terms of his analysis do not readily lend themselves to a discriminating comparative analysis of other Marxist-Leninist systems, largely because of his pursuit of the "unique" elements of Chinese political culture. Second, rather ironically much of what Solomon perceives as uniquely Chinese turns out to be something else. For example, the "uniquely" Chinese ambivalence toward authority and tendency to avoid conflict are, according to Crozier, distinctive and basic features of contemporary French political culture.[7] The outcome of this "exceptionalist" approach to political culture may very well create more obstacles in the way of comparative analysis and consequently in the way of establishing what is truly distinctive about individual Marxist-Leninist political structures and cultures.

A third approach to the study of organizations is that suggested by Michel Crozier. Crozier suggests that organizations should be analyzed in terms of the interaction of formal structure and informal relationships. In his terms these two elements should not be opposed. "They interpenetrate and complete each other. If one wants to understand them, one must study them together along with the system of power relationships that helps integrate them."[8] It is in this vein that I approach the study of political culture in Marxist-Leninist systems. Political culture will be studied in conjunction with political structure; structure and culture will be viewed as establishing mutual, though not necessarily equal, limits for one another. Approaching the concept of political culture from this perspective allows us to (a) take explicit account of the cognitive element of politi-

[6] In recent years the Romanian, Bulgarian, Chinese, Hungarian, and Albanian regimes have all directed their attention, in the form of Central Committee discussions and resolutions, to the realms of education, ideology, and culture. The Chinese case is clear to all in the form of the Great Proletarian Cultural Revolution; for the Romanians see *Scinteia* July 7, 13, November 4, 1971; for Bulgaria see the article by Marin V. Pundeff, "Bulgaria Under Zhivkov," and for Albania the article by Nicholas C. Pano, "Albania in the Sixties," both in *The Changing Face of Communism in Eastern Europe*, ed. Peter A. Toma (Tucson, Arizona: University of Arizona Press, 1970), pp. 89–121 and pp. 243–281. For Hungary see the comments of Barnabas Racz in "Political Changes in Hungary After the Soviet Invasion of Czechoslovakia," *Slavic Review*, 29 (December 1970), 638–639; and Bennett Kovrig, *The Hungarian People's Republic* (Baltimore: The Johns Hopkins Press, 1970), pp. 131–183.

[7] See Richard Solomon, *Mao's Revolution*, also Solomon, "Mao's Effort to Reintegrate the Chinese Polity: Problems of Authority and Conflict in Chinese Social Processes," in *Chinese Communist Politics in Action*, ed. A. Doak Barnett, (Seattle: University of Washington Press, 1969), pp. 271–365; and Michel Crozier, *The Bureaucratic Phenomenon* (Chicago: University of Chicago Press, 1964), chapters 8 and 9.

[8] Crozier, *Bureaucratic Phenomenon*, p. 164.

cal culture and to see political culture largely as a response to a regime with a given organizational format,[9] (b) avoid limiting the notion of political culture to the level of individual psychology as most students of the concept have done, (c) perceive the systemic significance of what is currently seen as either anomalous or unique in certain Marxist-Leninist political cultures, and (d) generate hypotheses dealing with questions of conflict, change, and system identity. In short, the attempt here is to devise an analytic understanding of political culture that facilitates comparison of Marxist-Leninist regimes in light of the interaction between formal-structural and informal-cultural elements, thereby avoiding on the one hand the exceptionalist or uniqueness pitfall and on the other the tendency to assert through definition that structure alone is necessarily decisive in shaping the political character of Marxist-Leninist regimes.

The relationship of political culture and political structure may be compared to the relationship that exists between the formal and informal organization of a factory. According to Barnard, "when formal organizations come into operation, they create and require informal organizations . . . they are interdependent aspects of the same phenomena—a society is structured by formal organizations, formal organizations are vitalized and conditioned by informal organization . . . there cannot be one without the other."[10] Barnard argues that informal organization may be "regarded as a shapeless mass of quite varied densities, the variations in density being a result . . . of formal purposes which bring [people] specially into contact for conscious joint accomplishments. . . . Thus there is an informal organization of a community, of a state."[11] It is the informal organization of the state that we shall refer to as political culture. More precisely, *political culture refers to the set of informal, adaptive postures—behavioral and attitudinal—that emerge in response to and interact with the set of formal definitions—ideological, policy, and institutional—that characterize a given level of society.*

Perceiving the different levels of society one can differentiate three types of political culture: elite, regime, and community political culture.[12] *Elite political culture* refers to the set of informal-adaptive (behavioral and attitudinal) postures that emerge as a response to and consequence of a given elite's identity-forming experiences. One might for example analyze the Romanian and Chinese elite political cultures in terms of the difference between a conspiratorial-prison experience and a guerrilla-partisan experience.

Regime political culture refers to the set of informal-adaptive (behavioral and attitudinal) postures that emerge in response to the institutional definition of social, economic, and political life. One would expect Marxist-Leninist regime cultures to vary according to whether a given regime structure was predicated on a command or semi-market principle and whether it emphasized the participatory or command component of democratic centralism.

Community political culture refers to the set of informal-adaptive (behavioral and attitudinal) postures that emerge in response to the historical relationships between regime and community. For example, one would expect very different community political cultures to emerge from a society in which organized religion stresses ritual and a mediated relation to God and one in which organized religion minimizes ritual and argues for a direct relation to God; similarly, the community political culture of a society in which state and society are related on the basis of an effectively institutionalized citizen-role should differ considerably from a society in which the mass of society has been historically excluded from political recognition and participation.

Having dealt with the basic orientations, concerns, and definitions, I should outline the basic analytic framework we shall employ to study political culture in Marxist-Leninist regimes. Reflecting the contention that the study of political culture is most fruitful in connection with the study of political structure, our two independent variables shall be structural. It is my thesis that all Marxist-Leninist regimes are oriented to certain core tasks that are crucial in shaping the organizational character of the regime and its

[9] With regard to (a) see the very important article by Carole Pateman, "Political Culture, Political Structure and Political Change," in the *British Journal of Political Science*, 1 (July, 1971), 291–305.

[10] Chester Barnard, *The Functions of the Executive* (Cambridge, Mass.: Harvard University Press, 1962), p. 120.

[11] *Ibid.*, p. 115.

[12] To date most studies of political culture have been made at the community level. Recently Robert Dahl has commented that "although students of political culture call attention to differences between elite and mass political cultures . . . up to now they have paid a good deal less attention to the beliefs of political elites." See *Polyarchy* (New Haven: Yale University Press, 1971), p. 167. For a perceptive analysis of elite culture, see Robert Putnam's "Studying Elite Political Culture: The Case of Ideology," in the *American Political Science Review*, 65 (September, 1971), 651–682. To date, however, no one has argued for or analyzed a third arena, that of the regime in contrast to the elite and community levels. Among system oriented studies there is of course Easton's differentiation of the regime from the elite and community arenas. See David Easton, *A Systems Analysis of Political Life* (New York: John Wiley & Sons, Inc., 1965), pp. 154–247.

relationship to society.[13] These tasks include: (1) *transformation*—the attempt to alter decisively or destroy values, structures, and behaviors which a revolutionary elite perceives as comprising or contributing to the actual or potential existence of alternative centers of political power;[14] (2) *consolidation*—the attempt to create the nucleus of a new political community in a setting that ideally prevents existing social forces from exercising any uncontrolled and undesired influence over the development and definition of the new community; and (3) *modernization*—the regime's attempt to develop more empirical and less dogmatic definitions of problems and policy, a formal, procedural approach rather than a substantive, arbitrary approach to the solution of problems, and an understanding of the executive function that stresses leadership rather than command competences.[15] Each of these tasks has organizational or structural corollaries. Transformation involves a confrontation between the regime and the "unreconstructed" society. Consolidation yields a structure of domination as the politically defeated but "hostile" society must be prevented from "contaminating" the nuclei of the new socialist society. Modernization, however, requires a rather significant redefinition of the relationship between regime and society from mutual hostility and avoidance to the regime's selective recognition and managed acceptance of society.[16] Depending on the core task, then, the regime will be structured differently both internally and in relation to society. It is to be expected therefore that (a) the character of political culture will vary with changes in tasks or task-mixes and (b) the introduction of a new task and regime structure will confront an already existing set (elite, regime, and community) of political cultures.

The second independent variable we shall work with is central ideological tenets. Briefly stated, all Marxist-Leninist regimes define themselves in terms of their commitment to certain ideological principles. We are interested in two: the dictatorship of the proletariat and democratic centralism. These are of particular significance precisely because they have explicit and direct structural implications for the organizational definition of the regime itself and for the organizational or structural relationship between regime and society. Core task and central ideological tenets with their structural correlates provide the boundary conditions for the organizational definition of the regime and its relations with society. It is assumed that they directly contribute to the type of political culture that develops at the elite, regime, and community levels. The framework, however, is still not complete. Organizations do not occupy vacuums but are defined in opposition to already existing organizations that have themselves generated corresponding cultures. Consequently, the process of specifying and acting on the organizational correlates of core tasks and ideological tenets is in part shaped by existing cultural postures. Furthermore, once a Marxist-Leninist regime has come to power and established itself organizationally, the political cultures it generates assume a certain integrity of their own and begin to reinforce the regime's structure. For both these reasons it is useful to consider political culture in the terms suggested by Harry Eckstein, namely, as an intervening variable.[17] In this formulation political culture provides some substance for the "black box" between the stimuli (independent variables) such as core task and central ideological tenet and the responses (dependent variables) such as the ease with which certain tasks are accomplished in contrast to others, the manner in which they are acted upon, and the ease or difficulty a

[13] An argument and analysis of changes in Marxist-Leninist systems based on different tasks is currently being written by the author with the title "Stages of Development in Marxist-Leninist Systems: A Comparative Analysis."

[14] See Kenneth Jowitt, *Revolutionary Breakthroughs and National Development: The Case of Romania, 1944–65* (Berkeley: University of California Press, 1971), pp. 8–9, 107–108.

[15] Leadership competences refer to an elite's ideological acceptance of the political membership credentials of the mass of society. Nonparty members are considered bona fide participants rather than unreconstructed inhabitants of the national community. Tenets such as "state of the whole people" and "party of the whole people" are illustrative of this shift (which of course can be reversed or modified). Structurally, the shift from command to leadership competences involves the shift from coercive to manipulative skills from a pattern of regime exclusivity to one of greater complementarity vis-à-vis society. Such a shift is illustrated by the decreased role of the secret police and the at least formal attempts to upgrade party activists in comparison to full time party functionaries (apparatchiks). Behaviorally, the shift in competence is reflected in new styles of leadership, greater emphasis on leadership visibility (i.e., Khrushchev vs. Stalin, Gierek vs. Gomulka, Dubcek vs. Novotny), direct contact with diverse social constituencies, and attempts to elicit social response to and confirmation of policy initiatives before the latter are given official status by the party.

[16] Selective recognition refers to the regime's willingness to expand power by allowing greater functional autonomy in various organizational settings; managed acceptance refers to the regime's organizational pre-emption of social strata with recognition aspirations or demands. An instance of managed acceptance in Romania is the recently formed Front for Socialist Unity, an instance of selective recognition is the emphasis on collegial decision making in factories.

[17] Harry Eckstein, "Memorandum to Participants in Conference on Political Culture and Communist Studies."

regime has in redefining its core task (i.e., from consolidation to modernization).

We must now ascertain whether the Marxist-Leninist international subsystem provides a recognizable and widespread structural format for dealing with the tasks of revolutionary transformation and political consolidation. Is there in the historical experience of some of these regimes a shared definition, a similar formal-organizational approach to the tasks of transformation and consolidation? And if there is, what are the elements that define the character of this format, what are the consequences of this regime format for the political culture of these systems, and what is the significance of the interaction of this culture with regime structure for political change?

System Building and Political Culture. Although there are fourteen Marxist-Leninist or Communist states, it is an historical fact that the Soviet Union's mode of development under Stalin has usually been the authoritative model, especially for most East European regimes. In addition, for regimes such as the Cuban, Chinese, and Yugoslav, the Stalinist model was either adopted and then later rejected or continues to be espoused as a model by certain elite members. The Stalinist mode of transformation and consolidation may be termed a system-building approach. It is this approach that has shaped the overall character of the Soviet regime and the majority of Marxist-Leninist regimes. And it is the legacy of this approach which these regimes must deal with in their current efforts to modernize.

System building contains three major related components. First, the dictatorship of the proletariat becomes the defining relationship for interactions both between regime and society and within the regime. One can specify three structurally significant aspects of this political principle: (a) the explicit and sustained policy of separating the elite and regime sectors from and opposing them to the rest of society, (b) the explicit utilization of widespread coercion and violence and the subordinate role of persuasion in the initial phases of "constructing socialism," (c) the tendency of Leninist parties to monopolize the public sector through their assumption of comprehensive and direct responsibility for social developments and the corresponding concentration of decision-making powers within the Party. What is involved here is the denial of any integrity to the public realm as distinct from the official realm.

Second, system-building regimes are concerned with the rapid development of their societies and the sustained mobilization of resources. What is structurally significant is that relations between regime and society are organized along elite designated priorities. In Marxist-Leninist terms this is a "commanding heights" strategy, an approach to development that I choose to label "revolutionary laissez faire." In such a system the emphasis is on controlling decisive points and sectors in a given social subsystem and on structuring a society on the basis of tacit trade-offs or exchanges. In return for performance in priority sectors, and with respect to priority items, members of society are "allowed" to manipulate non-priority sectors for their private benefit.[18] A variety of causal factors underlie this type of development strategy; some, such as the inadequate number of cadres are historical while others, such as the concern with rate of development,[19] are more ideological. Of interest here, however, is the type of regime organization and regime-society relations produced by this "commanding heights" or priority strategy. As Simon has noted, any formal organization "will always differ from the organization as it actually operates in several important respects. First, there will be many omissions in it."[20] As Simon goes on to note, what is decisive is whether or not the formal organization sets limits to the informal relations that develop within it. Mobilization in a system-building regime means that while all social areas are provided with political limits, several areas do not receive the kind of attention that characterizes priority areas. To emphasize the distinction, non-priority areas are controlled rather than transformed. In this sense they are "omitted" by the regime.

Third, system-building regimes are led by elites with "production mentalities." The Stalinist approach to socialist construction has been one of "walking on one leg,"—i.e., of defining success in constructing socialism as a function of political, economic, and social breakthroughs while viewing cultural transformation as basically a derivative accomplishment. The predominance of the production mentality in system-building regimes manifests itself in the following ways: (a) as an "island-hopping strategy" whose premise is that with the redefinition of the economic, political, and social domains the cultural domain can be effectively circumscribed, transformed, and the few "remnants" of bourgeois origin gradually

[18] For a vivid description of this exchange relationship see Merle Fainsod, *Smolensk Under Soviet Rule* (New York: Vintage Books, 1963), pp. 85 and 151. While there are variations in the format over time and between countries the essentials of this relationship persist in many Soviet-type systems.

[19] See Kenneth Jowitt, "Time and Development Under Communism: The Case of the Soviet Union," in *Temporal Dimensions of Development Administration*, ed. Dwight Waldo (Durham, North Carolina: Duke University Press, 1970), pp. 233–264.

[20] Herbert Simon, *Administrative Behavior* (New York: The Macmillan Company, 1961), p. 148.

"mopped up,"; and (b) as an instrumental approach to culture and individuals that attends to such areas when they directly affect the regime's ability to act effectively on its priorities—social and economic change.

The significance of these major aspects of the system-building approach is twofold. First, a system-building approach to the tasks of transformation and consolidation *unintentionally* reinforces certain features of the existing (non-Leninist) community political culture that are considered negative by Marxist-Leninist parties. This reinforcement occurs largely because of the congruence between major elements in the ideological and organizational format of system-building regimes and major elements in the community political cultures of the various societies that have become Marxist-Leninist. Second, as these societies develop under Marxist-Leninist auspices, they characteristically generate challenges to existing structural and *cultural* definitions and postures at the elite, regime, and community levels.

One implication of this analysis is that the current challenges faced by most Soviet-type regimes are not simply threats to power and efficiency. Rather, the current crises in these systems involve the confrontation of different structural-cultural sets, and what is at stake is not only power and efficiency but the *character of political authority and social relations*. This dimension of current developments has not been adequately examined and the study of political culture can greatly enhance our understanding of the complexity and relatedness of developments within these regimes.

The Paradoxical Character of System Building

It is paradoxical that in their attempt to critically redefine society, Marxist-Leninist regimes simultaneously achieve basic, far-reaching, and decisive change in certain areas, allow for the maintenance of pre-revolutionary behavioral and attitudinal political postures in others, and unintentionally strengthen many traditional postures in what for the regime are often priority areas. How and why are the obvious questions. In order to answer them, to deal with the substantive as well as formal dimensions of political culture in Marxist-Leninist systems, an empirical referent is required. For that purpose we shall focus primarily on a single political culture—that of the Romanian Socialist Republic—and on the interaction between its regime and society. The analytic results of this exercise should ideally be a greater appreciation of the partially contradictory character of Romanian development since 1948 and a heightened appreciation of the character of development and developmental problems in other system-building regimes.

The Dictatorship of the Proletariat and Political Culture. We have already stated that as a structural or organizational principle the dictatorship of the proletariat involves the separation of the elite and regime sectors from the rest of society. This separation has occurred in Romania and has also occurred in similar regimes. Its rationale was twofold. Initially, it was predicated on the need to minimize regime commitments to a politically unreconstructed and hostile society. After the achievement of a decisive political breakthrough, the posture of avoidance was still maintained, adhering to the Soviet thesis that even after the political defeat of the old regime, its influence still threatens the construction and consolidation of new institutions, roles, and values.[21]

In at least two respects the separation or dichotomization of regime and society had a critical impact on the cultural orientation of both elite and non-elite segments of the Romanian population towards the political realm. The first is the reinforcement of a status ordering of regime and society. In an article signaling a campaign against *bakshish*, its author noted that such a practice properly belonged to a period in the past, to a time when "our (Romanian) society experienced domination by a stratum of rulers or boyars," and he asked how a practice associated with such a political structure could survive in a socialist society.[22] The answer to this question can be found in the structural similarity between the dictatorship of the proletariat and the historic relationship of state and society in Romania. During the period of the dictatorship of the proletariat,[23] the Romanian Workers Party was in effect a corporate, privileged, status group that asserted its identity and perquisites *in opposition to* the rest of society. Our purpose here is not to deny the significant social, economic, and cultural transformations initiated during the Gheorghiu-Dej period, but rather to focus on a feature of this period that has not been examined by Western scholars or understood by the Romanian party itself.

Given its goals of transformation and consoli-

[21] See J. V. Stalin, *The Foundations of Leninism* (Peking: Foreign Languages Press, 1965), pp. 42–43. "The bourgeoisie has its grounds for making attempts at restoration, because for a long time after its overthrow it remains stronger than the proletariat which has overthrown it." See also the passages from Lenin that Stalin quotes.

[22] *Scinteia*, July 23, 1971. The title of this article was, "Let us wipe out Bakshish: A practice incompatible with the ethical climate of our society."

[23] While the dictatorship of the proletariat is still an operative concept in Romania, the current regime's structure and policy in many respects depart from our working definition of this principle. When I use the term dictatorship of the proletariat I am referring primarily to the time period 1948–1964.

dation and its perception of how to achieve those goals, the Romanian elite created a political structure that in certain basic respects not only allowed for maintaining, but even reinforced, certain traditional political attitudes and behavior within both elite and non-elite members of the population. Just as *boiar* (nobleman) and subject had historically been mutually exclusive statuses, so cadre and citizen became mutually exclusive statuses rather than complementary roles. The structure of the regime made it possible for cadres to "consider a public office a source of personal income and those in their charge as serfs."[24] The *nomenclatura*[25] became the equivalent of the legal underpinnings of nobility, an organizational statement that a certain category of the population was (is) subject to special considerations. Thus, Mihai Gere, a member of the Romanian party's secretariat, has noted that "sometimes those (cadres) who damage (*aduc prejudicii*) our society (i.e., through corrupt behavior) are shifted to another position or even promoted instead of being removed from their work."[26] Such behavior is not unnoticed by those who are lower in the political and social hierarchy. In contrast to the ideal regime criterion of worth based on social discipline, responsibility, and political maturity, the popular understanding of what is actually the criterion of worth reflects the dichotomous status ordering of regime-society relations. Reporting on an organization's failure to punish an accountant for proved offenses, a correspondent for the party newspaper *Scinteia* concluded that the incident involved "the removal of our principle criteria for appreciating men and their deeds (and their replacement) with subjective criteria." What were these subjective criteria? It seems that the prosecution of offenders depended on whether or not the individuals involved were *cei cu functie sau fara functie* (those with official positions or those without), in short the effective criterion was membership in the *categorie de privilegiati* (privileged category).[27]

There is some evidence that the posture of non-elite members of society toward the political realm is based on this dichotomous structure of privileged versus nonprivileged. In another report about the use of public resources for private ends, the party correspondent noted that when the culprits were faced with their offenses without exception they adopted a lamentable position, admitting only to the crime of not occupying a prominent enough position (*suficient nivel*).[28] Another example of the understanding which elite members have of their position, perquisites, role, and relation to political life is found in an article that discusses the phenomenon of *razgindirii* or rethinking of a verdict.[29] Although the reasons behind this phenomenon vary, depending on the case, it appears that local party units often act much like other corporate-privileged groups in trying to protect "their own."

It is significant that the tendency to dichotomize elite and non-elite membership during the dictatorship of the proletariat has reinforced the political culture that existed prior to the rule of the Communist party, a political culture in which the elite sector was distinct in character and prerogative, not simply in role. We have examined some instances of this perspective at the elite level; let us now turn again to the mass level.

It is striking that system-building regimes based on the dictatorship of the proletariat have reinforced many pre-Leninist political cultural dispositions (attitudinal and behavioral) at the community level. By responding adaptively to the political reality of a status-based relationship between regime and society, many members of Romanian society continue to behave in partially traditional modes. For example, it is often necessary for the ordinary citizen to *stimula atentia*, or gain the attention of those with a regime position, a doctor, bureaucrat, or party cadre, and many individuals with such positions demand *mica atentia*, a little attention (i.e., money) before the performance of a public service.[30] The terms themselves are revealing. In a society based on explicit and authoritative status differences it is rational for non-elite members to act on the premises enforced by the elite sector. As one of the "mass," the ordinary citizen must distinguish himself from other non-elite subjects if he is to get a response; he must "stimulate" the attention of a superior. Doing this is simultaneously a means of gaining the superior's attention, recognizing the superior's elite status, and often a means of decreasing the uncertainty and anxiety of the encounter.[31]

The widespread phenomenon of *pile* or "pull," "connections," may also be viewed as a continuation of pre-Leninist political culture and as an adaptive-informal response to a political structure

[24] *Scinteia*, July 23, 1971.
[25] The *nomenclatura* is a party listing of politically critical posts and the personnel eligible for them.
[26] *Scinteia*, November 5, 1971. See in this connection the article by Steven J. Staats, "Corruption in the Soviet System," *Problems of Communism*, 21 (Jan.-Feb. 1972), 40–48.
[27] *Scinteia*, November 13, 1971.
[28] *Scinteia*, November 30, 1971.
[29] *Scinteia*, November 10, 1971.
[30] *Scinteia*, November 17, 1971.
[31] In this connection see the arguments presented by Robert Price in "The Social Basis of Administrative Behavior in a Transitional Polity: The Case of Ghana" (doctoral dissertation, University of California, Berkeley, 1971), particularly chapters 1 and 5.

that devalues adherence to legal-rational rules. The dictatorship of the proletariat is, after all, defined by Lenin as the "rule—unrestricted by law and based on force—of the proletariat over the bourgeoisie. . . ."[32] *Pile* at the community level and in the interaction between community and regime is the informal, covert equivalent of the *nomenclatura* at the official level; both are mechanisms for dealing with a structure based on status, not on rule considerations.

The most important point, then, is that from the perspective of the structural dichotomization it generated, the dictatorship of the proletariat supported certain pre-Leninist political cultural dispositions at both the elite and non-elite level of the new Marxist-Leninist society. Ironically, these dispositions are essentially antithetical to the appearance of a Marxist-Leninist nation-state in which legal-rational (though not necessarily liberal-democratic) norms and institutions have a major role.[33]

It was asserted earlier that in at least two ways the dichotomous and oppositional relationship of regime and society contributes to the maintenance of a pre-Leninist political culture. We have examined the first, the reinforcement of a status ordering of regime and society. The second pertains to the basic cultural ethos of a peasant-traditional society, an ethos captured in Foster's brilliant notion of "limited good." According to Foster,

> broad areas of peasant behavior are patterned in such fashion as to suggest that peasants view their social, economic, and natural universes—their total environment—as one in which all of the desired things in life such as . . . friendship . . . honor, respect, status, power, influence, security, and safety, exist in finite *quantity* and *are always in short supply* . . . hence it follows that *an individual or a family can improve a position only at the expense of others*[34] [emphasis added].

In short, peasants tend to define relationships in zero-sum terms and to conceive of values rigidly and concretely. What is so striking in this instance is the high degree of congruence between the traditional cultural ethos of a peasant society and several Leninist-Stalinist precepts. Just as for the peasant, social relationships are zero-sum, so for Lenin and Stalin many of the relationships between the Party and society are also zero-sum. Examples include the basic political question raised by Leninists, *kto-Kovo* or who-whom—which political force will triumph? The outcome is seen as precluding long-term sharing of power: that is, either the bourgeoisie or the proletariat gains power—not both.

Similarly, the rigid Stalinist conception of the Party's leading role and of the unitary nature of leadership has made those whose political identities were formed during the Stalinist period fearful that a shift to collegial leadership would "dilute" the Party's power, place, and effectiveness. Finally, there is the Leninist-Stalinist aversion to faction; differences of opinion are often seen as a threat, as "weakening" the regime's unity.[35] It is remarkable how formally compatible these Leninist-Stalinist "limited good" interpretations of basic structural considerations have been with the "limited good" interpretations of value and structural considerations in a peasant society. Consequently, it is not surprising that the "limited good" conception of the dictatorship of the proletariat held by Lenin, Stalin, and their emulators in Eastern Europe, could enhance the traditional cultural postures of a peasant society and in turn be reinforced by them. As in the case of the hierarchical status relationship between regime and society, a major link between the regime's concep-

[32] Stalin, *Foundations of Leninism*, pp. 45–46.

[33] One can of course argue that a Marxist-Leninist regime cannot simultaneously maintain its political identity and incorporate legal-rational norms and institutions. In fact this assertion is one of the "proverbs" in the field of Communist studies. It may be a correct one but has not been demonstrated convincingly. Arguments about the incompatibility of Marxism-Leninism and Modernity often fail to recognize that all modern political communities are to varying degrees characterized by a conflict between several of the imperatives associated with modernity and the imperatives associated with the particular ideology of a given regime. Viewed in this light, Liberal as well as Leninist communities are characterized by persistent conflict between ideological commitments and commitment to a legal-rational ethos. One can identify one ideology (i.e., liberalism) with a particular ethos (modernity) and then conclude by definition that because Leninists aren't liberal they cannot be modern, or can only be modern if they become liberal; but it would be more productive analytically (a) to establish the range and type of conflicts that are typical of modern or modernizing societies with different ideologies, (b) to scrutinize more critically the varying dimensions of modernity and then attempt to establish what aspects of modernity different regimes are likely to stress and what institutional definitions of modernity they are likely to posit.

[34] George M. Foster, "Peasant Society and the Image of Limited Good," *American Anthropologist*, 65 (April, 1965), 293–315, see 296–297.

[35] One index of the modification of a Marxist-Leninist regime's character is its approach to internal party criticism. It is significant that since assuming power Ceausescu has repeatedly stated that criticism is not tantamount to disloyalty; however, the resistance to implementing this nondogmatic (though authoritarian) and nonlimited good notion of criticism is still widespread. See Nicolae Ceausescu, "Adunarea activului de partid al municipiului Bucuresti," in *Romania Pe Drumul Desavirsirii Constructiei Socialiste*, vol. 3 (Bucuresti: Editura Politica, 1969), pp. 199–200.

tion and definition of its organizational character and the society's response to it was that society's social composition. Romania was approximately eighty per cent peasant in the late 1940s. Peasants not only provided the regime with its major constituency but also with a large proportion of its membership. If one realizes that the urban population of Romania, particularly the industrial population, was basically peasants recently turned workers, one can better appreciate how easy it was for the large numbers of party cadres drawn from the ranks of the peasantry or peasant-worker stratum to understand, accept, and operate with the Party's "limited good" conception of its own identity, tasks, and prerogatives.[36] Here was a classic instance of the Party partially defeating its own purposes and not recognizing it. In its effort to recruit and socialize cadres, to create "new men," its very ideological images and organizational definitions acted in part to reinforce unintentionally the traditional view of the relationship between regime and society, a view that stressed the antagonistic, zero-sum character of that relationship. Once again the Party's attempts at transformation and consolidation produced a mixed set of outcomes.

Along with the separation or dichotomization of regime and society, an integral part of the dictatorship of the proletariat in Romania and elsewhere has been the regime's explicit and persistent use of coercion in relations between regime and society and within the regime sector itself. This coercion had a major impact on the type of political culture that has developed within these systems.

In a very perceptive article Emanuel Turczynski has pointed out the chasm that historically existed between state and society in Romania. The absence of an indigenous middle class, the "village oriented cultural and social structure," and the experience with foreign domination all contributed to a sharp difference between the private and social realms on the one hand and the public and official realms on the other. Turczynski suggests that this state of affairs was not conducive to the appearance of any effective popular identification with the state.[37] The coercive character of the dictatorship of the proletariat in certain respects (though not all) reinforced this separation of the private and public sectors of society, the antagonism between the two, and the perception of the public arena as a threatening, hostile, and alien sector. The Marxist-Leninist regime's employment of violence resulted in significant sectors of the population adopting a split posture, one of public compliance on the one hand and private avoidance, skepticism, or rejection on the other. A syndrome of this order is perfectly consistent with social psychological studies that have demonstrated, "if threatened punishment [for noncompliance] is very high, then compliance [public dependent influence] will occur without private acceptance. There is no dissonance since the high . . . punishment provides sufficient basis for the inconsistency between private belief or attitude and public behavior.[38]

And, in fact, it seems that the manner in which and the extent to which most Marxist-Leninist regimes employed coercion reinforced a cultural disposition that was highly calculative toward the political realm, a posture of complying publicly in order to protect and preserve one's private domain. Scalapino, for example, in his recent comprehensive study of North Korea notes that "a number of persons appear to adjust to the demands of an authoritarian state by developing a series of external responses including an intricate series of participatory actions that differ from and provide some protection for internal or private responses."[39] Unlike third-world regimes in which coercion and rewards are too low to produce compliance with modern norms, and unlike regimes such as the Chinese, which appear to have employed coercion more judiciously,[40] in their formative stages system-building regimes employed too much coercion and thereby reinforced what in certain respects might be termed a ghetto political culture. Before pursuing this point let me emphasize that the dictatorship of the proletariat in Romania and elsewhere has done more than unintentionally reinforce and enhance pre-Leninist political cultural postures that were and remain antithetical to several of a Marxist-Leninist regime's basic goals. This will become clearer in the final section of the article. One cannot, however, understand the current crises facing these

[36] See the recent article by Zygmunt Bauman, "Social Dissent in the East European Political System," in the *European Journal of Sociology*, 11 (1971), 25–52, for a much needed and very useful discussion of the importance of the "peasant factor" in the development of Marxist-Leninist regimes.

[37] Emanuel Turczynski, "The Background of Romanian Fascism," in *Native Fascism in the Successor States, 1918–1945*, ed. Peter F. Sugar (Santa Barbara, Calif.: ABC Clio Inc.), p. 109 and *passim*.

[38] Barry E. Collins and Bertram H. Raven, "Group Structure: Attraction, Coalitions, Communication, and Power," in *The Handbook of Social Psychology*, vol. 4, ed., Gardner Lindzey and Elliot Aronson (Reading, Mass.: Addison-Wesley Publishing Co., 1969), p. 181.

[39] Robert Scalapino, and Chong-Sik Lee, *Communism in Korea*, vol. 2 (Berkeley: University of California Press, 1972), p. 847.

[40] In this connection see Thomas Bernstein, "Leadership and Mass Mobilization in the Soviet and Chinese Collectivisation Campaigns of 1929–1930 and 1955–1956: A Comparison," *The China Quarterly*, 31 (July–September 1967), 1–47.

regimes without analyzing the ways in which they did reinforce and enhance such postures.

Historically, the Romanian community political culture appears to have stressed the antagonistic relationship between public and private spheres. Experience with the basically coercive, extractive, and status-based rule of Turks, Russians, Austrians, Hungarians, native boiars, and native regimes stimulated the development of what often resembled the political culture of a ghetto. As in the past and as in a ghetto, under the dictatorship of the proletariat the regime or official sphere represented "trouble" in being identified as the locus of demands and sanctions rather than of political support and recognition. The result was the adoption of a calculative, instrumental, and often dissimulative approach to the official or public sphere of life. In this respect the similarity to ghetto culture is quite clear. Rather than identify with regime values, norms, and goals, such as disciplined work, the community developed a set of postures that in many instances were antithetical to the regime's expectations. Certainly from the Romanian party's point of view its social investigations have too often discovered individuals whose highest value is *trai usor* (the easy life) and who place too high a value on *timp liber* (free time).[41] And yet, each of these postures is an instance of an informal-adaptive response to a political structure that has until very recently related to society in extractive and coercive terms, that failed to and in fact feared to identify with its society—a fear that may well be rational during the initial stages of a revolution but that produces mixed, unanticipated, and undesired consequences for a revolutionary elite.

Estrangement is the quality that unites the various postures we have referred to—the calculative, instrumental approach to social tasks and official priorities, the emphasis on free time and the easy life. If, as at least one student of the topic has argued, alienation "refers to an enduring sense of estrangement from salient objects in specified contexts," and denotes "the absence of basic attachments to and identifications with salient objects in a particular context,"[42] then it is plausible to conclude that during the period of the dictatorship of the proletariat (approximately 1948–1964) the Romanian regime partially reinforced the alienated posture of large segments of the Romanian population. The use of coercion and the easily identifiable source of that coercion —easy precisely because of the dichotomy the Party created between itself and society and because of the concentration of decision-making power within its boundaries—in certain respects sustained and increased the community distrust of things official, public, and political, and created a major obstacle for the regime in its current attempts to create a more positive relationship with society.

The third feature of the dictatorship of the proletariat component of system building is the marked tendency of such a regime to fuse the official or elite sector with the public sectors of society.

In one of Petru Dumitriu's novels, *Incognito*, there is a passage describing a party member's reaction to the criticism made of one of the higher party cadres at a social gathering. In response to the news of this occurrence the party member exclaimed, "I can't understand *why it has been allowed.*"[43] Phrased more formally, system-building regimes have tended to monopolize political discretion, initiative, and responsibility. Events do not occur, decisions are not made, and facts are often not recognized as facts until they are *allowed* to occur or to be recognized.

For such regimes the absence of a public domain with an integrity separate from that of the official-elite domain and the private-social domain has several consequences: Lacking any substantial control over their own definition and role, public institutions in such a regime often fail to elicit identification from those who work within them. Given the absence of any autonomous public domain, the Party's demands that individuals identify with the public interest rather than with their private interest are unrealistic; the political reality is that only two discernible interests exist: the regime's and the individual's. Without a differentiated public domain capable of eliciting identification from large sectors of society and with a regime that minimizes its commitments to society, it is not surprising to find so many instances of theft, pilfering, and lack of involvement with one's work. One could argue that given the fusion of official and public sectors and the low popular identification with these sectors, the public domain becomes the equivalent of a "treasure find" in a traditional peasant society. In such societies based on the notion of "limited good" one can only afford to appear more prosperous by attributing one's new wealth to a source outside

[41] See the article by Alexandru Ivasiuc in *Scinteia*, December 30, 1971, "Republica Muncii" (The Republic of Work); see also "Distractii inofensive care afecteaza grav destinele unor tineri," *Scinteia*, September 18, 1971; "Parazitul cu blazon," *Scinteia*, September 13, 1971; and "Acolo unde educatia si echitatea erau doar teme de sedinta . . . ," *Scinteia*, December 14, 1971.

[42] See Jack Citrin, "Political Disaffection in America, 1958–1968," (doctoral dissertation, University of California, Berkeley, 1972), pp. 66 and 73.

[43] Petru Dumitriu, *Incognito* (New York: The Macmillan Company, 1964), p. 53. Stress in the original.

the local community—simply because in a society based on an ethos in which resources are fixed, if one person adds to his well-being, it means someone else has become less well-off. Now, given the political structure of a system-building regime, specifically, its monopoly of all nonprivate domains, the public domain (i.e., the commercial sector of the economy) becomes for many the equivalent of a "treasure find." Prevented from effectively identifying as participants by the structure of the regime itself and often left to their own devices to supplement their income, the incidence of *ciupeli marunte* (petty pilfering) is understandable. While regimes such as the Romanian still do not recognize the causes of such behavior, they seem to be aware of the ethos that informs such behavior. One indication is the idiom that is employed when such practices are criticized. In a recent attack on theft, pilfering, and lack of work discipline the author of an article in *Scinteia* argued that "illicit gains are not *gifts of chance* [i.e., a 'treasure find'], they are the fruit of repeated violations of work discipline and of the law."[44] Apparently certain elite members are sensitive to the persistence of semi-traditional orientations to the public domain.

A second consequence of the regime's monopoly of the public domain, of its concentration of decision-making power, has been that those who have positions within the regime but not necessarily elite positions within the Party tend to separate the status and role components of their position. In his recent work on the social basis of administrative behavior in Ghana, Price discovered that Ghanaian bureaucrats tend to separate the status or prestige component from the role or behavioral component of their positions. His explanation is that the role demands of positions in a modern organization are incongruent with fundamental elements of traditional African culture and that consequently one would expect this separation to persist as long as those traditionally prominent elements retain their importance.[45] In Marxist-Leninist countries, particularly in the rural areas, one can find a tendency to separate the prestige from the behavioral component of official positions, and it is often related to the Party's organizational weakness in such areas, the low level of ideological sophistication among the cadres, and the claims made on them by families and friends. Additional and distinctive factors, however, are at work in Marxist-Leninist systems that lead officials to separate the prestige and role components of their positions. Given the sharp distinction made in the dictatorship of the proletariat between elite and non-elite positions, there is a corresponding tendency to emphasize the prestige component of one's position. The fact that historically the position of official was seen in terms of status and power rather than functions and services has only made the bias towards the prestige component easier to act on.

There is yet another reason, however, for the failure of many officials to act consistently with the formal definition of their position. In a system-building regime based on the dictatorship of the proletariat, where the Party places a premium on exercising a monopoly over politically relevant behavior and where the definition of politically relevant behavior is deliberately diffuse rather than specific, an official who bases his behavior primarily on the formal role prescription of his position is in effect posing a challenge to that monopoly. For to base official behavior on the formal role prescriptions of a given position is to have as one's main referent a universalistic, impersonal set of action determinants. In the framework of the dictatorship of the proletariat, however, the Party does not accept the value of such impersonality or neutrality. In fact, the dictatorship of the proletariat is defined explicitly in substantive, not procedural terms. The discrepancy between the concern of regime officials with status and their frequently poor performance in terms of the formal definition of their role is not primarily due, as in third world settings, to the conflict and incongruence between modern role prescriptions (i.e., treat all clients equally) and traditional cultural elements (i.e., favor one's relatives) but to the political penalities that often accompany attention to role-prescriptions rather than to political cues. Because regimes interested in transforming an existing society and consolidating the nuclei of a new one are committed to controlling and defining the premises and substance of politically relevant behavior, any such behavior with a non-party premise as its major referent is a potential challenge. What is routine behavior for an official in a non-revolutionary context becomes an illegitimate initiative for one in a revolutionary setting. The consequence is that in such a regime context one discovers that officials are reluctant to take the formal definitions of their positions seriously. In light of the structure of the regime and its political-ideological orientation, this is a quite rational stance to adopt. The net result of all of this is that in a third-world peasant society there are *societal* forces working against the integration of the status and role components of a position, and in a peasant society subject to the dictatorship of the proletariat there are additional *regime* forces working in the same direction. Both forces work against an effective appreciation of what role- and rule-based behavior implies and support a political culture that views politically relevant behavior

[44] *Scinteia*, September 15, 1971.
[45] Price, "The Social Basis of Administrative Behavior," pp. 72–86.

as dependent on permission by and familiarity with authoritative figures.

A final consequence of the regime's monopoly of the public domain is the premium that is often placed on withholding critical information from the purview of decision makers. Within system-building regimes a particularly large number of events are hushed up (*musamalizat*). It was argued earlier that intra-elite mutual protective behavior is partly a function of the Party's corporate group attitude of protecting "its own." However, mutual protection also occurs because the Party assumes responsibility for so much. Given the scope of the Party's responsibility, it is not surprising that cadres who were unaware of misdeeds in their sectors often work to prevent such deeds from becoming widely known since exposure will result in their own punishment. Such behavior could be documented throughout Eastern Europe and the Soviet Union by a random search of party newspapers. In his study of North Korea, Scalapino has found a classic example of this phenomenon. There it was brought to the attention of a provincial secretary that a "half-won business" was operating. He was unfamiliar with this term and it was explained that this referred to prostitution. The secretary and several other cadres carried out an investigation, identified the individuals involved and concluded, "This was a serious problem for us, because if we reported this matter to higher offices of the Party, we would all be subject to disciplinary treatment, including the *kun* Party secretary and myself, because we had allowed such a thing to develop. It was the season of the annual committee inspection and . . . we did everything to prevent them from reaching the hamlet . . . Later (we) called on those implicated . . . We could not implicate them in any case, however, or we would have been deeply involved."[46] Paradoxically, then, the Party's assumption of total responsibility for the public domain contributes in certain instances to collusion between elite and nonelite actors regarding illegal behavior, and to the withholding of important information since its release may result in negative sanctions against regime members. The consequence of all of this is the development of a regime political culture that is composed in part of dissimulation and complicity. Monopolization of the public domain, the fusing of elite and public spheres, and the corresponding denial of institutional integrity to public institutions (economic, administrative, social, and political) has in some ways reinforced the traditional political culture which viewed all relationships in terms of their hierarchical rather than their complementary character and which defined responsibility as the avoidance of public initiative.

[46] Scalapino, *Communism in Korea*, vol. 2, p. 777.

Mobilization and Political Culture. A second defining feature of all Marxist-Leninist regimes is their preoccupation with the rate of social development. Communist governments emphasize the fulfillment of potentiality. "Structurally, the political community is the means of translating potentiality into some sort of reality."[47]

A major feature of this mobilization ethos and the corresponding relationship between regime and society are the targets and priorities that are authoritatively established by the elite and that are used to gauge both the regime's and society's performance. Berliner has analyzed the character and consequences of this political structure in the Soviet regime's economic sectors. Berliner related the existence of extremely ambitious targets, tight time constraints, scarce resources, and authoritative sanctions ("the plan is law") to a series of informal-adaptive responses that included searching for a safety factor, hoarding, and dissimulation.[48] Though to date no study of this magnitude has been undertaken for other system-building regimes, one could in fact find similar informal-adaptive responses in most East European regimes.[49] What is most significant about Berliner's findings is that they can be generalized to other sectors of society.

In their effort to create an ethos of expanding good—an ethos that views resources and values as complementary and dynamic rather than mutually exclusive and static—mobilization regimes often unintentionally generate an understanding of resources and values based on "limited good." For example, just as in traditional peasant societies families jealously guard their own interests rather than cooperate extensively, so factory managers in a mobilization system are judged by their subordinates according to how successfully they defend their factory's interest *in opposition to* that of the ministry or trust. As one of Berliner's respondents commented: "We look to the director to promote our interests, to push through for us at the ministry and to defend the interests of the plant."[50] And just as a factory in such a system is always on the lookout for a safety factor and prone to hoard resources, so individuals in such a system tend to hoard "connections." Connections are as jealously guarded by individuals and

[47] David E. Apter, *The Politics of Modernization* (Chicago: University of Chicago Press, 1965), p. 31.

[48] Joseph S. Berliner, *Factory and Manager in the USSR* (Cambridge, Mass.: Harvard University Press, 1957), pp. 318–329 and *passim*.

[49] For a recent analysis which points out differences between the Romanian economic structure and the Soviet economic structure see David Granick, "The Orthodox Model of the Soviet-Type Firm Versus Romanian Experience," (International Development Research Center Working Paper, Indiana University, 1972).

[50] Berliner, p. 230.

families as material resources are by factories, and for similar reasons. Both the ethos and the structure of a mobilization regime are antagonistic to the routinization of tasks and the authority of impersonal-stable rule. Striving to insure that the potential of the society is completely realized, the regime defines targets, goals, and priorities substantively rather than procedurally, and rapidly redefines them. Thus a premium is placed on informal adaptive mechanisms (signified by the Russian term "blat"[51] and the Romanian term "pile" or pull) that allow for some stability and certainty in response to what is often perceived as an arbitrary and threatening regime. Both *blat* and *pile* refer to relationships and dispositions that obstruct the development of a political culture based on overt, public, cooperative, and rule-based relationships. Instead they reinforce the traditional community and regime political cultures with their stress on covert, personalized, hierarchical relationships involving complicity rather than public agreements.[52]

The ethos and structure of mobilization regimes influence the political culture of a society in a number of other ways. Along with the generation and reinforcement of a "limited good" perspective, mobilization regimes often unintentionally reinforce traditional conceptions of time. In an interesting book written in 1904, an insightful observer of Romanian society noted that Romanians at all levels of society worked in an "unregulated rhythm," and oscillated between periods of hard intense work and periods of prolonged inactivity. In Radulescu-Motru's words, "We [Romanians] are capable of any virtue so long as it doesn't require too great a persistence on our part."[53] In 1972, Nicolae Ceausescu, leader of a Marxist-Leninist regime that has been in power for twenty-five years, noted that policies initiated several years ago (at the National Party Conference in December 1967), "while leading to positive results are not being carried out consistently." He continued, "As a matter of fact, comrades, this [lack of measured persistence] is a habit of ours. We begin an action with a great deal of noise, obtain a series of results, but after a period of time we lose sight of segments of the policy . . . and we interest ourselves in other things."[54]

One could conclude that the changes in regime in Romania have been superficial and that although the form changes, the substance (i.e., cultural dispositions) remains. Such an argument would itself be superficial. The reality is more complex and intriguing as the character of the industrial production process in system-building regimes with a mobilization structure illustrates. A major characteristic of production in such a setting is the phenomenon of "storming," an uneven attention to tasks, varying intensity of job performance, and "campaign"-like resolution of tasks. These characteristics are a function of *the regime structure itself* and are highly congruent with traditional behavioral and attitudinal postures toward time and antithetical to modern notions of task performance based on scheduled —i.e., measured, continuous—behavior. In this instance one has an example of a regime concerned with and in some major respects succeeding in transforming the character of an entire social system and yet actually reinforcing certain cultural postures that are antithetical to its basic goal.

A third and highly significant consequence of the mobilization structure and ethos of a system-building regime is its connection with dissimulation as a modal behavioral posture in Marxist-Leninist societies. Berliner discovered that managers who wish to maintain their positions systematically engage in the practice of dissimulation. In Russian the term is *ochkovtiratelstvo* which literally refers to cheating at card playing by altering the character of a card. "In economic activity, it [ochkovtiratelstvo] signifies the simulation of a successful performance by some *deceptive manipulation*."[55] As with the case of hoarding, we should like to suggest that dissimulation is a *general* feature of a society and system based on a mobilization ethos and structure.

Several things can be said about dissimulation as a form of behavior. To begin with, it should be differentiated from formalism, which refers to strict adherence to external forms, usually to inadequate and inflexible understanding of a role's meaning. As Price has suggested, formalism is often fostered by the conflict between established societal norms and official or regime norms.[56] Dissimulation refers to something related but quite distinct. What is involved is not simply rigid adherence to rules in order to reduce uncertainty, but also "deceptive manipulation," the conscious adoption of false appearances. This posture does not arise as much from the conflict between estab-

[51] According to Berliner, *blat* "implies the use of personal influence for obtaining certain favors to which a firm or individual is not legally or formally entitled," p. 182.

[52] A number of phrases in contemporary Romanian parlance refer to different forms of complicity. Recently in *Scinteia* an article dealing with certain illegal practices in a factory found "un spirit de 'amabila," ingaduinto reciproca, de 'binevoltoare' toleranta, de 'delicata' inchidere a ochilor" (November 30, 1971). All of these phrases refer to semilegal or illegal covert cooperation.

[53] C. Radulescu-Motru, *Cultura Romana si Politicianismul* (Bucuresti: Cultura Nationala, 1904), p. 56.

[54] *Scinteia*, February 20, 1972, p. 2.

[55] Berliner, *Factory and Manager in the USSR*, p. 114.

[56] Price, "The Social Basis of Administrative Behavior," pp. 72–86.

lished traditional norms and newly initiated modern norms as from (a) the perceived need to deflect the regime's attention from possible or real under-fulfillment of tasks, and (b) from the general desire to minimize the scope of regime interference in one's private and social life. Dissimulation is based on fear and avoidance. It is an adaptive response to a regime that, in its concern for realizing the entire society's potential, periodically attempts to penetrate most areas within the society. As a political posture, dissimulation is hardly something foreign to the experience of the Romanian population, which in its history has had to develop adaptive-informal postures in order to maintain a private zone of unofficial, covert attitudes and practices while publicly complying with the coercive, extractive demands of alien rulers. The demands of a system-building regime have reinforced the Romanian population's tendency to separate sharply the public and private arena, and to perceive them in antagonistic rather than complementary terms. Dissimulation is the posture, response, and strategy that integrates the two arenas. In such a society one often finds a highly calculative and selective recognition of regime authority. This stance takes the form not so much of political opposition as a strong antipolitical privatism in which family and personal interests are emphasized at the expense of regime and societal interests.

Finally, system-building regimes with a mobilization ethos and structure are based on a "commanding heights" principle that entails the attempted *transformation* of certain areas, roles, and values and the attempted *control* of nonpriority areas, roles, and values. What differentiates a revolutionary Marxist-Leninist regime from a radical nationalist regime is *not* the Marxist-Leninist's attempt substantively and simultaneously to transform all areas of a social system but its determination (a) to control and/or transform critical points at *all levels* of the social system and (b) to prevent existing social forces from defining their resentment and hostility in terms of political opposition at any point or level of the society.

This pattern of transformation and control, of revolutionary *laissez faire*, contributes in the short run at least to a highly heterogeneous and often incompatible set of political cultures. One can distinguish at least three types of areas in system-building regimes: (a) poor rural areas experiencing more political control than cultural transformation. In such areas traditional-peasant political culture probably persists to a great extent. A classic illustration is the investigation of a Romanian cooperative in which a number of illegal practices were discovered. Asked about why such practices were not criticized in the assembly, one member noted that "here in this cooperative we are neighbors and relatives. There are too many relatives in the cooperative, and it isn't wise to get on the wrong side of them."[57] In nonpriority arenas, such as the rural, given the scarcity of their resources (i.e., time, cadres, goods) system-building regimes have emphasized political control more than cultural transformation. (b) Another area consists of modern-industrial projects in urban settings. The likelihood is that in these instances there has been a greater transformation of general and political culture reflecting the work-setting, nature of work, and the correlates of an industrial milieu. (c) There is also an intermediate area that combines modern forms and traditional practices.[58] The commercial and service sectors in system-building regimes provide a good deal of evidence for the argument that there is no simple demarcation between traditional and modern political cultures, between conceptions of authority based on hierarchical, personalized, and command considerations, and conceptions of authority based on complementary, rule, and leadership considerations. As the Romanian regime itself has noted the commercial and service sectors in urban as well as rural areas are major sites for the practice of *bakshish*. It is, in fact, noted that many individuals attempt to get jobs in these areas precisely because they are so lucrative. The incidence of corruption (or "behavior which deviates from the formal duties of a public role because of private-regarding [personal, close family, private clique] pecuniary or status gains . . . ")[59] in the service and commercial sectors provides another good example of regime and societal forces combining to produce behavior that is antithetical to official values. It is not accidental that the commercial and service realms are characterized by widespread corruption. In a traditional society services are not rendered on the basis of impersonal rules but on the basis of personal recognition; in the commercial and service realms (in contrast to the industrial realm) personal encounter is the basis of exchange and productivity. If, in addition one realizes that these are low priority realms for system-building regimes, it is not surprising that corruption is so pervasive within them. Actors within these realms, while complying with the externally imposed quotas or targets, concentrate on maximizing their personal, private interests much as cooperative farmers re-

[57] *Scinteia*, November 22, 1971.

[58] In this "area" one can find many instances of what Fred Riggs has termed "prismatic behavior." See *Administration in Developing Countries* (Boston: Houghton Mifflin Co., 1964).

[59] J. S. Nye, "Corruption and Political Development: A Cost-Benefit Analysis," *American Political Science Review*, 61 (June, 1967), 417–427, see p. 419.

late organizational tasks to their interest in their private plots. This propensity to deal instrumentally and calculatively with official responsibilities and ingeniously with personal interests characterizes many actors in all realms of the society but given the low-priority status of the commercial and service sectors and their personal-encounter character, this propensity is less constrained here and consequently flourishes.

Production-Mentality and Political Culture. The third and final aspect of a system-building regime to be considered here is its conception of tasks. We are particularly interested in the way in which system-building regimes approach the task of cultural transformation, a task that all Marxist-Leninist regimes assert as a major goal. System-building regimes tend to see the resolution of this task in derivative terms: it is assumed that as the consequence of constructing a new political, economic, and social system a new and congruent culture will emerge. Marxist-Leninist leaders often quote Lenin to the effect that "the transformation of all customs and practices . . . is a work of decades."[60] I am suggesting, however, something slightly more complex: the way in which certain Marxist-Leninist regimes deal with the task of cultural transformation is often contradictory and in part self-defeating. System-building regimes have tended to neglect the cultural realm in favor of other sectors of society and have been selective, often formalistic, in their efforts at transforming culture. The result has been an uneven and mixed developmental profile. The reasons behind this are varied and involve several elements: elements that are shared by certain elites (i.e., a Stalinist or neo-Stalinist conception and approach to the tasks of transformation and consolidation); elements specific to individual elites (i.e., their political identity-defining experiences); and elements specific to individual community cultures (i.e., the type of religious belief and organization that characterizes a given society). An analysis of the Gheorghiu-Dej regime in România will illustrate this argument.

Historically, formalism has been a noted and criticized component of the relationship between Romanian institutions and Romanian society. In observing a major Romanian institution, the Orthodox Church, Radulescu-Motru noted that it differed from Protestant churches in the West in being more oriented to ritual and formalistic practices than to practical moral and political lessons.[61] In a similar vein, Adolph Harnack noted that in Orthodoxy, "doctrine comes to be administered in stereotyped formulas accompanied by symbolic acts. . . . For ninety-nine per cent of these Christians, religion exists only as a ceremonious ritual, in which it is *externalised*."[62] At the community level, then, a major defining experience for the mass of the Romanian population involved uncritical adherence to a belief system that was manifested in terms of symbol and ritual and a private life that remained basically untouched by the formal-ritualistic components of that same belief system. This separation of religious ritual from private life characterized not only the mass of the population but also many of the Orthodox clergy.[63] Such a separation of spheres was not necessarily considered hypocritical, because the integrity of religious belief tended to be identified with the correct application of ritual. In short, ritual was regarded as both substance and form. It was out of this milieu that the faction headed by Gheorghiu-Dej came to power and established the dictatorship of the proletariat. Under Dej the party belief system was in some critical respects as dogmatic and externalized as Orthodoxy's belief system had been.[64] This tendency to externalize and ritualize the structure and ethos of the new regime was not simply a function of Dej's socialization in an Orthodox milieu. One must relate this socialization (Dej's as well as that of his followers and major sectors of Romanian society) to two other elements: the identity-defining experiences of the Romanian Communist Party (R. C. P.), and the adoption of a Stalinist approach to social transformation and political consolidation.

The Romanian party did not have a mass base or following before assuming power and was consequently highly concerned with identity questions. Its adoption of a very rigid interpretation of Marxism-Leninism was a response to its isolation during the interwar period from effective and sustained interaction with a mass following, insecurity over its "proletarian character" in the midst of a peasant society, and conflicts with parties whose ideology was primarily oriented to

[60] See for example D. Mazilu, "Socialismul—orinduirea cinstei si dreptatii," *Scinteia*, September 3, 1971.

[61] Radulescu-Motru, *Cultura Romana si Politicianismul*, p. 97.

[62] Adolf Harnack, "On Eastern and Western Christianity," in Talcott Parsons et al., *Theories of Society*, vol. 2 (New York: The Free Press, 1961), pp. 1114–1115 and *passim*.

[63] In novels such as Zaharia Stancu's *Barefoot* (New York: Twayne Publishers, Inc., 1971), one can find very insightful portrayals of the clergy's place in the community, and the community's perception of the clergy.

[64] To suggest that Dej was influenced by the Orthodox milieu of his society does not imply that he was religious or accepted the substantive tenets of Orthodoxy; only that certain formal attributes of Orthodoxy were important in shaping the way in which he approached and interpreted his own beliefs.

national culture and the peasantry. The Romanian leadership lacked the historical experience, political confidence, and consequently the ideological sophistication that enabled a party such as the Chinese to approach the basic social force in a traditional society—the peasantry—in a flexible rather than dogmatic, repressive fashion. As suggested, the third element behind the Romanian party's formal and dogmatic approach to cultural transformation was its adoption of a Stalinist strategy in dealing with the tasks of transformation and consolidation. This strategy consists of "walking on one leg," of stressing the organizational and instrumental components of development at the expense of a direct concern with multiple forms of political education and participation.[65] The interaction of these three elements—a community culture shaped by Orthodoxy, a party whose historic experience made it reluctant to engage rather than repress the peasantry and its culture, and a Stalinist conception of its political tasks—contributed to a dogmatic, ritualistic, and formalistic approach to the task of cultural transformation. A striking illustration of the uneven development that has resulted was discussed in the party paper, *Scinteia*. Under the heading, "Any work is clean, only laziness is dirty," the correspondents reported that several high school graduates were demanding office positions and refusing skilled-labor positions in a factory. Interviewing the family of one graduate, the correspondents quickly discovered that the father and mother had successfully socialized their son in a highly traditional fashion. Both they and their son were shocked to think that anyone who had been educated and who "spoke so beautifully" (*Ce frumos vorbeste!*) could be considered for a laborer's position.[66] This is not an isolated incident. The Romanian (and other) regimes are beginning to reap a harvest that is plentiful but unexpected and certainly undesired. While the regime has been quite successful in its attempts to increase the education and skill of many individuals, the corresponding and expected changes in cultural dispositions have not occurred as consistently or as widely as was hoped. Instead, traditional expectations of status defined as prestige have persisted in the face of formalistic demands that they disappear.

The restrictive understanding that characterizes a production-mentality approach to cultural transformation appears to complement the selective character of a "commanding heights" approach to social change. Both the commanding heights and the production-mentality orientations neglect critical elements in the units they are dealing with. The commanding heights or "revolutionary laissez faire" approach places a relatively low priority on the rural-agricultural areas and in certain respects the private arenas of society as well; the production-mentality approach fails to appreciate adequately or to come to terms with the expressive dimension of human behavior. The combination of a commanding heights mobilization effort and a production-mentality creates a milieu in societies and regimes such as the Romanian in which individuals are in effect rewarded for attending simply to their own interests both at work and in private settings. Within such a context those individuals who are *pusi pe capatuiala* (stimulated by the prospect of gain) and functionaries who *nu-si vad decit interesele lor* (don't see anything but their own interest) are reacting rationally and predictably to the regime's structure and ethos.

Through their organization and ethos, then, system-building regimes have stimulated a series of informal-adaptive responses—behavioral and attitudinal—that are in many respects consistent with and supportive of certain basic elements of the traditional political culture in these societies. These elements in turn are antithetical to the appearance of a regime and society with an ethos and structure predicated on a complementary relationship between the public and private realms,[67] on the viability of impersonal rules and norms, and on the value of egalitarianism expressed in the role of effective participant.

As the Romanian scholar Radulescu-Motru noted, there is a major difference between a social conglomerate and a viable nation-state. According to Motru, the basic condition for transforming such a conglomerate into a nation is the creation of a political formula that comes to terms with and reflects a conscious, sustained, and direct concern with a society's cultural dimensions.[68] Recently,

[65] The Chinese model of transformation and consolidation differs from the Stalinist precisely in giving lesser weight to the production mentality. Some scholars in effect combine the Leninist-Stalinist models and view Chinese development as a deviant case. I would view both Stalin and Mao as Leninists who have emphasized and partially revised the major elements of the Leninist political-ideological paradigm. As these revisions have drawn on different elements of the paradigm the resulting models do differ in significant respects. One can still argue, however (though for various reasons an argument of that order is not feasible at this point) that both models are in the political tradition of Lenin (just as Bendix and Parsons with all their real differences may be said to be in the intellectual tradition of Weber).

[66] *Scinteia*, September 29, 1971.

[67] I am currently working on a theoretical essay that approaches and interprets the nation-building process from the perspective of the emergence of a public domain that is distinct in character from the state (or official), social and private domains.

[68] C. Radulescu-Motru, *Romanismul* (Bucuresti: Cultura Nationala, 1936). For students of con-

many Marxist-Leninist regimes have begun to appreciate his general point. There is for example the critical observation made in *Scinteia* that not "all leadership elements, members of party committees [or] basic party bureaus have extended their observation to see how men are working, how they are living, what preoccupies them, how they think. We have been content with the fact that the plan is realized."[69] Political culture has become a salient issue for Marxist-Leninist regimes, including the Romanian. It is to the origins of this increased sensitivity, the type of responses offered by one regime, the Romanian, and the political significance of these responses that we shall now turn.

The Conflict and Resolution of Opposing Political Structure-Culture Sets

During the last decade most Marxist-Leninist system-building regimes have indicated their awareness of the growing complexity of their societies. The political significance of this complexity lies not so much in the noted "structural differentiation" that attends the developmental process as in the increase in the number of individuals and groups perceiving themselves as relatively unrecognized politically, economically, socially and ideologically by the regime. Twenty-five years of mobilization and industrialization have created a varied set of social groups with different defining features (partially antagonistic, partially complementary) and have confronted these regimes with the major pressing problem of political integration. The particular form this issue assumes and the particular response it elicits vary according to the leadership, international position, history, and composition of individual communist parties and according to the cultural and social character of each society. What is common to all such regimes, however, is the growing salience of the integration issue as these regimes begin to come to terms with the task of modernization and its imperative of regime-society complementarity rather than regime-society dichotomy.

Recognition of the Problem. In a speech delivered at a leading industrial center, the general secretary of the Romanian Communist Party pointed out to the assembled workers that, "As you can see, we have had an easy time constructing factories. But it is incumbent on us to transform man at the same rate, so that he will be capable of mastering new techniques . . . and new ways of thinking."[70] Taken together, Ceausescu's proposals of early July (1971), and his speech on November 3rd make it quite clear that culture, ideology, and education are now priority issues. There are two interesting and neglected aspects of this new focus. First, there is the continuing, if contradictory,[71] tendency to see the bourgeois past as the single source of a political culture that does not adequately approximate regime expectations. For example, in his November 3rd speech Ceausescu blamed the incidence of "parasitism" in Romania on the mentality generated by the exploiting classes, and he condemned the influence of bourgeois customs for the indifferent, calculating approach of Romanian workers towards socialist property.[72] This tendency to externalize the source of problems, a characteristic response of any organization concerned with minimizing the internal costs of structural and cultural redefinition, limits the regime's ability to see and act on the *multiple* and frequently *regime-generated* causes of such a political culture.

Second, there is the highly instrumental rationale behind the current appreciation of the cultural domain. Thus, in an authoritative article, Presidium member Gh. Pana[73] says that as the regime restructures its relations with Romanian society to allow for greater organizational recognition of various social elements, a cultural "revolution" is necessary to insure the correct interpretation of what this new relationship implies, the competent performance of complex tasks, and the harmonious integration of the regime's multiple goals. Two aspects of Pana's argument warrant attention: (a) culture has not become equal in status with industrialization and social change but has become increasingly significant at this point in time since it affects the regime's ability to enhance its capacity to direct the continued development of Romanian society and (b) at least in Ceausescu and Pana's view the cultural revolution is not considered a retrograde neo-Stalinist

temporary Africa and Latin America with their interest in neocolonialism, linkage-politics, and fragmented societies, and for students of nation building in general, Motru's work and the debate that took place in Romania in the latter part of the nineteenth and early twentieth century are of real comparative value.

[69] *Scinteia*, December 14, 1971.

[70] *Scinteia*, December 4, 1971.

[71] It is contradictory because on a number of occasions Ceausescu has explicitly argued that defects can no longer be blamed on the "bourgeois past," i.e., Nicolae Ceausescu, *Romania Pe Drumul Construirii Societatii Socialiste Multilateral Dezvoltate*, vol. 4 (Bucuresti: Editura Politica, 1970), p. 665.

[72] Nicolae Ceausescu, "Expunerea cu privire la Programul P.C.R. . . . ," *Romania Literara*, November 4, 1971, p. 6.

[73] Gheorghe Pana, "Conducerea politica de catre P.C.R. a intregii activitati de faurire a societatii socialiste multilateral dezvoltate," *Lupta de Clasa*, 12 (December 1971), pp. 3–15.

development but the condition for a new non-Stalinist relationship between regime and society.[74]

An Analytic Specification of the Conflict. Currently, the Romanian, along with other Communist parties, faces the politically complex task of reconciling two antagonistic constituencies; constituencies with a preference for opposing organizational formats and with different approaches to authority. More simply, it is a conflict between the Gheorghiu-Dej legacy and the reformist Ceauşescu posture. Between the years 1952 and 1964, the Dej regime was based on ritualistic, coercive, arbitrary, and personalistic principles. It is important to note that this kind of regime was not entirely unfamiliar or alien to the experience and social composition of Romanian society. In fact, one might hypothesize that the stability of Dej's regime was partially due to the congruence between the structure and ethos of his rule and the historic experience, social composition, and notion of authority held by large sectors of Romanian society.[75] One need not conclude from this hypothesis that the regime was highly legitimate.[76] One can, however, arrive at a more sophisticated understanding of the relationship that existed between regime and society. If indeed the formal organization of this regime generated a regime political culture that in many respects was congruent with and reinforced the traditional community political culture, then this very congruence contributed to the regime's stability during a period of comprehensive and intense societal dislocation.

The Dej regime, however, did not merely reinforce traditional political culture postures. Through its policies of mobilization and industrialization it created new social groups and corresponding political and cultural postures that in time challenged the very regime that produced them.[77] This dialectical process has been characteristic of all system-building regimes and has generated political crises in both Novotny's Czechoslovakia and Gomulka's Poland. As a consequence of their mobilization and industrialization policies, system-building regimes create strata of skilled workers, educated peasants, and professionals as well as urban-industrial and technological-scientific work settings.

These situational or societal developments accompany changes in the social composition of the Party and an increasing attention within the Party to the task of modernization. As these skilled and more articulate strata become more numerous and as the Party becomes increasingly concerned with modernization, the conflict between many of the current regime's defining features and the type of structure-culture set linked to the task and ethos of modernization becomes increasingly manifest. The conflict may be viewed symbolically as one between rules and patrons at the elite level, between contracts and *blat* (see footnote 51) at the regime level, and between merit and *pile* (connections) at the community level. The conflict is between those who wish to introduce modern—formal, empirical, and leadership—premises into regime-society relationships and those who wish to maintain or extend neo-traditional—substantive, dogmatic, and command—premises. It is a conflict between leaders capable of entertaining a wider range of solutions to problems and leaders with a more restrictive political mentality, between those who wish to maintain a regime-society relationship based on domination and avoidance and those who wish to establish a relationship based on manipulation and selective recognition. Finally, it is a conflict between those who favor a regime-society relationship that generates an "oblique" political culture: one stressing the dichotomization of public and private arenas and placing a premium on the private, covert presentation of demands (i.e., crypto-politics),[78] and those who

[74] Of course there are some Stalinists in the party who do see the "cultural revolution" in Stalinist terms. More seriously, given the lack of an alternative political tradition in the party, any move away from the recent emphasis on modernization may necessarily and somewhat unintentionally assume a neo-Stalinist cast.

[75] For the analytic statement that this argument is based on see the excellent piece by Harry Eckstein in his *Division and Cohesion in Democracy* (Princeton: Princeton University Press, 1966), pp. 225–289.

[76] In fact for many sectors of Romanian society, and one may argue for many other societies ruled by Marxist-Leninist parties, the question of legitimacy defined as politically conscious consent was not a major question at all. Defined in this way legitimacy presupposes the existence of a participatory constituency and tradition. The situation was quite different in peasant, patriarchal, and authoritarian Romania. A situation of this order does not remove the issue of regime-society relations; but it should sensitize the analyst to the task of specifying the dimensions of such in light of a specific historic reality.

[77] At another point in his recent work, *Polyarchy*, Dahl notes correctly that political culture studies have focused on "stable and persistent outlooks . . ." and that "investigation of political cultures may easily neglect sources of change . . ." pp. 166–167. In this article we are explicitly concerned with conscious and partially successful efforts at creating a new political culture with the resulting conflict between cultures and the attempts to resolve this conflict.

[78] An illustration of this "oblique culture" is found in an article in *Scînteia* (November 14, 1971). While being interviewed the director of a plant "was interrupted by an associate who tried to whisper something in his ear." "Speak clearly" the director said. But the associate attempted to find a favorable angle to whisper in the director's ear. The director responded, "What you say is of no interest, the law must be applied." The correspondent asked what all

favor a political culture stressing the complementary character of private and public arenas and recognizing the usefulness of a framework that allows for something more than the private presentation of demands and something less than the public presentation of demands. What is involved then is the ethos and structure of the Romanian regime.

A Political Resolution of the Conflict. The leadership of the R.C.P., specifically, Nicolae Ceausescu, is faced with the task of defining his regime in terms that are at least partially acceptable to those (including himself) whose defining experiences, political perspectives, and operating styles are to varying degrees the product of the Gheorghiu-Dej period: those whose political characters combine traditional peasant and Stalinist political postures. Simultaneously, Ceausescu and his supporters are aware that in twenty-five years the regime has created social groups whose aspirations for recognition require a political response, whose skills require a restructuring of power relations, and whose understanding of authority allows for and requires new institutional formats and a corresponding political-cultural ethos.

The leadership must come to terms with a party and a society in some ways more homogeneous than before 1944 but in others more heterogeneous.[79] Within the Party itself, the number of intellectuals has increased from 11 per cent (93,000) in 1960 to 23 per cent (437,000) in 1970, while the percentage of workers has decreased from 51 per cent to 46.5 per cent in 1972. In addition, it is very likely that an increasing percentage of these workers are recruited from the more skilled stratum.[80]

As for the society, the percentage of peasants has decreased at least 20 per cent in the last twenty years. More significant is the increase in the number of educated, skilled, and professional individuals. In a population of some twenty million the number of nonmanual workers increased by almost 400,000 (from approximately 900,000 to 1.3 million) between 1959 and 1969. In contrast to 1959, when approximately half of these had received higher or middle-level education, in 1969 close to three-quarters had done so. Furthermore, the rate at which such individuals are being educated and trained is increasing. In the eight year period 1960–1968, a greater number of individuals attended advanced study courses than during the period 1945–1959. To return to the Party, both at the center and in the provinces there is a noticeable tendency under Ceausescu to place men within the age group forty to forty-five in leading positions, and at the time of the Tenth Congress in 1969, within the whole Romanian party, 60 per cent of the membership was under forty years of age.[81] While the data are insufficient in this area, there are some grounds for assuming that the leading cadres are better educated than their older comrades and differ importantly (although not completely) in their understanding of authority relations.

These data are provided to illustrate two points. Within Romanian society a relatively new, urban, professional, skilled, educated stratum is growing at a significant rate. This development is not occurring within a vacuum, but rather affects and is affected by a society that is still more than half

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of that was about. The director responded, "*Pile*, illegitimate pressure, to hire someone without regard to their merit." In a very real way the conflict of cultures in Romania today is between the "oblique" presentation of demands and the overt or clear presentation of demands, between those who view the public realm fearfully and try to find "the right angle" to approach it from and those who *spune tare* (speak clearly) who want the public domain to develop a new type of integrity. In connection with the covert presentation of demands see the by now classic statement on crypto-politics in Soviet-type regimes by T. H. Rigby, "Crypto-Politics," in *Survey* 50 (January, 1964), 183–194.

[79] The Romanian party is quite attuned to the issue of social homogeneity-heterogeneity. See for example the discussion in *Lupta de Clasa*, 7 (July 1969), 55–79.

[80] In connection with the social composition of the Soviet and East European parties see the very valuable contributions by Frank Parkin, "Class Stratification in Socialist Societies," *British Journal of Sociology*, 20 (December 1969), 355–374; Frank Parkin, "Yugoslavia," in *Contemporary Europe: Class, Status and Power*, ed., Margaret Scotford Archer and Salvador Giner (New York: St. Martin Press, 1972), pp. 297–318; Mervyn Matthews, *Class and Society in Soviet Russia* (New York: Walker & Co., 1972), pp. 213–255; George Kolankiewicz, "The Working Class," in *Social Groups in Polish Society*, ed. David Lane and George Kolankiewicz (London: The Macmillan Press, Ltd., 1973), pp. 88–152; Peter Ludz, *The Changing Party Elite in East Germany* (Cambridge, Mass.: The M.I.T. Press, 1973), pp. 187–325.

[81] See respectively, Gheorghe Gheorghiu-Dej, *Articles and Speeches:* June 1960–December 1962 (Bucharest: Meridiane Publishing House, 1963), p. 66 and Gheorghe Pana, "Noua structura sociala a Romaniei ,si cresterea rolului conducator al partidului comunist," *Lupta de Clasa*, 2 (February, 1971), 9; *Ibid.*, p. 8; *Anuarul Statistic al Republicii Socialiste Romania 1970* (Bucuresti: Directia Generala de Statistica, 1970), Table 46, p. 122; *Anuarul Statistic Al* R.P.R. (Bucuresti: Directia Generala de Statistica, 1960), Tables 42, 43, 50, 51, pp. 114–115, 122–123; *Anuarul Statistic al Republicii Socialiste Romania 1970* (Bucuresti: Directia Generala de Statistica, 1970), Tables 56, 58, 59, pp. 136–137; *Breviarul Statistic Al Republicii Socialiste Romania 1971* (Bucuresti: Directia Generala de Statistica, 1971), Table 32, pp. 76–77; Virgil Trofin, "Raport Cu Privire La Modificarile Ce Se Propun A Fi Aduse Statutului Partidului Comunist Roman," *Congresul Al X-lea al Partidului Communist Roman* (Bucuresti: Editura Politica, 1969), p. 128.

peasant, a party composed of a good number of old and young cadres with a Stalinist set of experiences and dispositions, and a political community lacking any recent or past experiences with institutions based on effective legal-rational norms. It is hardly an exaggeration, then, to argue that this regime and its leader are faced with an extremely complicated task in responding to distinct and often antagonistic constituencies.[82] What has been emerging (through 1972 at least) is a regime with a *patriarchal-executive* character, one based on what Eckstein has termed balanced disparities.[83] This regime is attempting to manage constituencies whose preferences concerning institutional formats and authority relationships are, if not mutually exclusive, at least highly incongruent and often incompatible. One may look at this regime in terms of Gerschenkron's concept of historical substitutions;[84] namely, as a response to the universal imperatives of modernization (rather than as in Gerschenkron's formulation, industrialization) that reflects the historical legacy of Romanian society, the political character of the Romanian party, and the social, economic, and cultural definition of contemporary Romanian society.

The patriarchal-executive regime is oriented to three goals: authoritarian leadership, welfare, and efficiency. The Plan in Romania is an important example of the merging of modern, Leninist, and traditional premises in a far from harmonious but thus far (1972) workable synthesis. Certainly, one cannot interpret the Party's apparently absolute commitment to the Plan as simply an inevitable Leninist stance; the Hungarian and Yugoslav attitudes contradict such a notion. Rather the intense commitment to the Plan reflects three partially compatible and partially incompatible factors: (a) the Leninist commitment to directed mobilization, (b) the Party's "patriarchal" concern (*grija*) for the population's well-being (*bunastarii*), and (c) the Party's increasing appreciation of managerial and technical expertise.

As a mode of leadership, patriarchalism has historically had several distinguishing features including a stress on orderly rather than charismatic (i.e., highly random and arbitrary) management, direct concern with the economic welfare of the social unit involved, personalized attention to solidarity issues, and emphasis on authoritarian and disciplinary behavior.[85] Defined in these terms a "patriarchal" stance by the Party is consistent with its assumption of authoritarian responsibility for the overall development of Romanian society. It is also compatible with the Party's effort to respond to social dislocation and cleavage in a society that has experienced a prolonged period of mobilization-industrialization and that has no significant experience with institutions based on impersonal instrumental norms. Finally, to the extent that a "patriarchal" stance (in contrast to the "patrimonial" or "sultanist" structure under Gheorghiu-Dej) decreases the incidence of arbitrary behavior, it is compatible with a modern or "executive" structure. Similarly, a modern "executive" structure is compatible with a "patriarchal" stance insofar as executive organizations are authoritarian organizations.

The Romanian leadership is in the process, then, of elaborating a structure and ethos designed to elicit the approval of quite different constituencies within as well as outside the Party. The Ceausescu leadership has attempted to build into the regime a set of balanced disparities that are congruent with the different expectations and orientations of quite different groups of party members and social strata.

A good example of this blending or amalgamating process is the way in which Ceausescu's role is defined. Ceausescu has defined the leadership role as entailing respect for the corporate identity and integrity of the Party as an institution, adherence to Party rules, and enhancement of procedure in the workings of the party organization. He is portrayed as self-disciplined, hard working, tolerant of debate and expert opinion; yet as a leader in touch with the people. But he is also the concerned father of his people visiting the market places; Ceausescu, the proposed authoritative model for the Romanian people presented as a latter day Fat-Frumos, the mythical youthful hero of the Romanians;[86] Ceausescu, the focus of what is often referred to as a personality cult. This

[82] Ceausescu himself may be viewed as an amalgam leader, insofar as he not only represents and leads distinct constituencies that are themselves characterized by conflict-accommodative patterns, but also reflects in his own political personality these partially compatible, partially conflicting tendencies.

[83] Eckstein, *Division and Cohesion in Democracy*, pp. 262–269.

[84] Alexander Gerschenkron, "Economic Backwardness in Historical Perspective," in *Economic Backwardness in Historical Perspective: A Book of Essays*, ed. Alexander Gerschenkron (Cambridge, Mass.: Harvard University Press, 1962), pp. 5–31.

[85] On patriarchalism see Max Weber, *Economy and Society*, ed. Guenther Roth and Claus Wittich (New York: Bedminster Press, 1968), pp. 231, 240, 645, 1006–1010, 1112, 1118; Vera St. Erlich, *Family in Transition* (Princeton: Princeton University Press, 1966), pp. 31–60.

[86] Pavel Apostol has written an excellent article dealing with Fat Frumos titled, "Fat-Frumos si tineretea ca dimensiune spirituala," in *Trei Meditatii Asupra Culturii* (Cluj: Editura Dacia, 1970), pp. 19–51. While explicit parallels with Ceausescu are not drawn they are not hard to find. Ceausescu himself in some speeches has identified himself with the youth, while certain of his associates have pretented him as a "model" for Romanians to follow.

amalgamated image appears designed to elicit approval from diverse constituencies: on the one hand, the peasant, the apparatchik, and the "new class," and on the other, the professional, and the managerial-type party cadre.

The political task of eliciting and managing such approval, however, from what are basically antagonistic constituencies with different operating styles is by no means easy. In the course of the last seven years the Party has adopted two distinct strategies for balancing constituencies with different cultural postures and structural preferences. The first strategy was compartmentalism. The strategy consisted of isolating opposing elements from one another by parceling out areas that were in effect equivalent to protected fiefs. To call this pluralism would not be totally incorrect, but it would be more misleading than useful because of the incidence of conscious elite manipulation and the absence of a public arena. For these reasons I prefer the term authoritarian compartmentalism.[87] This strategy has quite recently been supplemented and largely replaced by one that places a greater emphasis on combining representatives with competing orientations in the same institutions. We mention these strategies in order to highlight the crucial fact that the emergence of a patriarchal-executive regime is not an ethereal process. Ceausescu's success will partially depend on the strategy (i.e., authoritarian compartmentalism or fusion) he chooses, and that strategy will in turn reflect the opposition he encounters and his own ambivalence toward cultural and structural change. Although he has given many indications that he is sensitive to the importance of culture, diverse constituencies, and the imperatives associated with modernizing, there is no guarantee that he will successfully provide for the consequences of his various policies. Nor is there any guarantee that he can continue to bridge the gap between his two constituencies within and outside the Party. In short, I am not at all suggesting that Ceausescu and his supporters have developed a perfect regime-amalgam or synthesis. The serious challenge he received in the summer and fall of 1971 and the resulting changes in policy suggest that Ceausescu is still the manager and leader of a coalition (not as yet the "patrimonial leader" of a political retinue as Dej was) and that this coalition is composed of elements that are capable of conflict as well as accommodation.

One might hypothesize that as modernization progresses, the choices before the Romanian Party may become more mutually exclusive, in which case the current patriarchal-executive amalgam may fail.[88] Noting all of this does not, however, minimize or underrate the significance and innovativeness of the current Romanian leadership's attempts to reform the existing character of the nation's political structure and ethos. The patriarchal-executive regime embodies the Romanian attempt to move with control and stability from an established neotraditional/Stalinist political society to a modern Leninist political society.[89]

[87] This article was written before I came across two recent articles that are of major significance in Communist studies. One is that by Jerry F. Hough, whose notion of institutional pluralism is very congruent with what I am terming authoritarian compartmentalism (see "The Soviet System: Petrification or Pluralism?" *Problems of Communism*, 21 (March-April, 1972), 25–46, and the already cited article by Zygmunt Bauman.

[88] In the period since this article was written one striking development has occurred within Romania. Mobilization has become the defining emphasis. At the leadership level this is seen in the increasingly patrimonial character of Ceausescu's position. With respect to the Weberian categories that have served as the basis for my characterization of the Romanian regime between 1970 and 1972, the regime in the last year and a half (this is being written in August 1973) has moved from an emphasis on neo-traditional (patriarchal) and legal-relational (executive) considerations to a much greater emphasis on the charismatic qualities of the party.

[89] I have attempted to deal with some of the questions connected with the compatibility of Leninism and modernity in an article titled "Inclusion and Mobilization in Marxist-Leninist Systems," which will be part of a volume edited by Mark Field titled *The Social Consequences of Modernization in Socialist Countries.*

Foreign Policy and Empirical Democratic Theory*

DAVID W. MOORE
University of New Hampshire

The studies of political and economic determinants of policy outcomes in the American states have been extensive, and the findings have generally been cumulative. V. O. Key's[1] original hypothesis about the importance of interparty competition on policy outcomes was subsequently supported and elaborated by Lockard.[2] The first serious challenge to Key's theory was presented by Dawson and Robinson,[3] who expanded the concern with policy determinants to include a wider range of political and economic variables. Subsequent studies by Jacob,[4] Dye,[5] and Hofferbert,[6] among others, also questioned the importance of political determinants, suggesting instead the greater importance of economic variables as correlates of policy outcomes. A comprehensive study by Dye[7] of the American states provided additional evidence that "differences in the policy choices of states with different types of political systems turn out to be largely a product of differing socioeconomic levels rather than a direct product of political variables." In reviewing this effort, Robinson was able to conclude that "thanks to Dye's extensive efforts, I believe judgment [about the relative importance of politics and economics to policy outcomes] may now be rendered with persuasiveness if not conclusiveness."[8]

But the challenge to the challengers was almost immediate and was provided in part by the challengers themselves. Dye[9] noted the greater importance of political variables in accounting for the distribution of wealth among the populations in the states, a point later elaborated and supported by Fry and Winters.[10] Sharkansky and Hofferbert,[11] Cnudde and McCrone,[12] and Clarke,[13] among others, concluded that for certain policies, particularly those that do not depend primarily upon monetary resources for implementation, political characteristics of state systems are more important than the state's economic wealth. Thus, one important hypothesis drawn from this most recent research is that political variables *are* important determinants of policy outcomes, but only (or at least primarily) when those outcomes are essentially political rather than economic.

A second hypothesis drawn from this research was offered by Cnudde and Neubauer,[14] who point out that despite some variation in electoral competition, all of the American states are basically democratic. Among democratic polities, the authors hypothesize, variation in democracy is slight and thus may not be as important as environmental or socioeconomic influences, while in undemocratic nations these latter influences will decrease, and variations in political characteristics will be more important. To test this hypothesis, cross-national studies would have to be conducted.

A third hypothesis is offered by Cutright,[15] who

* I am grateful to John R. Kayser and Michael K. O'Leary for comments on an earlier draft.

[1] V. O. Key, Jr., *Southern Politics* (New York: Vintage Books, 1949).

[2] Duane Lockard, *New England State Politics* (Princeton, New Jersey; Princeton University Press, 1959).

[3] Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables and Welfare Policies in American States," *Journal of Politics*, 25 (May, 1963), 265–89.

[4] H. Jacob, "The Consequences of Malapportionment: A Note of Caution," *Social Forces*, 43 (December, 1964), 256–61.

[5] Thomas R. Dye, "Malapportionment and Public Policy in the States," *Journal of Politics*, 27 (August, 1965), 586–601.

[6] R. I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," *American Political Science Review*, 60 (March, 1966), 73–82.

[7] Thomas R. Dye, *Politics, Economics, and the Public: Policy Outcomes in the American States* (Chicago: Rand, McNally, 1966).

[8] James A. Robinson, "A Review of Dye, *Politics, Economics, and the Public*," in the *American Political Science Review*, 61 (December, 1967), 113–14.

[9] Thomas R. Dye, "Income Inequality and American State Politics," *American Political Science Review*, 63 (March, 1970), 157–62.

[10] Brian R. Fry and Richard F. Winters, "The Politics of Redistribution," *American Political Science Review*, 64 (June, 1970), 508–22.

[11] I. Sharkansky and R. I. Hofferbert, "Dimensions of State Politics, Economics, and Public Policy," *American Political Science Review*, 63 (September, 1969), 867–79.

[12] Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review*, 63 (September, 1969), 858–66.

[13] James W. Clarke, "Environment, Process and Policy: A Reconsideration," *American Political Science Review*, 63 (December, 1969), 1172–82.

[14] Charles F. Cnudde and Deane E. Neubauer, "New Trends in Democratic Theory," in *Empirical Democratic Theory*, ed. Cnudde and Neubauer (Chicago: Markham, 1969).

[15] Phillips Cutright, "Inequality: A Cross-National Analysis," *American Sociological Review*, 32 (August, 1967), 562–78; and "Political Structure, Economic Development and National Social Security Programs," *American Journal of Sociology*, 70 (March, 1965), 537–51.

extended this type of research to the national level. Although he did not specifically test the Cnudde and Neubauer hypothesis mentioned above, he did study policies cross-nationally and discovered that the political representativeness of a nation is indeed an important determinant of policy. The importance, however, seemed to depend on whether the nations were developed or undeveloped. Among underdeveloped nations, he found that social insurance programs had yet to be adopted even though the nations showed considerable variation in representativeness. But among the highly developed nations, he found a positive relationship between governmental representativeness and social security coverage. This difference between developed and undeveloped nations was attributed to the "technological and bureaucratic prerequisites" needed to implement the social insurance programs, prerequisites which only the developed nations possess. The undeveloped nations, no matter how politically representative their national government, do not have the resources to adopt such programs. To generalize from this case, the third hypothesis can be stated as follows: the importance of governmental representativeness to policy outcomes is higher in developed than undeveloped nations.

The following is a restatement of the three hypotheses just mentioned, all drawn from the recent literature analyzing the relationship between democracy and policy outcomes:

(1) Political variables describing the extent of a government's democratic processes (e.g., party competition) *are* important determinants of some policy outcomes, primarily those outcomes which are political rather than economic.

(2) Among democratic polities, socioeconomic characteristics are more important than governmental characteristics in accounting for policy outcomes, while the opposite holds among undemocratic polities.

(3) The importance of governmental representativeness to policy outcomes is higher in developed than in undeveloped nations.

One of the notable characteristics of all of this research is its focus on domestic policy to the exclusion of foreign policy. Most of the research is even more narrowly focused on American state governments, but even Cutright's cross-national studies deal only with domestic policy. Yet, as the discussion above demonstrates, the types of policies subject to analysis have been increasing, and it seems only logical to extend the analysis from domestic to foreign policies.

Foreign Policy and National Attributes

All of the data and analyses reported here are based on a cross-sectional study of 109 nations for the year 1963. Nine independent measures of governmental and societal attributes, all statistically independent of each other, were defined by factor analyzing 64 governmental and societal attributes of nations.[16] The nine factors that emerged from the analysis are shown in Table 1, along with the highest loading variables on each

Table 1. Measures of Governmental and Societal Variables

1. Economic Development
   a. GNP per capita (.93)[a]
   b. Energy consumption per capita (.91)
   c. GDP per capita (.89)
   d. National income per capita (.88)
2. Size
   a. Total population (.97)
   b. Total GNP (.85)
   c. Total GDP (.79)
   d. Total national income (.78)
   e. Total energy consumption (.72)
3. Accountability
   a. Status of legislature (.82)
   b. Electoral system (.82)
   c. Horizontal power distribution (.78)
   d. Freedom of press (.77)
   e. System style (.76)
   f. Representative character of current regime (.73)
4. Governmental Stability
   a. Governmental stability (.75)
   b. Communist party (.64)
   c. Stability of party system (.64)
5. Christian Culture
   a. Per cent Christian (.88)
   b. Per cent Moslem (−.78)
   c. Per cent Roman Catholic (.68)
6. Elite Tenure
   a. Elite tenure (based on last two officials) (.84)
   b. Elite tenure (based on last ten years) (.80)
7. Population Density
   a. Population density (.81)
   b. Total land (.68)
8. Urbanism
   a. Population in cities over 20,000 (.55)
   b. No. of workers in agriculture as per cent of economically active population (−.49)
   c. Per cent GNP from Agric. (−.44)
9. Institutionalization
   a. Interest aggregation by the executive (.69)
   b. Period of national independence (−.61)

[a] Numbers in parentheses refer to each variable's loading on the respective factors. Thus, GNP per capita enjoyed a .93 loading on (or correlation with) the Economic Development factor.

[16] The data used here were provided by the Comparative Analysis of Policy Environments (CAPE) Project under the direction of Philip M. Burgess, The Ohio State University. A report on the factor analysis is included in my "Governmental and Societal Influences on Foreign Policy in Open and Closed Nations," in *Comparing Foreign Policies: Theories, Findings, and Methods*, ed. James N. Rosenau (Sage, 1974).

Table 2. List of Foreign Policy Measures

1. Cold War 2. Intervention in Africa 3. Supranationalism	The first three variables represent the three voting dimensions of the United Nations General Assembly, 1963. Data were all supplied by Bruce M. Russett.[19]
4. Alignment	A measure of international alignment based on the per cent of votes in the United Nations for the U.S. on issues which found the U.S. and the U.S.S.R. opposed. Correlates .68 with the Cold War issue above.
5. Defense Expenditures as a Percentage of GNP	
6. Conflict	A measure of verbal conflict based on the number of demands, protests, and accusations made by each nation. Data provided by R. J. Rummel.[20]
7. Participation	A measure of involvement in international relations based on the number of ambassadors sent to foreign countries.

factor. Seven foreign policy dimensions were also defined and are listed in Table 2. Except for Alignment and Cold War, the foreign policy measures are only moderately or slightly intercorrelated,[17] thus tapping a variety of foreign policy dimensions.

In a previous study,[18] each foreign policy measure was regressed against all nine independent variables to examine the relative importance of the political representativeness or accountability of a nation on its foreign policy actions (see Hypothesis #1). The Accountability variable proved to be the most important of all nine variables, as indicated by the size of its partial correlation coefficient, in accounting for the measures of Cold War, Supranationalism, Alignment, and Defense Expenditures. For Conflict, the correlation coefficient was statistically significant, but smaller than the Size correlation coefficient; for Participation, the coefficient was significant but only fifth in importance; and for Intervention in Africa, the correlation coefficient was not significant. Overall, Accountability had higher partial correlation coefficients with the foreign policy measures than did any of the other independent variables, including Size and Economic Development. Thus, it is clear that on the "economic" issue of Defense Expenditures as well as the "political" issues of Cold War, Supranationalism, and Alignment, the degree of a country's political accountability did exert significant impact even when other important variables, such as Size and Economic Development, were controlled.

In a related study,[21] the differences in determinants of foreign policy between democratic and undemocratic (open and closed) nations were examined. Both Cnudde and Neubauer[22] in discussing domestic policies, and Rosenau[23] in discussing foreign policies, hypothesized that in the democratic nations, environmental or societal variables are more important than governmental variables in determining policy, while the opposite is true in undemocratic nations (see Hypothesis #2). To examine this notion, all nations were classified either as "open" or "closed," and each foreign policy measure was regressed against the five societal and four governmental variables. The relative weight of each cluster was then assessed by adding partial beta coefficients. The results were less clear than those of the previous analysis, but on five of the seven issues, Intervention in Africa, Supranationalism, Alignment, Defense Expenditures, and Conflict, the proportional importance of governmental determinants turned out as predicted to be greater in closed than open nations. On the Cold War issue no differences emerged; on the Participation measure the differences between open and closed nations were opposite to those predicted, although the magnitude of the differences was smaller than that on any of the five issues that fit the predictions. Thus, overall, some support is given to the importance

[17] Of the 21 intercorrelations among the seven foreign policy measures, 13 are less than .30; only Cold War and Alignment correlate more than .50.

[18] Moore, "National Attributes and Nation Typologies: A Look at the Rosenau Genotypes," in *Comparing Foreign Policies.*

[19] For a report of these dimensions, see Bruce M. Russett, *International Regions and the International System* (Chicago: Rand McNally, 1967).

[20] See R. J. Rummel, "Some Dimensions in the Foreign Behavior of Nations," *Journal of Peace Research,* No. 3 (1966), 201–23.

[21] Moore, "Governmental and Societal Influences on Foreign Policy in Open and Closed Nations," in *Comparing Foreign Policies.*

[22] Cnudde and Neubauer, *Empirical Democratic Theory,* 511–34.

[23] James N. Rosenau, "Pre-Theories and Theories of Foreign Policy," in *Approaches to Comparative and International Politics,* ed. R. Barry Farrel (Evanston: Northwestern University Press, 1966).

in policy analysis of the distinction between democratic and undemocratic polities hypothesized by Cnudde and Neubauer and by Rosenau. The determinants of policy do seem to act in different ways in each of the polity types, thus suggesting different models for each of the two types.

A third analysis of foreign policy data deals with the relationship noted by Cutright between democracy and economic development (see Hypothesis #3). Cutright's study of national social insurance programs, it should be recalled, revealed that only developed nations had the technological and bureaucratic resources to respond to the demands of the population for such programs. Thus, the hypothesis inferred from this analysis is that political representativeness is more important for policy outcomes in developed nations than in undeveloped nations.

To examine the applicability of this hypothesis to foreign policy issues, all nations were classified as either developed or undeveloped,[24] and for both groups each foreign policy measure was regressed against the nine independent variables. If Cutright's observations are applicable to the foreign policy measures, the partial correlations of the Accountability variable with each foreign policy measure should ideally be strong among the developed nations and weak or nonexistent among undeveloped nations. In a less ideal case, the partial correlation coefficients should at least be larger among the developed than the undeveloped nations.

As Table 3 reveals, on every foreign policy measure, Accountability is more important in developed than undeveloped nations. On three of the seven issues, Accountability is statistically significant even in undeveloped nations; and on three other issues, statistical significance is not achieved even in developed nations. But tests of significance are at best only suggestive of substantive importance, and in this particular analysis the focus is on the overall pattern of coefficients in developed and undeveloped nations. The probability of finding the pattern described here, in which the correlation coefficients are larger in one group than the other for all seven cases, is less than one in a hundred.

Table 3. Relative Importance of Political Accountability in Developed and Undeveloped Nations

Foreign Policy Measure	Partial Correlation Between Accountability and Foreign Policy Measure	
	Developed Nations	Undeveloped Nations
Alignment	.96*	.50*
Cold War	−.89*	−.46*
Supranationalism	.91*	−.12
Defense Expenditures	−.47*	−.30*
Africa Intervention	−.34	−.05
Conflict	−.30	.05
Participation	−.24	−.05

* Statistically significant at the .05 level using a "t"-test.

Summary and Conclusion

In this comparison of the studies of American state domestic politics with those of national foreign policies, at least two methodological problems should be mentioned. First, the variables of democracy, governmental representativeness, and accountability, while conceptually similar, are all measured in different ways for different studies. Thus some doubt may exist about the validity of comparing findings from these different studies when one of the central concepts—democracy—is not measured identically in each study. In the studies cited in this paper, however, most of the findings tend to reinforce each other in spite of the different ways of measuring democracy. This convergence of findings in the face of methodological differences should, in fact, be reassuring because it demonstrates that the findings are not dependent upon just one measurement.

A more serious problem is the conceptualization of issue areas, particularly in the foreign policy analysis. In this study, seven issues were analyzed, only two of which are highly intercorrelated. There is no assurance, however, that those seven issues are the most important ones, nor that some very important issues have not been excluded. So far, little agreement has been reached among scholars about the most useful way to typologize issues, and the few quantitative attempts that have been made are too narrowly focused upon one kind of data.[25] Furthermore,

[24] For the method used to classify nations as developed or undeveloped, see Philip M. Burgess, "Nation Typing for Foreign Policy Analysis: A Partitioning Procedure for Constructing Typologies," in *Methodological Concerns in International Studies*, ed. Edwin H. Fedder (St. Louis: University of Missouri, Center for International Studies Monograph Series, 1970), pp. 3–66. Of the 109 nations included in this study, 42 are classified developed, 67 undeveloped.

[25] For a review of this literature and a creative attempt at devising a typology of foreign policy outputs, although based solely on WEIS events data, see Charles W. Kegley, Jr. *A General Empirical Typology of Foreign Policy Behavior*, Sage Professional Papers in International Studies (Beverly Hills: Sage, 1973). For a description of WEIS events data, see Charles A. McClelland, "The World Event/Interaction Survey: A Research Project on Theory and Measurement of International Interac-

these data are much more highly aggregated and less obviously policy-specific than the domestic policy studies cited in this paper.

In spite of these limitations, the comparison of the hypotheses generated by the studies of domestic policy with the results of the foreign policy analyses reveals many similarities. While the distinction between economic and political outcomes suggested by the first hypothesis does not seem to hold for the foreign policy measures, the importance of political accountability on some foreign policy outcomes is emphatically demonstrated. Furthermore, the second hypothesis, suggested by Cnudde and Neubauer from the results of American state studies, also receives support from the foreign policy analysis. Governmental influences on most of the foreign policy measures are proportionately greater in undemocratic than democratic nations. And the third hypothesis, drawn from Cutright's analysis of social insurance programs, was clearly supported by the analysis of foreign policy issues.

While the similarities are encouraging, many deviations from the general trend need to be explained. More important, some refinements of the underlying democratic theory may also be needed. Cutright's hypothesis, for example, was based on the assumption that more technological and bureaucratic resources are available for the democratic nations to respond to the demands of the people. Yet it is not at all clear that for the foreign policy issues analyzed in this study, a nation requires *more* resources in order to respond to the people's demands. In fact, as one example, the more representative a nation, the *lower* its defense expenditures. Furthermore, the underlying democratic theory is based on the notion that the more representative nations allow wider participation by the general public, and the general public in turn pressures its governmental representatives to adopt specific kinds of proposals. As Cnudde and Neubauer point out,[26]

> . . . democracy, however defined, includes the sharing of power or influence by wider sections of society than nondemocratic alternatives. Therefore, by widening the degree of representation, democracy in practice results in a greater tendency for the enactment of preferences of the lower social strata.

Implicit in this formulation is the assumption that members of the lower social strata will act to have government adopt policies favoring themselves, such as programs in health, education, and welfare. But it is not as easy to assume pressure from the lower social strata for specific kinds of foreign policies. In fact, research indicates that the general public is usually ill-informed and unconcerned about such issues and certainly does not act as a very important pressure group for specific policy actions.[27]

In spite of this incongruity between the theory underlying the hypotheses and the conditions that actually exist, the data reported above tend to support the three hypotheses. The reasons for this paradox may lie in the democratic theory which postulates direct linkages between the representatives and the general public, linkages which do not seem to exist, at least for foreign policy. It may be instead that representatives in the more democratic states, because of the democratic norms they have learned throughout their lives, operate within similar philosophic references and consequently favor similar kinds of policies, regardless of whether there is public pressure for these policies and perhaps even regardless of whether the policies are necessarily beneficial to the general public. Thus, the more democratic the nations, the greater the impact of a specific philosophical approach to foreign policy and the more likely that the representatives of such nations will arrive at similar foreign policy outcomes. While this reasoning differs from the democratic theory outlined by Cnudde and Neubauer, the first two hypotheses derived from that theory nevertheless are consistent with this reformulation.

Furthermore, the agreement among the democratic leaders of the developed nations may be greater than that among democratic leaders of the undeveloped nations because of the longer time that democracy has been operative in the developed nations and because of the greater impact that this philosophy has consequently exerted on decision-makers. The third hypothesis is thus also suggested, but again with a different theoretical tenet.

This paper is not intended, however, to argue for a specific modification of empirical democratic theory, but rather to show the similarity of findings between the empirical studies of domestic policies and of foreign policies, and to suggest that the implications of these similarities for democratic theory need to be considered. In a recent article,[28] Harvey Mansfield, Jr. points out that

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tion and Transaction," University of Southern California, Department of Political Science (mimeo), 1967.

[26] Cnudde and Neubauer, p. 524.

[27] For a review of much of the literature in this area as well as an illuminating study of the importance of public opinion to executive officials, see Bernard C. Cohen, *The Public's Impact on Foreign Policy* (Boston: Little, Brown, 1973).

[28] Harvey C. Mansfield, Jr., "Hobbes and the Science of Indirect Government," *The American Political Science Review*, 65 (March, 1971), 97–110.

Hobbes was the first philosopher to argue that the "indirect" question of whether policies are representative of the people's will ought to be asked before the "direct" question of whether the policy is "decent, good, or useful"; in short, one ought to focus first on process and only subsequently on the content of policy. It seems, however, that because of public ignorance and general unconcern with foreign policy issues, the question of whether or not such policy is representative of the people's will may not even be relevant. Thus, contrary to Hobbes's argument, for an understanding of the differences in foreign policies between democratic and undemocratic nations, perhaps attention should first be given to differences in the philosophical underpinnings of their policies rather than to the specific processes by which such policies are adopted.

The ABM Issue in the Senate, 1968–1970: The Importance of Ideology

ROBERT A. BERNSTEIN
WILLIAM W. ANTHONY
Texas A&M University

The U.S. Senate is beginning to play a major role in deciding national security/war and peace policy for the nation.[1] The modern Senate apparently perceives its role in this area to concern far more than just "real estate" questions.[2] The extensive debate and close votes on the ABM issue and various issues related to the Vietnam war give qualitative evidence of the Senate's policy concern. Furthermore, Russett's analysis of Senate voting behavior and defense spending gives strong quantitative evidence that something more than state economic advantage is motivating Senate behavior.[3]

The ABM issue caused more extensive debate in the Senate than did any other national security issue since 1945. Voting on the issue was about as close as possible, with killing amendments turned back 50–50 (the vice-president voting against) in 1969 and 47–52 in 1970. The issue spanned three years, two Congresses, and two administrations. It was one that the senators apparently considered important, on which they were forced to take a stand, and on which every vote counted. It was also an issue on which the factors influencing senatorial behavior had a chance to build over time. Thus, there is reason to believe that analysis of the ABM issue in the Senate will have general value.

The literature suggests at least three important theoretical reasons that a senator might adopt or change positions on such an issue as the ABM. One obvious hypothesis is that the greater the relative state economic benefits (in terms of production and research and development contracts) the more likely a senator will be to adopt a position favoring the issue.[4] Even where other factors suggest opposition to the bill, a senator might vote for a bill that would aid his constituency and presumably enhance his chances for re-election.[5]

A second hypothesis is that the more committed a senator is to a party (Republican or Democratic) the more likely he will be to reflect that party's position on the issue. At least by 1969, this would mean that the more consistently a Republican senator has voted with the party, the more he would favor the ABM. Hinckley states that in the 91st Congress, the Senate "debate on ABM . . . produced clear party lines, with an average of 62.3 per cent of the Democrats and 30.8 per cent of the Republicans voting in four roll calls against the expanded ABM program."[6] Party commitment then, might be the overriding influence in senatorial behavior.[7]

The third hypothesis is that senators vote in accordance with an internalized set of political beliefs variously termed political doctrines, conscience, or ideology.[8] It is assumed that the ideologies of virtually all U.S. senators may be ordered along a conservative-liberal continuum, and the more conservative the senator, the more likely he is to favor the ABM.[9] Hinckley states that the "ABM controversy in the Ninety-first Congress . . . split the Senate into liberal and conservative lines, with a majority of northern

[1] For an elaboration of this point, see Barbara Hinckley, *Stability and Change in Congress* (New York: Harper & Row, 1971), chapter 7.

[2] Even "real estate" questions have policy implications and the Senate has followed up on those implications, at least since the fifties; see Raymond Dawson, "Congressional Innovation and Intervention in Defense Policy: Legislative Authorization of Weapons Systems" *American Political Science Review*, 56 (March, 1962), 46–50.

[3] Bruce Russett, *What Price Vigilance? The Burdens of National Defence* (New Haven: Yale University Press, 1970), pp. 72–79.

[4] This is one of the general hypotheses tested by Russett, chapter 3.

[5] In regard to this issue, note the discussions of delegates, locals and opportunists in Roger Davidson, *The Role of the Congressman* (New York: Pegasus, 1969), chapter 4; and J. C. Wahlke, H. Eulau, W. Buchanan, and L. C. Ferguson in *The Legislative System: Explorations in Legislative Behavior* (New York: Wiley, 1962), part 4.

[6] Hinckley, p. 155.

[7] The American Political Science Association Committee on Political Parties in its 1950 report (*American Political Science Review*, 44 [September, 1950] supplement, 1–10) suggested that such party influence would be a desirable goal, rather than a current practice. However, Julius Turner in his *Party and Constituency: Pressures on Congress* (Baltimore: The Johns Hopkins Press, 1952) suggests that party influence is already felt strongly.

[8] Note the discussion of delegates and statesmen in Davidson, chapter 4, and Wahlke et al., part 4.

[9] While the conservative-liberal typology is sufficiently multidimensional (Robert Lane, *Political Thinking and Consciousness* [Chicago: Markham, 1969]) to cause serious conceptual problems in some cases, for this issue the subdimensions of the typology are unlikely to predict contradictory behavior.

Democrats opposing a majority of southern Democrats and a majority of Republicans . . . "[10]

It is our contention that ideology is a much more influential factor than either party commitment or economic incentives in determining a senator's position on national security/war and peace issues. National security issues such as this one, cause a fairly sharp liberal-conservative break because the relevant subdimensions of that typology tend to be reinforcing. That is, the conservative tends to support the ABM not only because of his focus on the "priority of American interests and rights"[11] but also because he believes that communism presents a grave danger to the U.S., that our defense against it should be adequately maintained, that defense is one of the few legitimate concerns of the national government, and that it may be necessary to risk war to prevent the advance of Communism.[12] The liberal tends to reject each of these positions. He emphasizes equality of nations,[13] suggests many other functions of government that should have priority over an increased defense effort and fears nuclear war more than he fears intermediate levels of Communist expansion.[14]

Asserting the importance of ideology does not mean, of course, that is the only influence senators experience in their choice of positions. Senators are subject to considerable party and administration pressure, and we expect party to be a minor factor in determining a senator's position.[15] In the case of the ABM issue, this may be especially true after the change of administrations in 1969.[16] We will try to demonstrate, however, that party commitment is not a major factor, that such evidence as Hinckley presented to illustrate the importance of party is spurious, and that party appears to be a strong factor primarily because it is so highly correlated with ideology; that is, the more committed Democrats tend to be more liberal and the more committed Republicans to be more conservative. If liberals tend to oppose and conservatives to favor the ABM, then Democrats will also tend to oppose and Republicans to favor the issue, even if party is of no importance in determining position on the ABM. We will attempt to demonstrate that if ideology is controlled for, party will become a relatively minor factor in determining a senator's position.

Even though it seems obvious that any representative would support spending beneficial to his constituents, and despite all the discussion of defense decisions being motivated by "real estate" considerations, we expect economic incentives to have virtually no influence in determining a senator's position on the ABM or, indeed, any other major national security/war and peace issue. We believe that the mechanisms regulating selection and tenure for the U.S. Senate were designed[17] and operate to increase self-direction and decrease constituency-direction. Further, it seems that national security/war and peace issues would not tend to be perceived as salient to the constituency.[18] Finally, Russett's empirical analysis of senatorial voting and defense-related expenditures for 1961–62 and 1967–68, yields virtually no evidence that economic incentives more than slightly influence the position that senators take on even general defense spending.[19] On any specific issue of major importance to the nation such as the ABM, we expect even this slight influence to disappear.

These arguments can be carried an important step further. Because ideology merely *tends* to influence position, not all senators would have adopted the position on the ABM that one would have anticipated on the basis of their degree of liberalism or conservatism. These deviants found the spotlight of national publicity turned on themselves and their position on the ABM from 1968 on. Especially as the vote grew close in 1969 and 1970, these senators found themselves under in-

[10] Hinckley, p. 151.

[11] Lane, p. 52.

[12] For elaboration of these themes, see especially Barry Goldwater, *Why Not Victory?* (New York: McGraw-Hill, 1962); James Burnham, *Suicide of the West* (New York: Day, 1964); Charles Malik, "The Challenge to Western Civilization," in *The Conservative Papers* (Garden City, New York: Doubleday, 1964), pp. 1–17.

[13] Lane, p. 52.

[14] For more elaboration, see especially Students for a Democratic Society, "America and the New Era," convention paper, Pine Hill, New York, June 14–17, 1963; H. Stuart Hughes, *An Approach to Peace* (New York: Atheneum, 1962); *The Liberal Papers* (Chicago: Quadrangle, 1962).

[15] For a discussion of administration pressures, including comments by Senator James Pearson regarding such pressures, see *Congressional Quarterly* (Fall, 1969) 99–104. Additionally, see Thomas Halsted's "Lobbying Against the ABM, 1967–1970," a paper delivered at the Sixty-sixth Annual Meeting of the American Political Science Association, California, 1970.

[16] As Hinckley notes, "With a Republican President . . . Democrats in Congress could give clearer voice to their 'dovish' inclinations than they could in the previous Democratic administration," p. 144.

[17] For a detailed discussion of this point, see Paul Eidelberg, *The Philosophy of the American Constitution* (New York: Free Press, 1968) chapters 5, 7 and 8.

[18] See Warren Miller and Donald Stokes, "Constituency Influence in Congress," *American Political Science Review*, 57 (March 1963), 45–56, for a discussion of how constituency influence varies directly with the saliency of the issue.

[19] Russett, pp. 72–76. It should be noted that both this and the Russett study have been restricted to the Senate; it is probable that economic incentives would be a more significant factor in determining the position of members of the House.

creasing pressure. By 1970, it is hypothesized that most of the deviants would have become painfully aware of a dissonance between their ideological beliefs and their 1968 position on the ABM.[20] In an effort to reduce this dissonance, the deviants were likely to change their positions on the ABM (this being easier than changing their ideological beliefs). Such a change in their positions regarding the ABM would also bring them in line with the positions of their principal reference groups in the Senate; thus, they would then conform to the expected behavior for their ideological group.[21]

It follows that senators who had adopted expected positions did not suffer from this type of cognitive dissonance. Additionally, they received support for the position that they had adopted by comparing their position with that of others like themselves. Hence, it is anticipated that those who adopted expected positions in 1968 would tend not to change them.

Methods

Position. A senator's position on the ABM is defined here as the extent to which his behavior indicates opposition to the establishment of an ABM system. Three scales were constructed, one for each year, by combining roll calls reported in *Congressional Quarterly* into Guttman scales. In each case, the roll calls were taken on amendments designed to kill or weaken the ABM program. Therefore, in the construction of the scales, every senator that had been paired or had announced against the amendment (i.e., in favor of ABM) was coded as a "one," and those who were paired or had announced in favor (i.e., against ABM) were coded as "zeros."

In 1968, there were six Senate votes on the ABM issue.[22] The Goodman test[23] indicated (at the .01 level) that these were indices of a single, unidimensional variable. In order to obtain the most valid and reliable scale, i.e., to minimize errors while still differentiating sizable groups, only three of the six votes (*Congressional Quarterly* Senate Votes #155, 81 and 154, in that order) were used in constructing the final Guttman scale of 1968 positions.[24] Therefore, all of the 1968 senators were classified as representing one of the four scale positions,[25] varying from zero (most anti-ABM) through three (most pro-ABM). The senators who were classified as zero's supported all of the killing amendments including the Young amendment to delete authorization for the ABM. Those classified as one's did not go as far as the Young proposal, but they were willing to reduce the army missile authorization $342.7 million, which would have had somewhat the same effect as the Young amendment. The two's supported neither of these amendments, but were willing to support the Cooper-Hart amendment delaying authorization for a year. The three's opposed all of these amendments. (See Appendix for classification of senators.)

There were five votes on the ABM issue in 1969,[26] all of which indexed a single, unidimensional variable (significant at the .01 level using the Goodman test). Again, to obtain the optimal scale, we combined only three of these votes (*Congressional Quarterly* Senate Votes #53, 211 and 54, in that order) to form the 1969 Guttman scale of positions on the ABM issue.[27] The 1969 senators were therefore also divided into four scale types. (See Appendix.) Senators classified as zero's supported all these amendments, including the first Smith substitute for the Cooper-Hart amendment, that would have prevented funds from being used for any aspect of the Safeguard ABM system. One's went almost as far, supporting deletion of Safeguard funds. Two's favored the second Smith substitute, which banned funds for Safeguard but allowed development of other ABM systems. Three's opposed all amendments.

In 1970, there were only three votes on the ABM issue.[28] While they all met the Goodman test criteria (at the .01 level), only two (*Congressional Quarterly* Senate Votes #237 and 238) could be used for the final Guttman scale.[29] There-

[20] These may both be treated as "cognitive elements," hence we have an application of the theories developed by Leon Festinger, *A Theory of Cognitive Dissonance* (Stanford: Stanford University Press, 1957).

[21] Note also Festinger's, "A Theory of Social Comparison Processes," *Human Relations*, 7 (1954), 117–140; and George Homans, *Social Behavior: Its Elementary Forms* (New York: Harcourt, 1961).

[22] *Congressional Quarterly*, Senate Votes #81, 82, 154, 155, 222, and 257.

[23] Leo J. Goodman, "Simple Statistical Methods for Scalogram Analysis," *Psychometrika*, 24 (March, 1959), 20–43.

[24] For example, we could have included *Congressional Quarterly* Senate Vote #22 in the scale, but it added one scale error and it created a scale step which included only four senators. For the scale we did compute the Coefficient of Reproducibility (.983) and Menzel's Coefficient of Scalability (.933).

[25] Except Senator Long (Mo.) who took no position on any of the votes and Kennedy (NY) who was assassinated before his position was clearly established.

[26] *Congressional Quarterly*, Senate Votes #53, 54, 55, 56 and 211.

[27] Coefficient of Reproducibility = .990; Menzel's Coefficient of Scalability = .971.

[28] *Congressional Quarterly*, Senate Votes #237, 238, and 242.

[29] *Congressional Quarterly*, Senate Votes #242, 238 had nearly identical cutoff points. The Coefficient of Reproducibility and Menzel's Coefficient of Scalability for the 1970 scale were both 1.00.

fore, the 1970 senators were classified only as 0, 1½ or 3—anti-ABM, neutral, or pro-ABM, respectively. (See Appendix.) Those classed as zero's opposed any funds in any bill being allocated for deployment of the ABM system at any site. The one and one-half's merely opposed specific allocations for deployment of the ABM at Whiteman and Warren Air Force bases. The three's opposed both amendments.

Relative Change in Position, 1968–70. Change in position refers to significant shifts, either more for or more against the ABM. This is how senators change vis-à-vis each other, not how the whole body changes vis-à-vis some outside standard.

Measuring this change would have been simpler if positions were indexed by interval data, or more discrete ordinal data. Unfortunately, given the nature of the data, senators could only be assigned to groups. This was done for the beginning (1968) and end (1970) of the time period. Thus, relative change was indexed by a change from one group to another.

To best ensure comparability of groups, equal numbers of groups were formed for each year, with corresponding groups (e.g., anti-ABM) being of roughly equal size. This partitioning could be done by simply collapsing the 1968 zero and one types to form a single anti-ABM group. The three's for each year remained the pro-ABM groups, while the other group was termed neutral, yielding the results shown in Table 1.

Relative change in position was then indexed by a change from one of these groups in 1968 to another in 1970. If the change was out of the anti-ABM group or from the neutral to the pro-ABM group, it was termed a *pro-ABM move.* Change out of the pro-ABM group or from the neutral to the anti-ABM group was termed an *anti-ABM move.*

Ideology. Ideology refers to how liberal or conservative a given senator's voting behavior has been relative to that of other senators. *Congressional Quarterly* (1968–70) reports Senate Conservative Coalition support and opposition scores and ACA and ADA indices for each senator for the Ninetieth Congress and for 1969, all of which tend to index ideology. Most of these measures of ideology, however, were influenced slightly by each senator's position on the ABM. To avoid a possible overlapping of the independent and dependent variables, the measures of ideology were recalculated, excluding all ABM votes from the roll calls. For as many years as the relevant data were available, the measures were combined to form an index based on the following: (conservative support score plus ACA index) minus (conservative opposition score plus ADA index). Senators were rank-ordered on the resulting index, from 1 for the most liberal (Hart) to 83 for the most conservative (Gurney). Thus, Fulbright, with a neutral index of zero, ranked forty-ninth. This rank order is reported in the Appendix.

Table 1. Size of Groups

Year	Anti	Neutral	Pro
1968	28	14	56
1970	26	11	53

Party Commitment. Party commitment refers to how consistently Democratic or Republican a given senator's voting behavior has been relative to that of other senators in his party. *Congressional Quarterly* (1968–1970) reports for each senator the percentage of party unity roll calls for 1969 and the Ninetieth Congress in which that senator voted with and against his party. "Party unity roll calls are those in which a majority of voting Democrats opposed a majority of voting Republicans."[30] Each senator's commitment was indexed by the percentage of his votes supporting his party minus the percentage of his votes opposing his party for the relevant sessions. All of the senators were then rank-ordered on party commitment: first the Democrats, from 1 for the most committed (Hughes) to 47 for the least committed (Allen); then the Republicans, from 48 for the least committed (Case) to 89 for the most committed (Gurney). The rank order is reported in the Appendix.

State Economic Benefit. State economic benefits refers to the relative economic advantage that would accrue to the senator's state if the ABM program were to be adopted. Economic incentive was indexed by dividing the total research and development and production contracts and subcontracts of $1,000,000 or more for each state[31] by that state's total personal income.[32] (See Appendix.)

Deviation From Expected Position. Deviation from expected 1968 position was established by comparing each senator's position with his ideological ranking. Since there were 28 anti-ABM senators in 1968, any one of the 28 most liberal senators was considered to have deviated from his expected

[30] *Congressional Quarterly Almanac,* 1969, p. 1070.
[31] Reported, *Congressional Quarterly,* May 30, 1969, pp. 848–849.
[32] Reported, *The American Almanac,* 1970, p. 318.

Table 2. Deviants from Expected 1968 Position

A. More Pro-ABM Than Expected		B. More Anti-ABM Than Expected	
Dodd	Magnuson	Williams (Del)	Cooper
Inouye	McGee	Ellender	Scott
Bayh	Muskie	Aiken	Prouty
McIntyre	Javits	Fulbright	Church
Harris	Pell	Hatfield	Smith
Pastore	Moss	Symington	Mansfield
Yarborough	Ribicoff	Burdick	Gore
Jackson	Brewster	McCarthy	Bartlett
		Lausche	Gruening
			Morton

position if he was not in the anti-ABM group. Similarly, the 56 most conservative senators were expected to adopt a pro-ABM position. And any moderate senator (i.e., one more conservative than the 28 and more liberal than the 56) was considered to have deviated from his expected position if he was not in the neutral group[33] (Table 2).

Tests and Results

Position. It was hypothesized that the more conservative a senator, the more likely he was to adopt a position favoring the ABM. The Gamma rank-order correlations[34] reported in Table 3 lend substantial support to this hypothesis. For each year there is a close association between ideology and position, Gamma being statistically significant and higher than between any other explanatory variable and position.

It might be expected that a general measure of liberalism-conservatism, such as we have used here would not have as close an association with position as a more specific measure of ideology based solely on defense issues. But in actual fact, the general ideology index more closely associated with position on the ABM each year than did Russett's general defense index.

Our contention that economic incentives would have little effect on the position each senator adopted is strongly supported. As Table 3 shows, none of the correlations between economic incentive and position are statistically significant, and while they are positive, none exceeds .10.

[33] The procedure adopted here follows closely that used by Robert Bernstein and Ralph Meyers ("An Approach to the Study of Change Over Time," presented at the Forty-Third Annual Meeting of the Southern Political Science Association, Gatlinburg, Tennessee; November 13, 1971) in their residual model of short term change.

[34] Gamma correlations were used here and later because Gamma is the ordinal measure of association best interpretable in terms of proportional reduction in error. (See Herbert Costner, "Criteria for Measures of Association," *American Sociological Review*, 30 [June, 1965], 341–353.)

It was argued that party commitment would be only a minor factor in determining position. Simply examining the degree of association between party and position will not settle that argument. Because of the anticipated closeness of association between ideology and both party commitment (Gamma = .63) and position (mean Gamma = .76), it was expected that there would be a close association between party and position even if the independent influence of party was very limited. In order to ascertain the comparative independent influence of each of the factors, and to check for significant interactions among them, the position scores for each cross-classification for each year were averaged (summarized in Table 4). The means were then subjected to an analysis of variance for each year. The analysis of variance (summarized in Table 5) clearly supports the theoretical propositions. Ideology accounts for a significant amount of the variation in position every year. It is the only variable which accounts for a significant proportion of the variance, and furthermore, in each case it accounts for more than 50 times the variance accounted for by all of the other variables together.

Party commitment and state economic benefits, alone or in interaction with other variables, never account for anything approaching a significant proportion of the variance in position. The change in administrations in 1969 apparently makes little difference as none of the other variables becomes appreciably stronger over time. Ideology is the only influential variable in 1968, and its influence grows stronger each year.

Change in Position. It was further hypothesized that if ideology proves to be very influential, then a senator whose position in 1968 was not consonant with his ideology would experience sufficient cognitive dissonance to make it likely that he would change positions in order to reduce that dissonance. On the other hand, a senator who adopted a position that was in accord with his ideology would not be expected to change positions.

Table 3. Gamma Correlations between Explanatory Variables and Position 1968, 1969, 1970

Variable	ABM Position 1968	1969	1970
Ideology	.65*	.77*	.86*
Party Commitment	.45*	.59*	.66*
Economic Incentive	.04	.10	.05
General Defense	−.64*	−.75*	−.78*

* Significant at the .05 level.

Table 4. Mean Position Scores and (Number) of Senators for all Crossclassifications[a]

		Party Influence			
		Republican		Democratic	
	Ideology	Economic Incentive	No Economic Incentive	Economic Incentive	No Economic Incentive
1968	Conservative	2.91	2.70	3.00	2.87
		(8)	(20)	(7)	(15)
	Liberal	1.60	1.33	1.45	1.43
		(5)	(3)	(11)	(28)
1969	Conservative	3.00	2.67	3.00	2.92
		(10)	(21)	(5)	(13)
	Liberal	1.33	1.33	1.18	1.15
		(6)	(6)	(11)	(27)
1970	Conservative	3.00	2.81	3.00	2.88
		(10)	(21)	(5)	(13)
	Liberal	0.50	1.00	0.95	0.56
		(6)	(6)	(11)	(27)

[a] 3 = strongest pro-ABM position; 0 = strongest anti-ABM position. Conservative is defined as having a positive score on the ideology index; liberal as having a negative score.

Table 6 indicates extraordinary support for the hypothesis. Of the 16 senators who changed position, 15 had deviated from their expected positions in 1968 and then moved to reduce their dissonance by 1970. Of the 56 senators who had adopted positions in accordance with their ideology in 1968, only Montoya changed his position by 1970. The Gamma correlation for Table 6 is .99 (significant at the .01 level.)

In conclusion, then, the position a senator adopted on the ABM issue has been shown to reflect his ideology, not his party commitment or the economic benefits his state would derive from the adoption of the ABM. The influence of ideology seems to have grown during each successive year. Virtually all the senators who changed positions between 1968 and 1970 had initial positions which were not in accord with their ideology, and they moved so as to bring them more in accord. Virtually all those senators whose initial position was in accord with their ideology maintained that position.

Table 5. Analysis of Variance in Position: 1968–1970

	F Ratio		
Source of Variance	1968	1969	1970
Main Effects			
Ideology (I)	33.47*	85.54*	125.66*
Party (P)	0.00	0.00	0.00
Economic Incentive (E)	0.40	0.46	0.00
Interaction Effects			
I×P	0.08	0.62	0.00
I×E	0.00	0.00	0.26
P×E	0.08	0.15	1.32
I×P×E	0.00	0.15	1.45

* Significant at the .01 level. All ratios were computed using a variation on the harmonic mean procedure for analyzing unbalanced designs suggested by Charles Gates of the Statistics Department, Texas A&M University.

Table 6. Deviation from Expected Position, Based on Ideology and Relative Change in Position, 1968–1970

	Deviation from Position, 1968		
Relative Change in Position, 1968–1970	More Anti-ABM than Expected	As Expected	More Pro-ABM than Expected
Pro-ABM move	5	0	0
None	10	55	5
Anti-ABM move	0	1	10

APPENDIX

State Senator	ABM Position 68	69	70	Ideological Ranking	General Defense	Party Commitment Ranking	State Economic Benefits
ALABAMA							
Hill	3			69	0	34	1.19
Sparkman	3	3	3	68	9	39	1.19
Allen		3	3	86		47	1.19
ALASKA							
Bartlett	1			43	23	27	0.00
Gruening	0			36	46	26	0.00
Gravel		1	0	21		19	0.00
Stevens		3	3	56		65	0.00
ARIZONA							
Hayden	3			57	9	23	2.14
Fannin	3	3	3	89	0	87	2.14
Goldwater		3	3	78		70	2.14
ARKANSAS							
Fulbright	0	0	0	49	47	30	0.00
McClellan	3	3	3	85	0	46	0.00
CALIFORNIA							
Kuchel	3			46	6	55	1.40
Murphy	3	3	3	78	0	77	1.40
Cranston		1	0	11		5	1.40
COLORADO							
Allott	3	3	3	86	5	84	0.00
Dominick	3	3	3	75		78	0.00
CONNECTICUT							
Dodd	3	3	3	39	11	25	0.36
Ribicoff	2	0	1½	15		17	0.36
DELAWARE							
Boggs	3	3	3	62	10	72	4.22
Williams	2	3	3	72	2	75	4.22
FLORIDA							
Holland	3	3	3	79	9	41	4.11
Smathers	3			67	10	38	4.11
Gurney		3	3	93		89	4.11
GEORGIA							
Russell	3	3	3	77	1	42	0.00
Talmadge	3	3	3	82	4	43	0.00
HAWAII							
Inouye	3	1	0	17		15	0.00
Fong	3	3	3	53	7	58	0.00
IDAHO							
Church	0	1	0	39	37	22	0.00
Jordan	3	3	3	77	1	80	0.00
ILLINOIS							
Dirksen	3	3		76	2	73	0.00
Percy	2	2	1½	35		58	0.00
Smith			3	60		63	0.00
INDIANA							
Bayh	3	1	0	28		14	0.00
Hartke	0	1	0	26	17	11	0.00
IOWA							
Hickenlooper	3			91	12	83	0.00
Hughes		1	0	6		1	0.00
Miller	3	3	3	76	1	74	0.00
KANSAS							
Carlson	3			73	9	69	0.00
Dole		3	3	81		81	0.00
Pearson	3	2	3	67	9	70	0.00

APPENDIX—(*Continued*)

State Senator	ABM Position 68	69	70	Ideological Ranking	General Defense	Party Commitment Ranking	State Economic Benefits
KENTUCKY							
Morton	1			63	11	66	0.00
Cook		1	3	51		56	0.00
Cooper	2	1	1½	47	18	59	0.00
LOUISIANA							
Ellender	2	3	1½	71	7	37	0.00
Long	3	3	3	74	46	36	0.00
MAINE							
MUSKIE	2	0	0	9	19	7	0.00
Smith	1	0	0	63	6	62	0.00
MARYLAND							
Brewster	3			32		20	0.00
Tydings	1	1	0	10		12	0.00
Mathias		1	0	24		49	0.00
MASSACHUSETTS							
Kennedy	0	1	0	8		16	5.68
Brooke	1	1	0	25		53	5.68
MICHIGAN							
Hart	0	1	0	1	38	6	0.10
Griffin	3	3	3	54		67	0.10
MINNESOTA							
McCarthy	0	0	0	31	18	28	0.62
Mondale	1	1	0	2		4	0.62
MISSISSIPPI							
Eastland	3	3	3	83	0	43	0.00
Stennis	3	3	3	88	0	44	0.00
MISSOURI							
Eagleton		1	0	7		3	0.00
Symington	1	1	0	37	17	26	0.00
MONTANA							
Mansfield	1	2	1½	45	18	10	0.00
Metcalf	0	1	0	16	18	15	0.00
NEBRASKA							
Curtis	3	3	3	91	0	86	0.36
Hruska	3	3	3	90	0	85	0.36
NEVADA							
Bible	3	3	3	64	10	33	0.00
Cannon	3	2	3	57	9	31	0.00
NEW HAMPSHIRE							
McIntyre	3	2	3	29		17	0.00
Cotton	3	3	3	79	1	79	0.00
NEW JERSEY							
Williams	1	1	0	3	18	2	18.66
Case	0	1	0	10	18	48	18.66
NEW MEXICO							
Anderson	3	3	3	48	17	29	0.00
Montoya	2	1	0	34		20	0.00
NEW YORK							
Goodel		1	0	12		49	0.79
Javits	2	1	0	14	31	51	0.79
NORTH CAROLINA							
Ervin	3	3	3	87	7	44	9.45
Jordan	3	3	3	82	7	40	9.45
NORTH DAKOTA							
Burdick	1	2	1½	30	46	10	0.00
Young	3	3	3	84	2	65	0.00

APPENDIX—(*Continued*)

State Senator	ABM Position 68	69	70	Ideological Ranking	General Defense	Party Commitment Ranking	State Economic Benefits
OHIO							
Lausche	2			66	4	27	0.00
Young	0	0	0	13	26	10	0.00
Saxbe		0	0	41		57	0.00
OKLAHOMA							
Monroney	3			52	18	28	0.00
Harris	3	1	0	20		13	0.00
Bellmon		3	3	86		74	0.00
OREGON							
Morse	0			23	47	27	0.00
Hatfield	0	1	0	40		52	0.00
Packwood		3	3	44		64	0.00
PENNSYLVANIA							
Clark	0			4	47	18	1.02
Schweiker		1	0	27		50	1.02
Scott	2	3	3	46	5	60	1.02
RHODE ISLAND							
Pastore	3	3	1½	19	18	21	0.00
Pell	2	1	0	9	23	14	0.00
SOUTH CAROLINA							
Hollings	3	3	3	65		35	0.00
Thurmond	3	3	3	92	0	82	0.00
SOUTH DAKOTA							
McGovern	0	0	0	19		8	0.00
Mundt	3	3	3	80	3	68	0.00
TENNESSEE							
Gore	1	0	0	42	47	24	0.00
Baker	3	3	3	70		77	0.00
TEXAS							
Yarborough	3	1	1½	22	46	9	0.54
Tower	3	3	3	85	0	76	0.54
UTAH							
Moss	2	1	0	15	46	12	0.00
Bennett	3	3	3	86	2	82	0.00
VERMONT							
Aiken	2	2	1½	50	18	54	0.00
Prouty	2	3	3	55	9	61	0.00
VIRGINIA							
Byrd	3	3	3	84	0	46	0.00
Spong	3	3	3	58		32	0.00
WASHINGTON							
Jackson	3	3	3	18	23	8	0.11
Magnuson	3	1	1½	30	17	12	0.11
WEST VIRGINIA							
Byrd	3	3	3	59	17	30	0.00
Randolf	2	1	1½	38	17	12	0.00
WISCONSIN							
Nelson	0	0	0	5		3	0.00
Proxmire	0	1	0	21	13	14	0.00
WYOMING							
McGee	3	3	3	33	18	20	0.00
Hansen	3	3	3	88		88	0.00

Need for Achievement and Competitiveness as Determinants of Political Party Success in Elections and Coalitions*

BRUCE BUENO DE MESQUITA
University of Rochester

The personalities of individual leaders and the strategies of groups, such as political parties, are important in determining the outcome of political contests. While analyses of both the individual and the group have been used extensively in studying political parties, relatively little empirical research has analyzed parties in terms of the linkages between individual and group behavior. The research that has been done in this area generally focusses on the effects that group pressures have on individual behavior.[1] That is, individual behavior has been the dependent variable, and group structure and behavior the independent variables. To my knowledge, no rigorous nonexperimental research has treated political party behavior as dependent on the collective personality attributes of the individuals who compose the group. Yet the goals and the strategies of political parties are in part determined by the predispositions of individual leaders.

This article identifies some of the linkages between the individual and the group level of analysis. The principal concern is with the impact of each leader's need for achievement[2] on his party's strategies in elections and in coalitions. In particular I will be concerned with the linkage between the need for achievement of individuals and the preferences of political parties for high, moderate, or low risks. In developing the hypotheses for this study, some reliance is placed on the experimental literature concerned with games and with need for achievement. Tests of the hypotheses, however, are restricted to the performance of political parties in actual political contests.

Two sets of dependent variables are used in this paper. The first set is concerned with the *electoral success* of parties. The second set concerns the *success in coalitions* of the same parties. Electoral success is defined by a party's ability to secure votes and win legislative representation. Success in coalitions is defined by the ability to gain control over the important resources of a coalition government, such as key cabinet portfolios. In short, a party's performance in both elections and coalitions may be defined as the ability to secure control over scarce, valuable resources.

Under conditions of scarcity, keen competition for control over valuable resources is inevitable. Where competition exists, success usually accrues to that group which combines the best mix of *resources* and the *skills* necessary to mobilize and utilize those resources. Size, or representation, is probably one of the most important resources of political parties, but size alone usually is not sufficient to insure success. History is replete with examples of major parties that, through a succession of defeats, lost most of their influence.[3] Sheer size, or representation, then, is important, but the ability to use resources wisely is probably more important. Thus, one may defeat a stronger adversary provided one can mobilize the *right* resources and make the *right* strategic choices. Indeed, there is evidence that when a party surpasses

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[1] Angela S. Burger, *Opposition in a Dominant-Party System: A Study of the Jan Sangh, the Praja Socialist Party, and the Socialist Party in Uttar Pradesh, India* (Berkeley: University of California Press, 1969); Paul R. Brass, *Factional Politics in an Indian State: The Congress Party in Uttar Pradesh* (Berkeley: University of California Press, 1965); Michael Brecher, *Political Leadership in India: An Analysis of Elite Attitudes* (New York: Praeger, 1969); Thomas A. Flynn and Frederick M. Wirt, "Local Party Leaders: Groups of Like-Minded Men," *Midwest Journal of Political Science*, 9 (February, 1965), 77–98; Samuel C. Patterson, "Characteristics of Party Leaders," *Western Political Quarterly*, 16 (June, 1963), 332–352; M. Margaret Conway and Frank B. Feigart, "Motivation, Incentive Systems, and the Political Party Organization," *American Political Science Review*, 62 (December, 1968), 1159–1173; Bruce Bueno de Mesquita, *Personality, Risk and Coalitions: The Case of India* (New York: Cambridge University Press, 1975).

[2] John W. Atkinson, *An Introduction to Motivation* (Princeton: Van Nostrand, 1964); John W. Atkinson, ed., *Motives in Fantasy, Action, and Society: A Method of Assessment and Study* (Princeton: Van Nostrand, 1958); David C. McClelland, *The Achieving Society* (Princeton: Van Nostrand, 1961).

[3] A few examples include the British Liberal Party, the American Federalist Party, and the Muslim League in India.

some threshhold in legislative representation its size militates against its ability to implement its best strategy for electoral and coalitional success.[4] From this discussion it follows that within certain parameters—including a minimal and an optimal size—the probability of achieving success under conditions of competition increases when a party is willing and able to make the appropriate strategic choices.

If the selection of contending strategies is a major determinant of party performance, then it is important to identify the determinants of strategic preferences among political parties. It is a hypothesis of this study that the attitude or *predisposition* that party leaders have toward risk taking determines their strategic preferences. These predispositions, in turn, are hypothesized to be an important predictor of party success. The covariation between risk-taking preferences on the one hand, and party performance on the other, then, is one of the central themes of this study.

The Model

In the next few pages I will develop a model intended to account for (a) the precoalition electoral success of political parties; (b) the success of the same parties in coalition governments; and (c) the postcoalition electoral success of those parties. The principal independent variables are the *strategic and motivational predispositions* of political party leaders.

The derivation of the model follows from these assumptions:

(1) Political parties compete in elections because they want to increase their organizational strength and legislative representation.
(2) When political parties form coalitions, some of them are more concerned with the potential electoral gains they will derive from a coalition than with fulfilling the immediate policy objectives of the coalition.
(3) (a) A political party will attempt to remain in a coalition as long as its leaders perceive that their party is not suffering a net loss from the alliance.
    (b) If the leaders perceive that their party is losing by remaining in the coalition, they will withdraw the party from the coalition.
(4) One large loss is more readily perceived than a comparable set of small losses.
(5) The more recent a loss, the more salient it is to a party's leaders and the greater its impact on their perception of the costs and benefits of the coalition.

Assumption 1 merely suggests that most parties are pragmatic, mass-oriented electoral parties; only a few are primarily ideological.[5] Assumption 2 contends that some pragmatic parties behave differently when they achieve power than they do in elections. That is, some parties use coalition governments primarily to fulfill their programmatic, or policy, goals, while other parties attempt to exploit the coalition's resources primarily to enhance their future electoral prospects. The third assumption posits that party leaders behave rationally in the context of their perceptions and their goals. Finally, the fourth and fifth assumptions maintain that political leaders, like all people, do not have perfect memories. Thus, they evaluate the costs or benefits of a particular decision from the perspective of the most recent experiences of the party.[6] Leaders therefore are more likely to tolerate a nibbling away of their resources than the loss of large portions of their resources in a single bite. That is, given imperfect memories, leaders compute the seriousness of some specific loss in terms of their base of strength at the time of the loss. They are less careful to compute the seriousness of the cumulative impact of such losses over an extended period of time.

Among the implications that may be derived from these assumptions are the following: (1) Different parties give different degrees of priority to fulfilling their policy goals as compared to their electoral goals. (2) Some parties will be satisfied (or at least not seriously dissatisfied) with less than their fair share of the coalition's payoffs.[7]

[4] For a theoretical discussion of this point see William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962); William A. Gamson, "Experimental Studies of Coalition Formation," in *Advances in Experimental Social Psychology*, ed. Leonard Berkowitz (New York: Academic Press, 1964); Theodore Caplow, "Further Developments of a Theory of Coalitions in the Triad," *American Journal of Sociology*, 64 (March, 1959), 488–493; Bueno de Mesquita, *op. cit.*; and Jerome M. Chertkoff, "Sociopsychological Theories and Research on Coalition Formation," in *The Study of Coalition Behavior: Theoretical Perspectives and Cases from Four Continents*, ed. Sven Groennings, E. W. Kelley, and Michael Leiserson (New York: Holt, Rinehart and Winston, 1970). Nonexperimental evidence for this point may be found in Peter H. Merkl, "Coalition Politics in West Germany," in Groennings et al.

[5] Joseph G. LaPalombara and Myron Weiner, eds., *Political Parties and Political Development* (Princeton: Princeton University Press, 1966); and William E. Wright, "Ideological-Pragmatic Orientations of West Berlin Local Party Officials," *Midwest Journal of Political Science*, 11 (August, 1967), 381–402.

[6] Melvin Guyer, "A Review of the Literature on Zero-Sum and Non-Zero-Sum Games in the Social Sciences," Mental Health Research Institute, University of Michigan (mimeo), n.d.

[7] Gamson, "Experimental Studies," in Berkowitz; Mancur Olson, Jr., *The Logic of Collective Action: Public Goods and the Theory of Groups* (Cambridge: Harvard University Press, 1965).

(3) Parties have different tolerance limits for dissatisfaction or relative gains and losses.

One of the most important decisions all coalition governments must make is the assignment of cabinet portfolios. On this decision depend both the performance of government functions, and the control over patronage, publicity and the establishment of grass-roots contacts and organizations which are so important to party growth. Given these stakes, why do some parties accept a disproportionately smaller share of important portfolios than other parties do? The preceding assumptions will be used to derive hypotheses concerning the distribution of important coalition payoffs.

It is an old adage that the squeaking wheel gets the oil. Perhaps in much the same way, a party that asks for more has a higher probability of getting more than a party that does not make any special demands. But how much more can it demand before it surpasses the limits of tolerance proposed by assumptions 3 and 4? I hypothesize that the distribution of benefits favors parties predisposed to pursue a mixed competitive/cooperative strategy at the expense of those parties preferring either pure cooperative or pure competitive strategies. Why?

In order to answer this question, I must first define some key terms. A party's strategic orientation is defined as cooperative if its leaders are predisposed to accept less than their fair share of the coalition's private benefits in the interest of achieving the coalition's collective goals.[8] Conversely, a competitively oriented party is one whose leaders are predisposed to sacrifice the collective goals of the coalition in the interest of their own party's pursuit of private benefits.

Cooperative parties prefer to distribute coalition payoffs (or private benefits) so as to minimize disruptive bargaining over individual benefits and maximize attention given to the policy goals of the alliance.[9] Because of their attentiveness to policy goals, cooperative parties attempt to distribute private benefits in accordance with an equity norm.[10] Such a decision rule would give each party a proportion of the private benefits that is commensurate with its contribution to the strength, or resource base, of the coalition. If a member of the coalition demands more than its "fair share" of the payoff, the cooperators meet that demand if they believe it will help facilitate the attainment of the coalition's policy goals. Of course, if the demand is so great that it violates assumption 3, the cooperators withdraw from the coalition rather than sacrifice any additional resources. In this way, they avoid any further erosion of their position, thereby preserving enough of their resources to help insure their influence in future coalitions. Insofar as such a demand does not arise, the dependence on an equity rule for distributing payoffs does, incidentally, help maintain the relative representational and organizational status quo between the parties within the coalition. Thus, if each party receives its fair share of patronage, publicity, and grass-roots contacts, then each party may expand its organizational and electoral base relative to those outside the coalition, but *ceteris paribus*, no party within the coalition will achieve disproportionately more than any other party within the coalition. Consequently, a completely cooperative coalition is in the best interest of the largest party, but not in the best interest of ambitious, smaller parties.

Competitive parties prefer to distribute the private benefits of the coalition in accordance with a non-equity rule. That is, by behaving competitively they attempt to convince their more cooperative partners that they are willing to disrupt the coalition's quest for the collective goals unless the cooperators give part of their share of the private benefits to the competitors. Of course, by sacrificing some of their share, the cooperators are surrendering a portion of the long-term growth of their party. Because the sacrifice of such resources is costly to the cooperators, it turns out that competitors who always pursue their private benefits at the expense of their partners can acquire a disproportionately large share of the payoff only for a limited time. Once they demand too large a portion, or demand one resource too many, the cooperators will lose their incentive to remain in the coalition. At this point, the coalition is terminated, with the competitor losing access to the private benefits it values, and the cooperator losing access to the collective benefits it values. The precise time when this occurs will depend, according to assumptions 4 and 5, on whether competition takes the form of constant demands or only of occasional, small demands for sacrifices. A completely competitive party, by definition, makes constant demands, while a party following a mixed strategy makes occasional demands.

If a party pursues a mixed strategy, sometimes facilitating the attainment of policy goals, and sometimes demanding extra incentives for its support of these goals, it should be effective in using the coalition to increase its organizational and representational potential. In fact, by nibbling away at the resources of its coalition partners, the

[8] Olson, *Logic of Collective Action;* Mancur Olson, Jr. and Richard Zeckhauser, "An Economic Theory of Alliances," *Review of Economics and Statistics,* 48 (August, 1966); 266–279.

[9] Gamson, in Berkowitz.

[10] The equity norm is defined as follows: If a party contributes X% to the legislative strength of a coalition, it is entitled to X% of the coalition's payoffs.

mixed-strategy party should be able, in the long run, to gain more private benefits from a coalition than a completely competitive party would. The mixed-strategy party depends on two factors to insure its long-run success in coalitions. It depends on the faulty memory of its allies, and on its ability to convince its cooperative partners that a small sacrifice increases *everyone's* opportunity to enjoy access to all the benefits of the coalition for the longest possible time.

From this discussion, two hypotheses follow:

H1: There is a positive association between the predisposition to pursue a mixed competitive/cooperative strategy, and the acquisition of a relatively large share of the important payoffs in a coalition.

and

H2: There is a positive association between the pursuit of a mixed strategy and the subsequent electoral success of political parties.

Need for Achievement

Deciding to pursue a competitive, cooperative, or mixed strategy depends, at least in part, on the attitude party leaders have toward taking risks.[11] If, for example, the leaders want to minimize the probability that their party will be ousted from a coalition government, then they behave as cooperatively as possible toward their partners. Regardless of the strategy chosen by the other members of the coalition, a cooperator is always a welcome partner, and as such, may expect to remain in a coalition government until it chooses to withdraw. Of course, in cooperating, a party implicitly accepts a high probability that its private benefits from the coalition will be relatively small in return for continued membership and the opportunity to pursue the collective goals of the alliance.

If a party's leaders choose to risk continued membership in a coalition government, in the hope of securing substantial benefits quickly, they behave very competitively toward their partners. By doing so they have the opportunity to secure large gains before being ousted from the government. Of course, they also run the risk that the other coalition members will balk at their demands before they can convert their new resources into real strength. Thus, offering patronage which you lose control of, or receiving unfavorable publicity may reduce a party's future electoral appeal, rather than enhance it. Such unfavorable consequences are more likely to be experienced by very competitive parties than by either very cooperative or mixed-strategy parties.

If the leaders of a party are predisposed to take moderate risks, their best choice is a mixed strategy, which allows them to make several incremental gains—in legislative seats, in popular votes, and in important cabinet portfolios. Because the party is not always competitive, it can capitalize on at least some of the extra patronage, publicity and grass-roots organizing that it has enjoyed. Nevertheless, it does run a higher risk than a cooperative party of being ousted from the coalition. Put somewhat differently, cooperative parties have a low probability of securing a disproportionate share of the coalition's private benefits at any point in time and a low probability of being ousted from the coalition. Mixed-strategy parties have a higher probability of securing extra payoffs at any point in time, and a higher probability of being ousted from the coalition and thereby being cut off from those benefits in the long run. Finally, competitive parties have the highest probability of securing a disproportionate share of the coalition's private benefits at any point in time, and the highest probability of losing access to those benefits in the long run by being ousted from the coalition. Thus, cooperation is a low-risk strategy, competition is a high-risk strategy, and the mixed strategy involves moderate risks.

How do parties decide among a high-risk competitive strategy, a moderate-risk mixed strategy, or a low-risk cooperative strategy? One attribute which influences this decision is the degree of need for achievement manifested by the party's leaders. Atkinson and others have repeatedly found that risk-taking preferences are significantly associated with a person's need for achievement. People above the mean in the need to achieve (N-Ach) tend to prefer activities involving *moderate* risks, while people below the mean prefer either high or low-risk activities.[12] This variable is a particularly reliable indicator of risk-taking preferences in competitive situations that the participants evaluate as being important to them. Elections and coalitions clearly are both competitive situations in which the outcome is important to the participants. (Assumptions 1, 2, and 3 deal with this point.) We may conclude, therefore, that parties whose leadership is high on the need to achieve are more likely than the others to implement a mixed strategy and, thereby, gain disproportionately both within the coalition and in post-coalition elections. But how does need for achievement help to account for the precoalition success of political parties?

Parties must make important strategic decisions when they decide which constituencies to contest and which to ignore in political systems characterized by single-member districts. Of course, no

[11] Guyer, "Review of the Literature."

[12] Atkinson, *Motives.*

party would willingly forego the opportunity to contest a constituency it is almost certain to win. Thus, virtually no variance is expected in the probability of contesting sure constituencies because of differences in need for achievement. Parties must frequently choose, however, between committing the bulk of their resources to contests they expect to be close or channeling some of those resources into constituencies where they have a low probability of winning. In a close constituency, the probability of success approaches the 50:50 level. That is, a party could reasonably expect to win half the time and to lose half the time. Contesting such a constituency involves, by definition, a moderate risk. Contesting a remote constituency, on the other hand, involves a high probability of defeat.

Why would any party commit resources to remote constituencies? First of all, success in such contests is an indication that the base of the party is expanding. A good showing in a remote constituency helps to increase the overall visibility of a party. A poor performance in a remote constituency, on the other hand, is not unexpected and so probably does not detract significantly from the party's image or base of support. Thus, little support is lost by failure, and some support may be gained by success in remote constituencies. The decision, however, to commit resources to remote contests does incur costs. Even if the party contests all of its potentially close constituencies, its resources are diverted if it is also contesting remote constituencies. The failure to concentrate its efforts in close constituencies may have serious repercussions for the future growth of the party which outweigh the gains derived from scattered successes in remote constituencies.

What kind of leaders are likely to prefer that their party pursue close contests, and what kind are likely to lead the party into remote constituencies? If the experimental findings are applicable here, we would expect leaders high in need for achievement to prefer the 50:50 chance of victory offered by close constituencies, while leaders low in need for achievement are expected to prefer high-risk remote constituencies. Because this difference in risk-taking preferences affects the selection of constituencies, parties with leaders who score high in the need to achieve should perform better in both pre- and postcoalition elections than parties low in need for achievement.

The discussion above can be restated in the form of the following hypotheses:

H3: Parties whose leaders are *above* the mean in need for achievement within their political system are likely to contest close constituencies, while parties *below* the mean are more likely to contest remote constituencies.

H4: Need for achievement is positively associated with the acquisition of important coalition benefits.

and

H5: Need for achievement is positively associated with the pre- and postcoalition electoral performance of political parties.

Research Design

A test of these hypotheses requires (a) indicators of party performance; (b) indicators of strategic preferences; and (c) indicators of need for achievement. The appropriate political system in which to gather data for these variable sets is one that has experienced several coalition governments composed of a broad spectrum of political parties. That system must also have experienced at least one pre- and one postcoalition election. Eight Indian states meet these criteria within a fairly brief, and comparable, time span. These states are Uttar Pradesh, the Punjab, Bihar, Madhya Pradesh, West Bengal, Kerala, Orissa, and Haryana.

India is an excellent political system for testing the hypotheses presented in this article for several other reasons as well. There probably is no other political system with as great a potential for developing diverse forms of government. Certainly, no other nation has experienced the diversity of governments that India has since 1967.

The widely differing experiences with coalition behavior that the Indian states have undergone provide an abundant opportunity for the formation and examination of a broad spectrum of generalizations. Since 1967 there have been more than twenty different coalition governments in the Indian states. In West Bengal, and to a lesser extent in Kerala, the governments have been based on common programs agreed to by ideologically homogeneous political parties of the left, while in Orissa there has been a coalition between ideologically like-minded parties of the right. In Uttar Pradesh, Bihar, Madhya Pradesh, Haryana, and the Punjab, the coalitions have consisted of parties with extremely disparate ideologies. Thus, in India we are able to examine the strategies used by parties in rightist, leftist, and nonideological alliances.

The Indian electoral and coalitional experiences are also representative of the diverse possibilities for governments in terms of the *number* of parties involved in the various state elections and coalitions. In Orissa, for example, there was a two party government between 1967 and 1971, while in West Bengal a coalition of no less than fourteen parties was formed. The other state governments have fallen at various intervals between these two extremes.

Table 1. Parties Studied, By State

Party	State
Jana Sangh	Uttar Pradesh
Communist Party of India (CPI)	Uttar Pradesh
Samyukta Socialist Party (SSP)	Uttar Pradesh
Praja Socialist Party (PSP)	Uttar Pradesh
Swatantra Party	Uttar Pradesh
Jana Sangh	Bihar
CPI	Bihar
SSP	Bihar
PSP	Bihar
Jana Sangh	Punjab
CPI	Punjab
Swatantra	Orissa
Communist Party of India (Marxist) (CPIM)	West Bengal
CPI	West Bengal
SSP	West Bengal
PSP	West Bengal
CPIM	Kerala
CPI	Kerala
SSP	Kerala
Jana Sangh	Madhya Pradesh
SSP	Madhya Pradesh
PSP	Madhya Pradesh

India is, in short, an excellent source of data for testing the model presented above because it is more representative than any other single political system of the entire spectrum of political ideologies, with a broad range of numbers of parties competing against each other in single-member districts, and a large assortment of coalition structures and behaviors. Thus, by being tested in India the model attains greater external validity than if it were tested in any other single parliamentary system.[13]

The principal data of this study come from extensive interviews with prominent political leaders in all the above states but Haryana. These interviews serve as the source for indicators of strategic preferences and elite need for achievement. The indicators of electoral performance are derived from several published sources of Indian voting statistics.[14]

Of the more than twenty coalition governments investigated in this article almost all included both national and regional parties. For the purposes of this study only the state branches of national parties that were represented in at least one coalition cabinet within their state are included in the analysis. Using this criterion, we consider the 21 state party branches of national parties listed in Table 1. National parties were selected because their participation in coalitions in several states facilitates cross-state comparisons of performance, and because their national-level aspirations play an important part in defining the significance of risk taking behavior for them. Regional parties, unlike the national parties, often have such a secure hold over their constituencies that they need not worry about the electoral repercussions of their risk-taking behavior in coalitions. They simply are not very likely to lose their base of support. National parties, on the other hand, are considerably more vulnerable to the consequences of risk-taking behavior in coalition governments. That is, regional parties can afford to be primarily ideological, while national parties with greater aspirations for power are generally compelled to be more pragmatic.[15] Selecting national parties also has the advantage of permitting an evaluation of the model's predictive power in national parliamentary elections (i.e., Lok Sabha) as well as in state legislative assembly elections (i.e., Vidhan Sabha).

We assume that the predisposition of a party to pursue a competitive, cooperative, or mixed strategy is equivalent to the individual predispositions of its principal leaders. A party's need for achievement is also defined as being equivalent to the individual achievement motivation of its leaders.[16] The principal leaders of a state party branch of a national party between 1967 and mid-1970 included any individual who (a) held the office of president, vice president, or secretary (or their equivalent) in one of the parties listed in Table 1;[17] (b) was a full cabinet minister in a coalition government (but not a deputy minister, or state minister); or (c) was named as a principal leader in the course of my interview with a majority of the people of the same party who met either of the above criteria. Through the use of these overlapping criteria, 119 individuals were identified as the relevant population. Of these, 88, or nearly seventy-five per cent, were interviewed by the author.[18]

[13] Donald T. Campbell and Julian C. Stanley, *Experimental and Quasi-Experimental Designs for Research* (Chicago: Rand McNally, 1963).

[14] Craig Baxter, *District Voting Trends in India: A Research Tool* (New York: Columbia University Press, 1969); R. Chandidas, Leon Clark, Richard Fontera, and Ward Morehouse, eds., *India Votes, A Source Book on Indian Elections* (Bombay: Popular Prakashan, 1968); India Office of the Registrar General, *Fourth General Election: An Analysis* (New Delhi: Ministry of Information and Broadcasting, 1967).

[15] Bueno de Mesquita, chapter 4.

[16] Alvin Zander and John Forward, "Position in Group, Achievement Motivation, and Group Aspirations," *Journal of Personality and Social Psychology*, 8 (March, 1968), 282–288.

[17] Further discussion of the criteria for selecting subjects, all of whom were interviewed between October, 1969 and June, 1970, may be found in Bueno de Mesquita.

[18] The 31 potential subjects who were not interviewed were missed generally because of logistic or

Each respondent's level of need for achievement was assessed with a set of open-ended, projective questions concerning the individual's and the party's future prospects and hoped-for accomplishments. These responses were examined for the presence of achievement imagery according to the revised coding scheme developed by Joel Raynor.[19] The questions used in the measurement of need for achievement are found in the appendix to this study.

From the individual need for achievement scores, the arithmetic mean was calculated to determine each party's achievement level. Although this computation is probably an oversimplification of the true relationship between a party's need for achievement and the achievement motive of each of its leaders, the experimental literature does lend some credence to the procedure.[20] In addition, other nonexperimental studies suggest that averaging does not commit as grave an injustice to reality as would several available alternative procedures.[21]

After the need for achievement level was computed for each state party unit, an index of the relative need for achievement of the parties was computed using the following procedure:

$$\text{N-Ach} = \frac{X_{ij}}{\sum_{i=1}^{n} X_{ij}} - 1/N$$

where X_{ij} is the need for achievement level of party i in state j and N is the number of national parties that participated in at least one coalition with i in state j. (A party is counted only once per state no matter how many of the state's coalitions it participated in.)

The achievement index reflects the extent to which a party's leaders' need for achievement was above or below the mean *in its state*. A positive sign indicates a party whose leaders were high in the need to achieve, while a negative sign indicates a party whose leaders were low in the need to achieve. This indicator is more meaningful than the raw need for achievement score of the party's leaders because it reflects the strength of a party's achievement motive relative only to those parties that it must actually compete against. The raw scores, on the other hand, reflect the distribution of need for achievement across parties that are not necessarily involved in competition with each other.

Before concluding this section, I should mention that there are a host of potential sources of error in the need for achievement variable. Some result from linguistic problems, some from the situational context, and still others from our virtually complete ignorance of the reliability of interview data for coding achievement imagery. The reader who is interested in these and related measurement problems is referred to an extensive discussion presented elsewhere.[22] Fortunately, it is highly unlikely that the achievement scores are contaminated by the actual degree of success experienced by the individuals interviewed for this study. Need for achievement is an extremely stable personality attribute which, in several long-term panel studies, has not been found to fluctuate with the waxing and waning of personal success. Instead, each individual's score remains essentially constant across time and across life's experiences.[23] Therefore, it is safe to assume that whatever measurement error there is in the achievement index is probably random and unbiased. Such error tends to reduce associations rather than inflate them.

The strategic *predispositions* of the parties were measured from the leaders' responses to a series of questions that could have elicited competitive or cooperative or other replies. The questions presented hypothetical situations that might arise in a variety of coalition settings. The leaders were asked to specify the ways in which they would respond to such situations. All the questions were open-ended so that the respondent could elaborate on the problem-solving techniques he would employ in the situations described.

Responses that indicated a preference for actions that would aid the respondent's party *at the expense of the other members of the coalition* were judged competitive. Responses that indicated that the respondent was willing to sacrifice some benefit that his party might have acquired in *order to*

scheduling difficulties. Many of them were defeated in the 1969 midterm election and so returned to their homes which were often situated in remote villages or inaccessible regions of the country. One potential respondent was in jail, and three were engaged in prolonged hunger strikes at the time it would have been feasible for me to interview them. Among the 88 who were interviewed, 83 were interviewed in English, I interviewed 1 in Hindi, 3 others were interviewed in Hindi, with the aid of an interpreter, and 1 was interviewed with an interpreter in Malayalam. Other details concerning the interviews may be found in Bueno de Mesquita, Appendix II.

[19] Joel Raynor, "Preliminary Notes on the Coding of the Professor's *Interview on Job and Job Pressures* for *n* Achievement, *n* Affiliation, and *n* Power," (Mimeo, Department of Psychology, University of Buffalo, March 28, 1967). Raynor's coding scheme is a slightly modified version of the standard coding procedures for assessing *n* Achievement. These procedures are described in John Atkinson, ed., *Motives*. The major modifications involve the use of controls for the length of time a respondent takes to answer interview questions.

[20] Zander and Forward.

[21] Bueno de Mesquita, chap. 6.

[22] *Ibid.*, chap. 4 and Appendix II.

[23] Atkinson, *Motives*.

protect the interests of the coalition were judged cooperative. For instance, one of the questions each respondent was asked was "Would you rather win a legislative victory that would lead to the break up of a coalition your party was in, or would you rather have the coalition remain intact and not win the victory?" If the respondent preferred to win a legislative victory at the cost of the coalition's survival, he received a competitive score on this question. If he preferred to give up the legislative victory and, thereby, preserve the coalition, he received a cooperative score on this question. To be included in the subsequent analyses of strategic predisposition, the respondent had to score as cooperative or competitive on at least one of the eight questions in this series. Only two respondents did not meet this requirement. From the raw scores each party's strategic predisposition, or *strategy index*, was computed using the following formula:

$$\mathrm{SI} = 1 - \left|\frac{f_1 - f_2}{f_1 + f_2}\right|$$

f_1 is equal to the frequency of competitive responses for the leaders of a given party, and f_2 is equal to the frequency of cooperative responses for the leaders of the same party. The strategy index assesses the extent to which a party deviates from a purely cooperative or purely competitive strategy, without regard for the direction of its deviation. Thus, a score of 0.00 indicates that a party is predisposed to pursue either a purely cooperative or a purely competitive strategy. A score of 1.00 indicates that the number of competitive and cooperative responses given by the leaders of that party are balanced.

The strategy index (SI) is a measure of a party's *predisposition* to compete or cooperate and *not* a measure of actual behavior. Indeed, the very questions from which the index was derived dealt with situations that logically could have occurred but never actually did. All other things being equal, one can reasonably assume that strategic behavior is highly correlated with strategic predispositions. This inference is helpful, though not essential to the utility of the model being tested here.

Data Analysis

Hypothesis 1 says that parties predisposed to follow a mixed strategy are more likely than others to secure a disproportionate share of the important payoffs in a coalition. Earlier I defined important payoffs as the percentage of key portfolios controlled by a party.[24] A key portfolio was one that involved control over a substantial amount of patronage, publicity, and grass-roots contacts. The relative importance, or desirability, of each cabinet portfolio was determined from a question asking the party leaders to specify the portfolios they would most like to have under their own control. The portfolios were then ranked according to the frequency with which they were named. Eight portfolios were named most often: Home, Finance, Agriculture, Education, Labour, Land and Land Revenue, Power and Irrigation, and Industries.[25] Each party's success at acquiring important coalition payoffs was computed from the percentage of these important portfolios controlled by each party. This percentage indicated their absolute control over these ministries. Hypothesis 1, however, is concerned primarily with whether a party received a disproportionate share of these portfolios, rather than with the actual amount it received. To measure disproportionality, it was assumed, in accordance with the equity norm, that a fair distribution would result if each party received a percentage of important portfolios equal to the percentage of seats it contributed to the legislative strength of the coalition. Thus, if a party received a larger percentage of portfolios than the percentage of seats it contributed to the alliance, its share was disproportionate. In fact, if the number that results from dividing the first of these percentages by the second is greater than one, a party is receiving a disproportionately large share, and if it is less than one, it is receiving a disproportionately small share. This measure is termed the ministerial index (MI).

The correlation between the strategy index and the ministerial index is .21 ($N=20$). Although this correlation is fairly weak, it is in the predicted direction, and does lend some support to the hypothesis.

Hypothesis 2 suggests that the greater a party's predisposition to follow a mixed strategy, the better it does in postcoalition elections. This hypothesis was tested by computing the correlations between the strategy index and (a) the number of seats each party won in the 1969 midterm elections (which occurred following the termination of the first round of coalitions); (b) the percentage of votes won in 1969; (c) the number of seats won by the parties in the 1971 national parliamentary (Lok Sabha) elections; (d) the percentage of votes won in the 1971 Lok Sabha elections; (e) the change in state legislative assembly (Vidhan

[24] Michael Leiserson, "Factions and Coalitions in One-Party Japan: An Interpretation Based on the Theory of Games," *American Political Science Review*, 62 (September, 1968), 770–787.

[25] The respondents were explicitly asked not to include the Chief Ministership among their choices. This was done because it was assumed this choice would add no additional variance since all the respondents almost certainly would have wanted the highest office in the government.

Sabha) seats won between 1967 and 1969; (f) the change in votes won between 1967 and 1969; (g) the change in the number of Lok Sabha seats won between 1967 and 1971; (h) the change in Lok Sabha votes won between 1967 and 1971; (i) the percentage change in Vidhan Sabha seats; and (j) the percentage change in Vidhan Sabha votes. The results are presented in Table 2.

An examination of Table 2 reveals several interesting patterns. First of all, each of the coefficients is in the predicted direction. While one of them is only negligibly positive, four are sufficiently large, given the number of observations, to be considered statistically significant if a random sample had been used. Although we cannot, strictly speaking, generalize about their significance, probability tests are useful here as a means of comparing the relative magnitude of correlation coefficients based on different numbers of observations. It is for this purpose that significance levels are reported in this paper.

The four significant coefficients are especially interesting because they occur with the measures of *change* in party performance over time. In each instance the initial year used to measure the party's performance is a precoalition year. That is, it is a time period before the parties could implement their strategic predispositions within the framework of a ruling government. The strategy appears to have paid off handsomely. Thus the correlations suggest that the greater a party's predisposition for a mixed strategy, the greater the improvement in its ability to win seats in *both* the state assembly and national parliamentary contests. Furthermore, the greater its orientation toward a mixed strategy, the greater the improvement in its vote-getting ability in Lok Sabha contests. Correspondingly, the ability to win votes in the state assembly elections increases with orientation toward a mixed strategy, but it is not as strong. In general, we may conclude that the evidence lends considerable support to the second hypothesis.

Hypothesis 3 is concerned with the extent to which need for achievement affects a party's propensity to take moderate risks. Risk taking here is operationalized by assessing the propensity each party had in 1967 to contest legislative assembly constituencies in which it had a reasonable or a remote chance of winning. The Indian Election Committee defines a constituency as being reasonable, or *close*, for a party if that party wins or loses the constituency by fewer than 5,000 votes. Constituencies which are won or lost by more than 15,000 votes are all defined as *remote*. Winning by more than 15,000 votes is taken as an indicator that a party took a low risk, while losing by that much suggests a high risk. Close constituencies approximate a fifty-fifty chance of winning, and so involve a moderate risk. As noted earlier, all parties are expected to pursue all constituencies that they are very likely to win, regardless of their need for achievement level. In fact, this is borne out by the evidence. The parties whose leaders were high in need for achievement contested 19 constituencies that were safe for them (out of a total of 678 constituencies contested), while the parties whose leaders were low in need for achievement contested 20 such constituencies (out of a total of 731 contested).

Table 2. Strategy and Postcoalition Performance

	Strategy Index (SI)	*N*
Vidhan Sabha Seats, 1969	.38	18
Vidhan Sabha Votes, 1969	.15	15
Lok Sabha Seats, 1971	.37	21
Lok Sabha Votes, 1971	.07	12
Change in Vidhan Sabha Seats, 1969–1967	.56*	18
Change in Vidhan Sabha Votes, 1969–1967	.38	15
Change in Lok Sabha Seats	.42*	21
Change in Lok Sabha Votes	.58*	12
Percentage Change in Vidhan Sabha Seats	.66**	19
Percentage Change in Vidhan Sabha Votes	.35	15

* significant at the .05 level or better
** significant at the .01 level or better

The risk taking indicator being used does not, strictly speaking, reflect the *subjective probability* of success perceived by each party in each constituency. Rather, it reflects the *post hoc* probability of success. One might argue that this indicator should not be used to express each party's awareness of its likelihood of success or failure in these constituencies. This argument, however, is probably incorrect. I have excluded from the analysis those constituencies that were won or lost by more than 5,000 votes but fewer than 15,000 votes. These constituencies, rather than the extreme cases I have selected, are likely to have been the most difficult for the parties to formulate reasonable expectations about. It is unlikely that any party would systematically assess its chances as good across the several constituencies where it was severely defeated. Similarly, it is unlikely that many parties systematically evaluate very close constituencies as offering a remote probability of success or failure. In any event, even if these perceptual errors did occur, there is no reason to believe that they would be systematically related to a party's aggregate level of need for achievement. Thus, insofar as the indicator used reflects

Table 3. The Association Between the Number of Political Parties Preferring Close or Remote Constituencies and the Parties' Levels of Need for Achievement[a]

		Constituency Risk	
		Close	Remote
Need for Achievement	High	5	2
	Low	3	5

$Q = .61$
$\phi = .34$

[a] Based on 15 political parties

such perceptual errors, these errors are probably randomly distributed across the parties under investigation.

In the first analysis, the percentage of close constituencies contested by each party is treated as the indicator of its propensity to take moderate risks. If a majority of the constituencies contested by any given party was close, then the party is assumed to prefer moderate risks. If, on the other hand, the majority of a party's contests were in remote constituencies, then the party is assumed to prefer high risks. If the hypothesis is to be supported, then most parties with leaders who have relatively high need for achievement should prefer close constituencies, while most parties with leaders who have low need for achievement should prefer remote constituencies. Table 3 presents the relationship between need for achievement in parties and their risk-taking orientation in selecting constituencies to contest. As we can see, high achievement parties do prefer close constituencies and low achievement parties prefer remote constituencies, as predicted. The strength of this difference can be determined by computing two simple measures of strength of association. The first, Yule's Q, reflects the extent to which some condition, such as high need for achievement tends to accompany some other condition, such as contesting close constituencies. For Table 3, Yule's Q is a fairly substantial .61.

The second measure of association (ϕ) assesses the extent to which high need for achievement is accompanied by a disproportionately great tendency to avoid remote constituencies, while low need for achievement is accompanied by a disproportionately great tendency to contest remote constituencies. For Table 3 the ϕ coefficient is .34. Although the ϕ coefficient is only moderately large, ϕ, like Q, is in the predicted direction and so does support the hypothesis. From these two coefficients we may conclude that high need for achievement very frequently occurs with a preference for close constituencies, while low need for achievement disproportionately occurs simultaneously with a preference for remote constituency contests.

The hypothesis can also be examined another way. Whereas in Table 3 the cell entries were the number of political parties that shared the particular row and column trait, in Table 4 the cells are the number of constituencies in which a party shared each of the row and column characteristics. This test is considerably more demanding than was the first test of the hypothesis. Here we are asking not simply whether any particular party contested a majority of close or remote constituencies, but rather how many constituencies in all were contested by parties whose leaders were high in need for achievement or low in need for achievement. Yule's Q for Table 4 is .33, while ϕ is .17. Given the large number of cases here, these coefficients may be considered as fairly substantial deviations from the null hypothesis that there is no association between need for achievement and the type of constituency preferred.

We may tentatively conclude from the evidence presented in Tables 3 and 4, that high need for achievement is substantially associated with a disproportionately large preference for moderate risk, close constituencies.

Hypothesis 4 posits that need for achievement is positively associated with the acquisition of important coalition benefits. To test the hypothesis, both the ministerial index and the absolute percentage of important portfolios controlled by a party were used as dependent variables. Because the hypothesis simply predicts that parties with leaders who have high need for achievement do better than parties with leaders who have low need for achievement, rather than predicting a linear relationship between need for achievement and coalition performance, a simple one-way analysis of variance was used to test the hypothesis. Here we measure the extent to which the presence of a

Table 4. The Association Between the Number of Candidacies in Close or Remote Constituencies and Each Candidate's Party's Level of Need for Achievement[a]

		Constituencies	
		Close	Remote
Need for Achievement	High	376	283
	Low	287	424

$Q = .33$
$\phi = .17$

[a] Based on 1370 candidates

particular characteristic, such as high or low need for achievement, systematically differentiates a set of subjects on some other dimension, such as coalition performance. If, for example, the parties with high achievement leaders and the parties with low achievement leaders each secured the same average percentage of important portfolios, the analysis of variance would indicate that need for achievement does not adequately differentiate parties on the basis of performance. If, on the other hand, the difference in the coalition success of parties is much larger across the two need for achievement categories than within each of the categories, the analysis of variance would indicate that we can differentiate between successful and unsuccessful parties based on their need for achievement. Tables 5 and 6 present the analysis of variance for the ministerial index and for the absolute percentage of important portfolios secured in the coalitions.

As can be seen from the two analyses of variance, the parties whose leaders were high in need for achievement did better both on the ministerial index, where the difference is slight, and on the percentage of important portfolios secured during the tenure of the coalitions, where the association is far more substantial. The high need for achievement parties secured, on the average, more than their fair share of important portfolios (103 per cent of their fair share), while the low need for achievement parties received, on the average, less than their fair share (87 per cent of their fair share). The high need for achievement parties also received a much larger absolute percentage of important portfolios than did the parties with leaders who have low need for achievement (nearly 18 per cent compared with 4 per cent for the low achievement parties). In other words, the parties with high need for achievement leaders controlled a disproportionately large number of important cabinet portfolios. Control over these cabinet positions, in turn, gave the parties with high need for achievement leaders greater control over such important coalition benefits as the distribution of patronage, publicity, and

Table 5. Disproportionality in the Distribution of Important Cabinet Ministries as Associated with Need for Achievement

	Ministerial Index		
	Mean	Variance	*N*
High *N*-Ach Parties	1.03	.075	7
Low *N*-Ach Parties	0.87	.070	8
All Parties	0.95		

Significance $< .30$

Table 6. Percentage Distribution of Important Cabinet Portfolios as a Function of Need for Achievement

	Percentage of Important Portfolios		
	Mean	Variance	*N*
High *N*-Ach	.178	.008	7
Low *N*-Ach	.040	.001	8
Total	.105		

Significance $< .002$

greater contacts with the masses. We can conclude, therefore, that hypothesis 4 is substantially supported by the evidence drawn from these Indian political parties.

Need for achievement has proved to be a good indicator both of risk-taking preferences in elections and of performance in coalitions. Now it remains to be seen if parties with leaders who have high need for achievement are able to capitalize on these advantages. That is, do these parties actually perform better than low need for achievement parties in elections? The response to this question is more complex than were the responses to the previous four hypotheses. Electoral performance is an intricate phenomenon that requires a broader set of variables and controls than we have utilized thus far. First, electoral performance is operationalized using the following fourteen variables: the number of state legislative assembly seats won (1) in 1967 and (2) in 1969; the number of Lok Sabha seats won (3) in 1967 and (4) in 1971; the percentage of votes secured for the legislative assembly (5) in 1967 and (6) in 1969; the percentage of votes secured for the Lok Sabha (7) in 1967 and (8) in 1971; (9) the change in the number of seats won in the state assembly contests between 1967 and 1969; (10) the percentage change in the number of seats won in the state assembly contests between 1967 and 1969; (11) the change in the number of Lok Sabha seats won between 1967 and 1971; (12) the change in the percentage of votes won in the state assembly elections between 1967 and 1969; (13) the percentage change in the percentage of votes won in the state assembly elections between 1967 and 1969; and (14) the change in the percentage of votes won for the Lok Sabha between 1967 and 1971.

Variables 9 and 10 differ in that 9 is a measure of the absolute shift in number of seats for each party, while variable 10 controls for the initial base of support a party had in 1967. Thus, a party losing twenty seats out of forty would be considered equal to a party losing twenty seats out of twenty seats on variable 9. Variable 10, however,

Table 7. Party Performance on 14 Indicators of Electoral Success as Associated with Need for Achievement

Var.	Need for Achievement	Mean	Variance	*N*	Significance
1	High	53.29	858.90	7	<.002
	Low	12.75	33.07	8	
2	High	46.33	353.87	6	<.001
	Low	9.71	73.24	7	
3	High	6.86	13.14	7	<.01
	Low	2.00	3.71	8	
4	High	6.00	50.33	7	<.10
	Low	1.38	3.98	8	
5	High	16.09	58.05	7	<.001
	Low	4.40	3.85	8	
6	High	13.68	28.21	6	<.001
	Low	-3.57	10.74	7	
7	High	16.41	55.63	7	<.001
	Low	4.24	9.32	8	
8	High	26.25	103.07	3	<.03
	Low	1.25	0.13	3	
9	High	−2.83	871.77	6	<.96
	Low	−3.43	22.95	7	
10	High	.18	.36	6	<.12
	Low	−.29	.18	7	
11	High	−.86	63.14	7	<.94
	Low	−.63	0.84	8	
12	High	−.37	13.13	6	<.84
	Low	−.70	4.49	7	
13	High	.01	.08	6	<.27
	Low	−.21	.16	7	
14	High	8.19	88.24	3	<.17
	Low	−1.05	0.89	3	

would record the first party as having suffered a 50 per cent loss, while the second party would have suffered a 100 per cent loss. The difference between variables 12 and 13 is comparable to the difference between variables 9 and 10.

Table 7 is a summary of fourteen analyses of variance that measure the relative performance of parties whose leaders score high or low on need for achievement. Only in the analysis of the change in the number of Lok Sabha seats won between 1967 and 1971 did the low need for achievement parties have a higher average level of performance than did the high need for achievement parties, and in that one case the difference between the two groups is negligible. Of the remaining thirteen analyses of variance in which the parties with high need for achievement leaders *did* out-perform the parties with low need for achievement leaders, seven of the differences between the average level of performance of the high and low need for achievement parties is significant at the .05 level or better. Six of these seven are significant at the .01 level or better, and four of these six are significant at the .001 level or better. In other words, hypothesis 5 is overwhelmingly supported by the evidence. The parties with leaders who have high need for achievement clearly outperformed the parties with leaders who have low need for achievement in pre- and postcoalition electoral situations. What is more, they apparently used the advantages they gained during the tenure of the coalitions to expand their legislative representation disproportionately and likewise disproportionately to increase their vote-getting ability between the pre- and postcoalition periods.

The bivariate analyses are now complete. All five hypotheses have been supported by the evidence. In other words, parties whose leaders are predisposed to follow a mixed strategy and who are high on the need to achieve seem to do better, at least in India, in elections and in coalitions than parties whose leaders prefer cooperative or competitive strategies and who are low in need for achievement. It is now appropriate to explore the combined effects of the predictor variables on party performance. In doing so, only two of the most interesting dependent variables will be examined further in this particular study. Others have been explored elsewhere.[26]

Using multiple regression, I attempt to account for the absolute change and percentage change in the number of seats won in the state legislative assembly contests between 1967 and 1969 as a function of the model's predictor variables. These dependent variables have been chosen because they reflect the dynamic, longitudinal impact of the model on party representation. These two variables are reliable indicators of the expansion or contraction of a party's legislative base. Thus, although measures of change in voting percentages, or measures of absolute strength at a single point in time, are interesting, they are not as informative for party leaders, nor are they as indicative of the future potential of a party as are the two variables I have selected. If the model does accurately predict changes in representation, then it provides a useful supplement to the intensive investigation of the idiosyncratic aspects of Indian politics which account for the success or failure of political parties.

[26] Bueno de Mesquita, chaps. 5 and 6.

Table 8. Percentage Change in Vidhan Sabha Seats Won Between 1967 and 1968 as a Function of the Strategy Index and Need for Achievement

Variable	Coefficient	Std. Error	*T*-Statistic	Significance
Intercept	−1.311	.301	−4.361	<.001
SI	1.64	.405	4.043	<.001
N-Ach	1.71	.873	1.959	<.07

R^2 = .63 Std. Error = .34 F = 10.93 Sig. = .002 Number of Parties = 16

Positive findings in the multivariate analyses would suggest that exploring the psychological dynamics of risk taking in political parties has great utility. The success of the multivariate analyses would also suggest that the overall performance of political parties is very much determined by factors controlled by the party, as well as by the electorate. Such a finding would be theoretically interesting to social scientists and policy-relevant to party leaders. Along with several other studies,[27] it would suggest that when elites are recruited on the basis of political style and other psychological attributes, handsome electoral gains may result. That is, the regression models will help to identify the parameters within which parties may manipulate their electoral performance by recruiting leaders with particular strategic and achievement orientations.

First, the percentage change in Vidhan Sabha seats won between 1967 and 1969 is estimated as a function of the strategy index (SI) and need for achievement (*N*-Ach). The results are presented in Table 8. The regression coefficients indicate the sizable benefits that result from being high in need for achievement and from preferring a mixed strategy. Thus, a hypothetical party predisposed to follow a completely mixed strategy and having in its ranks all of the leaders in its state with high need for achievement could expect, all other things being equal, to treble its representation.

Parties, of course, are interested in the actual number of seats they can expect to win or lose more than in percentage changes in their strength. The second dependent variable assesses actual changes in numbers of seats. To estimate these changes we must add an additional variable to the regression analysis to account for the initial base of strength from which each party contests subsequent elections. The number of seats won by each party in 1967 is used to assess that base of strength. The other independent variables are, again, the strategy index and need for achievement. These three variables account for 72 per cent of the variance in the change in the number of seats won between 1967 and 1969. Table 9 presents the regression analysis, while Figure 1 is the scatterplot of the observed change in seats compared with the estimated change in seats. The coefficients in this analysis indicate the number of seats a party could expect to gain or lose as a function of its performance on any one of the three predictor variables if we control the effects of the other predictor variables. For instance, all else being equal, the average party could have expected to lose half its representation between 1967 and 1969. On the other hand, if it followed a completely mixed strategy, it could have expected a gain of 39 seats, all other things being equal. If a party had in its ranks all the leaders in its state who had high need for achievement it would have gained 75 seats, all other things being equal.

[27] Gordon S. Black, "A Theory of Political Ambition: Career Choices and the Role of Structural Incentives," *American Political Science Review*, 66 (March, 1972), 144–159; Joseph A. Schlesinger, *Ambition and Politics: Political Careers in the United States* (Chicago: Rand McNally, 1966); Dwaine Marvick, ed., *Political Decision-Makers: Recruitment and Performance* (New York: Free Press of Glencoe, 1961); Robert D. Putnam, "Studying Elite Political Culture: The Case of 'Ideology,'" *American Political Science Review*, 65 (September, 1971), 651–681.

Table 9. Changes in Vidhan Sabha Seats Won Between 1967 and 1969 as a Function of the Number of Seats Won in 1967, the Strategy Index and Need for Achievement

Variable	Coefficient	Std. Error	*T*-Statistic	Significance
Intercept	−18.91	11.43	−1.654	<0.12
Number of Seats Won in 1967	−0.47	0.13	−3.690	<0.003
SI	39.31	13.40	2.934	<0.01
N-Ach	75.12	31.82	2.360	<0.04

R^2 = .72 Std. Error = 10.91 F = 10.07 Sig. = .002 Number of Parties = 16

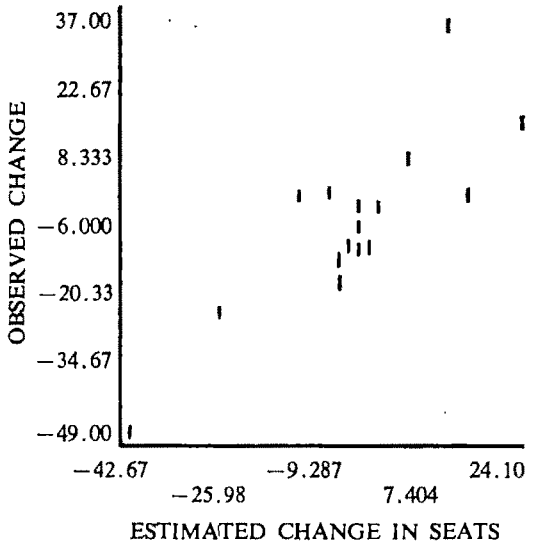


Figure 1. Scatterplot of the Observed Change in Vidhan Sabha Seats Won by 16 Parties Between 1967 and 1969 Compared to the Estimated Change in Their Representation As a Function of Their Legislative Strength in 1967, Their Strategic Predispositions, and Their Need for Achievement.

Conclusion

This study has developed and tested a model of strategic and motivational predispositions which successfully accounted for differences in the pre- and postcoalition electoral success of Indian political parties. It has also proved to be a good indicator of at least one aspect of party success within coalition governments. The psychological attitudes of party leaders toward risk taking accounted for much of the variance in the political success of their parties.

The model also explains differences in party performance without examining such questions as voter attitudes and behavior, party platform and ideology, or historical and situational context. Although those factors are important, and probably account for much of the unexplained variance, it is interesting to know that we are able to make significant generalizations about political parties based on elite scores on a very small set of variables.

The model underscores the significance of elite recruitment processes in determining the future prospects of political parties. It also highlights the ability of present party leaders to manipulate their party's strategic predisposition and level of need for achievement so as to improve future success in winning elections and in securing important positions in government.

APPENDIX

Questions Pertaining to Need for Achievement

The questions from which need for achievement was measured are as follows:

Would you please tell me what are the most important objectives that your party has for 1970? Any others?

How do you think coalitions affect the public image and political influence of your party?

Do you think there is any advantage to initiating negotiations [with other parties]? What are the advantages?

Would you rather win a legislative victory that would lead to the breakup of a coalition your party was in, or would you rather have the coalition remain intact and not win the victory? Why?

Of course being in the government gives you the opportunity to distribute patronage. What effect has this had on the relations between the party's leaders and its rank and file membership?

Do you feel that the distribution of patronage can be an effective tool for improving the party's electoral appeal? How?

Can patronage improve the party's legislative effectiveness? How?

In what ways do coalitions affect the organization and cohesion of your party? Its discipline?

What has your party gained from joining coalitions?

What did you originally expect to gain from the coalitions?

Do you think you have gained more or less than the parties you coalesced with? In what ways?

Does your party have a coalition strategy? What is it?

Compared to coalitions, what are the advantages and disadvantages of pre-election alliances? Any others?

This is a very hypothetical question. I realize that this situation shall never arise in India. Nevertheless, I am interested in your general attitude toward the problem. Let's say there were a legislature with one dominant party, several parties of approximately equal strength, and one or two weaker parties. If in that legislature your party had 18 per cent and the other parties had 30 per cent, 20 per cent, 15 per cent, 11 per cent and 6 per cent respectively, which of the following do you think would be the best coalition for your party?

(a) 30, 20, 18 = 68%
(b) 30, 18, 15 = 63%
(c) 30, 18, 11 = 59%
(d) 30, 18, 6 = 54%
(e) 20, 18, 15 = 53%
(f) 20, 18, 11, 6 = 55%

Why would you prefer that one?

Bases of Budgetary Incrementalism*

JOHN WANAT
University of Kentucky

The lure of incrementalism as an explanatory tool in the social sciences is strong because the chaos in human behavior which scholars seek to rationalize makes sense according to incrementalist explanations. Lindblom's "Science of 'Muddling Through' "[1] gave intellectual respectability to apparently mindless and minor decisions by pointing out that these decisions follow a pattern in being successive, limited, and comparative. Lindblom explains such decisions by noting that because of resource and rationality constraints they cause only a slight departure from the status quo. This means that the apparently mindless change is reasonable and even predictable.

Unfortunately, incrementalism, though intuitively and experientially appealing, infrequently meets the canons of academic and scientific explanation. Incrementalism, I suggest, can be used in a descriptive sense or an explanatory sense. A phenomenon is descriptively incremental if changes in the system are only marginally different from the status quo. A phenomenon is explained by incrementalism if reasons for the marginality of the changes can be given. In most situations incrementalism fails because it is difficult to define the status quo adequately and to measure the small change from that state. Even where that is possible, it is very difficult to show that the change is a result of limited rationality and resources or, indeed, of any other cause.

In the budgetary arena, however, scholars have been able to define the status quo as the previous appropriation and to define changes from that state both as the increase requested for the next fiscal year and also as the increase appropriated by the legislature. Fenno, Sharkansky, Crecine, and Davis, Dempster, and Wildavsky[2] all have found empirical evidence for incrementalism so operationalized. They have established that the agency requests are a small increment over the previous appropriation and that the current appropriation is only a small amount less than the amount requested. Incrementalism, then, at least descriptively obtains in the budgetary process.

But to be an explanatory tool, incrementalism must specify not only that the change is a slight modification of the existing state, but also must specify why that modification is slight. Davis, Dempster, and Wildavsky's prominent work on federal budgeting attempts to show that descriptive incrementalism occurs because of strategic considerations on the part of the participants.[3]

The present essay proposes to show that the techniques used by Davis, Dempster and Wildavsky do not allow valid inferences to be made about *why* budgetary incrementalism occurs. Second, evidence from recent Labor Department budget experience will be presented to explain why budgetary change at the federal level can properly be called incremental, not only in a descriptive sense, but also in an explanatory sense.

The Davis, Dempster, and Wildavsky Model of Budgetary Incrementalism

Davis, Dempster, and Wildavsky explain federal budget behavior through linear regression models—three for the agency decision about what to request and three for the congressional decision about what to appropriate. These six models are all variants of the following basic models:

$$\text{APPROPRIATION}_t = \alpha\ \text{REQUEST}_t + \phi \qquad (1)$$

$$\text{REQUEST}_t = \beta\ \text{REQUEST}_{t-1} + \theta \qquad (2)$$

$$\text{REQUEST}_t = \gamma\ \text{APPROPRIATION}_{t-1} + \rho \qquad (3)$$

The congressional decision rule says basically that the appropriation is a relatively fixed per-

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[1] Charles E. Lindblom, "The Science of 'Muddling Through,'" *Public Administration Review*, 19 (Spring, 1959), 79–88.

[2] Richard Fenno, *The Power of the Purse* (Boston: Little, Brown, 1966), pp. 267, 354, 356; Ira Sharkansky, "Agency Requests, Gubernatorial Support, and Budget Success in State Legislatures," *American Political Science Review*, 62 (December, 1968), p. 1223; John P. Crecine, *Governmental Problem-Solving* (Chicago: Rand McNally, 1969), pp. 206–208; O. A. Davis, M. A. H. Dempster, and Aaron Wildavsky, "On the Process of Budgeting: An Empirical Study of Congressional Appropriation," in Gordon Tullock, ed., *Papers on Non-market Decision-Making* (Charlottesville, Va.: Thomas Jefferson Center for Political Economy, 1966), pp. 102–123.

[3] O. A. Davis, M. A. H. Dempster, and Aaron Wildavsky, "A Theory of the Budgetary Process," *American Political Science Review*, 60 (September, 1966), 530–531.

centage of the request plus some random variations, ϕ. The agency decision rules say that the request is a percentage change from either the last request or the last appropriation plus suitable random variations, θ or ρ. Variants of these models allow for other more sophisticated interpretations of budgetary interaction. These models were fitted to actual appropriations data and the variant that yielded the best fit was adjudged to represent budget decision behavior. Davis, Dempster, and Wildavsky say:

> Our models are not predictive but explanatory. The alternative decision equations can be tried and the most appropriate one used when data on requests and appropriations are available. The appropriate equation explains the data in that, given a good fit, the process behaves "as if" the data were generated according to the equation.[4]

Thus, if the fit to the data is best for the equation $\text{REQUEST}_t = \gamma\ \text{APPROPRIATION}_{t-1} + \rho$, the data is explained "as if" the agency, "convinced of the worth of its programs, tends to be aware that extraordinarily large or small requests are likely to be viewed with suspicion by Congress"[5] and consequently "requests a percentage of its previous year's appropriation."[6]

Similarly, if the fit to the data is best for the equation $\text{APPROPRIATION}_t = \alpha\ \text{REQUEST}_t + \phi$, the data is explained "as if" "Congress believes that an agency's request, after passing through the hands of the Budget Bureau, is a relatively stable index of the funds needed by the agency to carry out its programs," and therefore "responds by appropriating a relatively fixed percentage of the agency's request."[7]

The principal criterion used to choose one of the equations to represent budget behavior is correlation between the variables.[8] Davis, Dempster, and Wildavsky accept the equations and the basic models underlying the equations because

> estimated correlation coefficients for the best specifications of each agency are generally high. Although the calculated values of the multiple correlation coefficients (R's) tend to run higher in time series than in cross-sectional analysis, the results are good. We leave little of the variance statistically unexplained.[9]

Estimation of the equations (1), (2), (3) and their variants yielded high r's and regression coefficients such that typically

$$.90 < \alpha < 1.00$$
$$1.00 < \beta < 1.20$$
$$1.00 < \gamma < 1.20$$

These findings corroborated Fenno's conclusions that at the federal level the budget process is at least descriptively incremental since the requests and the appropriations are only slightly different. These findings are also congruent with those of Sharkansky at the state level and Crecine at the municipal level. The correlation coefficients typically are well above .90 for the best fitted agency decision equation, with the congressional decision equation usually having a correlation exceeding .96. Thus it would seem that the equations are good fits to the data and it is valid to infer that the "as if" models explain budgetary incrementalism.

A cautionary note must be sounded, however. Comparison of the r's and other statistics associated with equations (1), (2), (3) and their variants allows choosing those two equations of the six options that "fit" the data best; but the choice is limited to only those six options. Even though the correlations are very high, the analyst does not know whether the correlations are "significantly" high. That is, can these high correlations be generated by random data? To answer that question, data were generated that fitted the constraints of descriptive incrementalism but which varied randomly within those constraints. The size of the correlations from the randomly generated data relative to the correlations from the Davis, Dempster, and Wildavsky equations allows judging whether their "as if" explanations are justifiably inferred.[10]

To investigate this question the descriptive model of budgetary behavior at the federal level was defined by the inequalities:

$$\text{REQUEST}_t \geq \text{APPROPRIATION}_{t-1} \quad (4)$$

$$\text{APPROPRIATION}_{t-1} \leq \text{APPROPRIATION}_t \quad (5)$$

$$\text{APPROPRIATION}_t \leq \text{REQUEST}_t \quad (6)$$

and by the added constraint that generally the terms of the inequalities are to be only marginally unequal. The data presented by Crecine, by Davis, Dempster, and Wildavsky, by Fenno, and by Sharkansky with few exceptions bear out the accuracy of this characterization of the budget relations. Because their work in different arenas of budgeting says that relations (4), (5), and (6) apply, I am confident that the budgetary system behaves as the model says it does. Therefore, any data that meet constraints (4), (5), and (6) can be interpreted as simulating budgetary data, and these data can be used in a sensitivity analysis of

[4] *Ibid.*, p. 542.
[5] *Ibid.*, p. 532.
[6] *Ibid.*
[7] *Ibid.*, p. 534.
[8] *Ibid.*, p. 536.
[9] *Ibid.*, p. 537.

[10] For a good discussion of the use of sensitivity analysis, see L. V. Grant, "Aspects of Theory Evaluation," *British Journal of Political Science*, 3 (January, 1973), 101–110.

Table 1. Frequency Distribution of Correlations on Various Data Bases

	Random Variation Data			Davis-Dempster-Wildavsky Data*	
R	$APP_t=\alpha REQ_t+\phi$	$REQ_t=\beta REQ_{t-1}+\theta$	$REQ_t=\gamma APP_{t-1}+\rho$	Best Congressional Equation	Best Agency Equation
.995–1	9.4%	0.4%	1.2%	41.8%	13.1%
.990–.995	16.0	0.8	4.0	4.5	3.3
.980–.989	35.6	8.6	21.7	22.4	3.3
.970–.979	17.6	14.3	24.2	7.5	14.8
.960–.969	11.9	10.6	19.7	1.5	6.6
.950–.959	4.9	12.3	10.6	4.5	4.9
.940–.949	2.0	13.9	7.8	6.0	6.6
.930–.939	0.4	8.2	3.7	1.5	4.9
.900–.929	1.2	16.0	5.7	4.5	8.2
.850–.899	0.8	10.2	1.2	4.5	18.0
0.0 –.849	0.0	4.5	0.0	1.5	16.4
$N=$	244	244	244	67	61

* Source: O. A. Davis, M. A. H. Dempster, and Aaron Wildavsky, "A Theory of the Budgetary Process," *American Political Science Review*, 60 (September, 1966), p. 537.

the Davis, Dempster, and Wildavsky decision models.[11]

When only these constraints were used, sets of number pairs representing requests and appropriations were generated in the following fashion:

(1) A number between 0 and 1000 was randomly chosen to represent the appropriation in year zero, APP_0

(2) With a ceiling of a 10 per cent increase over APP_0, the request, REQ_1, was generated by using a random number, V_1, such that $0 \leq V_1 \leq 1$. Then REQ_1 was defined as equalling $APP_0+(.1)(V_1)(APP_0)$. REQ_1 is therefore equal to or a random increase over APP_0, but less than or equal to a 10 per cent increase.

(3) The appropriation for year one, APP_1, was generated by using a second random number, W_1, such that $0 \leq W_1 \leq 1$. This number W_1 represents a random cut from the increment requested by the agency. Remember that the base, APP_0, is sacrosanct. Therefore, APP_1 is defined as

$$APP_1 = APP_0 + (.1)(V_1)(APP_0)(W_1)$$

(4) By using APP_1 as APP_0 had been used in step (2), the next request and appropriation were iteratively generated in accord with the following equations.

$$REQ_t = APP_{t-1} + (.1)(V_t)(APP_{t-1})$$

$$APP_t = APP_{t-1} + (.1)(V_t)(APP_{t-1})(W_t)$$

(5) The process was repeated until a sequence of 15 requests and 15 appropriations were generated. The number 15 was used to approximate the period from FY 1947 to FY 1963 used in Davis, Dempster, and Wildavsky's study.

(6) This entire process was then repeated using another randomly generated base number between 0 and 1000 for a total of 50 times.

Steps 1 through 6 were then repeated using ceilings of 20 per cent, 40 per cent, 80 per cent, and 160 per cent. This yielded sequences of size 50, 50, 48, 48, and 48 for a total of 244 randomly generated pairs of sequences representing appropriations and requests.

By lagging the variables APP and REQ, the variables APP_t, APP_{t-1}, REQ_t, and REQ_{t-1} were made available. These allowed calculating the correlation coefficients for the basic equations (1), (2), and (3) on the random variation data. By comparing correlations for the random variation data on the three basic equations with the best fit correlations for the longest period from Davis, Dempster, and Wildavsky's study, the validity of their "as if" explanation could be adjudged. Since the distributions of the correlations for ceilings of 10 per cent, 20 per cent, 40 per cent, 80 per cent, and 160 per cent were substantially the same, all the runs were combined into the one distribution displayed in Table 1.

Analysis of Table 1 shows that 76 per cent of the best congressional specifications from Davis, Dempster, and Wildavsky have $r \geq .97$, while 80 per cent of the random variation cases have $r \geq .97$. For agency decision equations, 34 per cent of the Davis, Dempster, and Wildavsky cases have

[11] Consistency of simulation output and actual data is one of two criteria Crecine suggests for evaluating a simulation model. See his *Governmental Problem-Solving*, pp. 111–114, 143–145.

Table 2. Correlations Between Sets of Two Random Variables, One of Which Dominates the Other

R	Frequency
.95–1	1
.90–.94	3
.85–.89	6
.80–.84	9
.75–.79	11
.70–.74	14
.65–.69	12
.60–.64	9
.55–.59	9
.50–.54	9
.45–.49	5
.40–.44	4
.35–.39	2
.30–.34	3
.25–.29	0
.20–.24	2
.15–.19	1

For $N=15$, if $r \geq .412$, $p < .05$
if $r \geq .558$, $p < .01$
if $r \geq .725$, $p < .001$

$r \geq .97$, while 24 per cent of the $REQ_t = \beta\ REQ_{t-1}$, and 51 per cent of the $REQ_t = \gamma\ APP_{t-1}$ correlations exceed or equal .97. While the Davis, Dempster, and Wildavsky results are clustered a little higher than the random variation data correlations, we are dealing with differences of only .01 or .02 in the absolute value of Pearson correlation coefficients. Also we are comparing the r's of the basic equations on the random data with the r's of the best fits of all the variants on the actual data. We conclude that while the maximum correlation criterion is adequate for distinguishing among the models presented by Davis, Dempster, and Wildavsky, it does not justify accepting that whole class of models because the correlations are of the same magnitude as those generated by random data under budgetary constraints. This is not to say that the Davis, Dempster, and Wildavsky characterization of budgetary decision rules is inaccurate. It may very well be that those decision models are true representations of budgetary decision phenomena, but such a judgment cannot be made solely on the basis of high correlation coefficients.

Though slightly tangential to the analysis, it is important to know what constraints generated the high correlations obtained by Davis, Dempster, and Wildavsky. Logically it has to be either the marginality of the differences or the constant dominance of one variable by the other. If the strong correlation is caused by marginality, correlation techniques might still be useful if increments requested and cut were the units of analysis. However, the fact that the distributions of correlation coefficients in the random data analysis were substantially the same no matter what the request "ceiling" suggests that the domination of one variable by the other is the determining relation. To investigate that conjecture, 100 pairs of variables were generated in the following fashion:

(1) 15 random numbers between 1 and 1000 were generated, X_i, $i=1, 15$. This is the first variable.
(2) 15 random numbers, V_i, were generated between 0 and 1.
(3) The second variable, Y_i, is created according to the equation,

$$Y_i = (X_i)(V_i)$$

The correlations between X (corresponding to the requested increment) and Y (corresponding to the amount appropriated) are displayed in Table 2. The median r is in the range .65 to .69. For $N=15$, an $r>.41$ is significant at the .05 level, which means that about 90 per cent of the correlations are "significant" at the .05 level. Since these sets of sequences were created only with the constraint that, pairwise, one variable dominate the other, and since the r's are very high, we must conclude that for an N of 15, the high correlations are a function of one variable's always dominating the second and not of marginality. This implies that dealing with budget increments will not normally allow the use of correlation as a criterion by which one can clearly discriminate among linear models.

Another Basis of Budgetary Incrementalism

What then can be done to explain the incremental increases on budget requests sent to the legislature and the restriction of cuts to those requested increments? In other words, how does one explain the basic and descriptively incremental relations:

$$REQUEST_t \geq APPROPRIATION_{t-1}$$

$$APPROPRIATION_t \geq APPROPRIATION_{t-1}$$

$$APPROPRIATION_t \leq REQUEST_t$$

where generally the terms of the inequalities are only marginally different. I will first present evidence that the inequalities obtain, then reasons why the terms of these inequalities are only marginally different from each other.

Since it is desirable that the terms of explanation be isomorphic to elements in the real phenomenon to be explained,[12] it is reasonable to

[12] Similarity of decision mechanisms between those in the real world and those of the model is another criterion suggested to evaluate a simulation model. See Crecine, *Governmental Problem-Solving*, pp. 111–114, 143–145. See also Richard M. Cyert and James G. March, *A Behavioral Theory of the Firm* (Englewood Cliffs, N.J.: Prentice-Hall, 1963), p. 317.

examine agency and congressional behavior for one's formulations of the crucial variables.

To that end, all general fund line-items in the regular Labor Department budget bills for FY 1968 through FY 1972 were examined. All House appropriations hearings and all the House and Senate reports on the bills were studied. This analysis corroborated the conclusions of other budget research that the base, or the previous year's appropriation for all practical purposes, is not considered to be fair game.[13] For instance, the Budget Bureau requires that the justification materials given Congress by the agencies spell out the increases requested over the previous year's base and the purposes to which the increases are to be put.[14] Questioning in the hearings focused on the requested increases. The text portion of Congressional appropriations reports almost always dealt entirely with the reasons for allowing or disallowing the increment requested and seldom paid any attention to the total amount requested. All of this points to the fact that Congressional and executive actors specifically focus on the increments and not on the total amount requested.

The composition of these increments, therefore, was next examined. In the justification material the agencies presented to Congress, a primary distinction was always made between mandatory and programmatic requests.[15] This means that the agency conceived of the request as composed of three parts: the base, the agency's mandatory needs, and the agency's programmatic desires. Mandatory requests are all those new costs needed to keep the agency operating at the same level as in the previous year. Usually these are the amounts necessary to cover increased pay costs, within-grade pay increases, and increased telephone, mail, utility, and other service costs. These costs occur year after year, though not always in the same amounts, because the costs of "staying in business" rise irrespective of changes in programs. These costs generally represent only a small percentage of the total request. Programmatic requests are those made for initiating new programs or for expanding services or products.

Since the Labor department agencies and the Budget Bureau thought in terms of base, mandatory requirements, and program desires, the question arises whether Congress did also. Evidence to be presented indicates that the Congress did, in fact, use the distinction between programmatic and mandatory requests as a basis for cutting the requests. Representative selections from the House and Senate Reports on the Labor Department Appropriations bills illustrate Congressional awareness of this distinction.

To begin with, most of the House reports typically contained an introductory section titled "Controllable and Uncontrollable Appropriations," referring to programs like Medicaid, Social Security, retirement pay for the Public Health Service veterans, grants to states for unemployment services, etc., in which existing law specifies that the appropriation be tied to some measure out of the agencies' control, such as number of eligible applicants. These items occasioned comments like the following:

> The total "uncontrollable" appropriations in the bill account for an increase of almost $2.2 billion over the appropriations for fiscal year 1971. The Committee has in each case approved the budget estimates for these programs.[16]

Even where Congress was not compelled by law to appropriate an "uncontrollable" amount of money, it appeared to treat mandatory requests without question. For instance, reporting on requests from the Bureau of Labor Statistics, the House Committee once said, "the amount recommended provides for all mandatory cost increases and one-half of the $2,855,000 requested for program increases."[17] Similarly, it reported, "The action of the Committee provides for mandatory

[13] It is very curious that with all the qualitative evidence presented by Wildavsky, by Fenno, and by Sharkansky about the concept of the base, Davis, Dempster, and Wildavsky's operationalization of the budget relations did not utilize that concept. Consider the basic agency equation, $\text{REQUEST}_t = \gamma\ \text{APPROPRIATION}_{t-1} + \rho$. This means that the request, which we know consists of the accepted base plus the desired increase, is "equated" to a percentage of the base appropriation. This makes the decisionable increase a function of the already accepted base and thereby muddles concepts the qualitative literature holds to be distinct. The same kind of criticism can be made of the congressional decision equations.

[14] "The justification of each appropriation or fund account will begin with a statement relating the estimate to past and current year requirements. This statement will follow the form of exhibit 24 . . ." Exhibit 24 is titled "Analysis of Changes in Requirements." Executive Office of the President, Bureau of the Budget, "Preparation and Submission of Annual Budget Estimates," Circular No. A-11, June 1970, p. 27.

[15] Actually a fourfold distinction can theoretically be made. The Budget Bureau in Circular No. A-11 of June 1970, Section 24.2, says that increases requested over the budget authority of last year should be categorized as program increases, automatic increases (equivalent to our mandatory category), financing increases ("Activities financed by another program or agency in the preceding year."), and administration commitments ("Program increases to which the president has publicly and specifically committed the Administration."). In the data used in this study no administration commitments were spelled out, and only rarely was a financing increase found. For all practical purposes, therefore, the mandatory/programmatic distinction is all that applied.

[16] U.S., Congress, House, Appropriations Committee, "House Report 92–374," p. 4.

[17] U.S., Congress, House, Appropriations Committee, "House Report 91–1310," p. 7.

cost increases, but allows for no program increase" for the Wage and Labor Standards Administration for FY 1970.[18] In like manner, the Bureau of International Labor Affairs for FY 1969 was cut $24,000, while being allowed a $38,000 increase over the FY 1968 appropriation, the House report noting that "The amount recommended is the exact amount estimated to be necessary to continue activities at the current year's level."[19] These quotations are typical of those made in the reports on the Labor-HEW appropriations bills in the period under consideration.

The Senate Committee's reports evince a similar criterion for cutting. For example, the Bureau of Apprenticeship and Training's FY 1969 request merited the following comments, "The amount allowed includes $496,100 to cover mandatory and financing increases and $299,600, with 24 of the requested 40 new positions. . . ."[20] The Women's Bureau's request for FY 1968 was granted because, "The small increase over 1967 is for mandatory changes in pay for employees of the Bureau."[21] And the Wage and Hour Division's FY 1970 request was granted with the observation that, "The increase is for mandatory costs and for increased costs in the enforcement activity."[22]

As the reports indicate, Congress gave at least the Labor agencies the money they requested for "mandatory" purposes. Since the mandatory requests usually recur every year, we now have a reason *why* $\text{REQ}_t \geq \text{APP}_{t-1}$. And since Congress seemed to always grant the request for the mandatory items but not always for programmatic items, we have reason to believe that $\text{REQ}_t \geq \text{APP}_t \geq \text{APP}_{t-1}$.

It is not necessary to rely solely on representative quotations from the reports to see what Congress did with the requests. Additional verification can be found by collecting data on the requests and the appropriations themselves, and subjecting those quantitative data to regression analysis. Regression is appropriate here because our argument centers about the estimated coefficients and not the associated correlations. To that end, data were collected on the amounts requested and appropriated for all 68 general fund line items in the five regular Labor Department appropriations bills from FY 1968 through FY 1972. The components of the request were found in the hearings.[23] The appropriations were regressed on the components of the request: the base, the mandatory requests, and the programmatic requests. One estimated equation follows:

$$\begin{aligned}\text{APPROPRIATION} &= .9901\ \text{BASE} \\ &\quad SE = .0018 \\ &\quad + .9446\ \text{INCREMENT REQUESTED} \\ &\quad SE = .0078 \\ &\quad R^2 = .9998\end{aligned}$$

This regression shows that the base was practically untouched by Congress, while the increment was cut by more than 5 per cent, as previous qualitative literature led us to expect. Since the base is evidently sacrosanct, the increment requested, ΔREQ, and the increment appropriated, ΔAPP were analyzed. By breaking the requested increment, ΔREQ, into the mandatory request, $\Delta\text{MANDREQ}$, and the programmatic request, $\Delta\text{PROGREQ}$, the following regression equation was estimated:

$$\begin{aligned}\Delta\text{APP} &= 1.0038\ (\Delta\text{MANDREQ}) \\ &\quad SE = .0374 \\ &\quad + .9398\ (\Delta\text{PROGREQ}) \\ &\quad SE = .0091 \\ &\quad R^2 = .9938\end{aligned}$$

This equation very clearly shows that Congress gave the mandatory requests from the Labor Department clear passage, while cutting requests for new or expanded programs significantly more.

All this means that since agencies have mandatory needs to present to Congress,

$$\text{REQUEST}_t \geq \text{APPROPRIATION}_{t-1}.$$

And since the mandatory elements of the request are honored, we now know that

$$\text{APPROPRIATION}_t \geq \text{APPROPRIATION}_{t-1}.$$

But because the requests for programmatic purposes are not always fully honored,

$$\text{APPROPRIATION}_t \leq \text{REQUEST}_t.$$

Thus we have been able to explain the budgetary relations (4), (5), and (6) for the Labor De-

[18] U.S., Congress, House, Appropriations Committee, "House Report 91–391," p. 7.
[19] U.S., Congress, House, Appropriations Committee, "House Report 90–1575," p. 7.
[20] U.S., Congress, Senate, Appropriations Committee, "Senate Report 90–1484," p. 3.
[21] U.S., Congress, Senate, Appropriations Committee, "Senate Report 90–469," p. 6.
[22] U.S., Congress, Senate, Appropriations Committee, "Senate Report 91–610," p. 13.

[23] Financing increases were combined with mandatory increases. I felt that this class of increases did not exhibit clear programmatic content but was "imposed" on the agencies and thus was closer to mandatory increases. Furthermore, since there were few such increases, where they were classified would have little impact on analysis.

partment. There is little reason to believe that Congress would act in a substantially different manner with regard to agencies in other departments, and so I feel that the mandatory/programmatic distinction would in general explain budgetary relations at the federal level.

There still remains the task of accounting for the marginality of the budget relations. The median percentage increases requested by the Labor agencies for mandatory items for fiscal years 1968 through 1972 were respectively 0.85 per cent, 4.05 per cent, 2.18 per cent, 1.63 per cent, and 8.61 per cent. For those same years the comparable figures for programmatic items were 2.51 per cent, 5.26 per cent, −0.25 per cent, 6.74 per cent, and 1.52 per cent. All of these are small percentages. Since the reasons for the marginality of the requests and the subsequent marginal appropriations cannot be explained by "gaming" considerations on the basis of high correlations, how can we account for the marginality?

While I have no definitive explanation for the marginality of budgetary changes based on the Labor Department experience, the source of mandatory increases offers a clue. Inflation, increases in the client population, within-grade pay increases, and other components of the mandatory increases are consequences of forces exogenous to the agency/Congress nexus. For the most part these external forces are tied to the state of the economy, which does not usually exhibit massive and radical changes in the short run. If the exogenous economic forces giving rise to the need for mandatory increases are slow moving, the demands for mandatory requests will also be small.

Why then is the programmatic component of the request usually small? It is not usually tied to economic forces. Furthermore, Fenno and Wildavsky[24] have detailed the programmatic drive of agency officials, a drive which unalloyed could hardly be called incrementalistic. Yet the requests presented to Congress generally are small. The explanation offered here is based on a theme presented by Crecine, who argues that external constraints restrain requests from being grossly expansionary.[25]

[24] Fenno, pp. 266–274 and Aaron Wildavsky, *The Politics of the Budgetary Process* (Boston: Little, Brown, 1964), pp. 18–31.

[25] "A Simulation of Municipal Budgeting: The Impact of Problem Environment," in William Coplin, *Simulation in the Study of Politics* (Chicago: Markham, 1968), pp. 119–122, 136–139, 144 and "Defense Budgeting: Organizational Adaptation to Environmental Constraints," in R. F. Byrne, A. Charnes, W. W. Cooper, O. A. Davis, and Dorothy Gilford, *Studies in Budgeting* (Amsterdam and London: North-Holland Publishing Company, 1971), pp. 218–222, 227–235.

The "great equation" binding budget makers demands that

EXPENDITURES

= REVENUES + BUDGET DEFICIT.

Since the size of a tolerable deficit is pretty well circumscribed, and since revenues will not increase appreciably from one year to the next, the amount that the President and Budget Bureau allow for *both* mandatory and programmatic increases is marginal compared to the total budget. These constraints at the macro level are transmitted down to each agency and are then manifested in the marginal increases requested by the individual agencies. Just how a given agency receives its particular allotment is not predicted by this line of argument but this approach explains the overall marginality of the budget relations.

Summary and Conclusions

In the social sciences incrementalism has been used in both descriptive and explanatory senses. For a phenomenon to be descriptively incremental, changes in the system have only to be marginally different from the status quo. In this regard budgeting has been well established as incremental. For a phenomenon to be explained by incrementalism, on the other hand, reasons must be given for the marginality of changes. In the literature, budgeting has not been successfully explained as incremental in this sense.

This paper analyzed the explanatory power of the theory of budgetary incrementalism presented in the prominent work of Davis, Dempster, and Wildavsky. By means of sensitivity analysis it was demonstrated that inferences to "gaming" explanations of budgetary incrementalism are not warranted on the bases of correlation analysis.

To explain budgetary incrementalism, recourse was made to concepts and variables explicit in the vocabulary of the budget process participants. The distinction between mandatory and incremental requests and the differential treatment of those two kinds of requests by Congress allowed good explanation of the incremental changes in requests and appropriations.

In particular, the inexorable but small mandatory request that was automatically granted was alone adequate to explain why requests always increased and why one year's appropriation slightly exceeded the previous year's appropriation. Fiscal constraints binding the government as a whole were called upon to explain the overall marginality of budget changes. Because of the isomorphism between concepts and variables used by budgetary actors and the theory presented in

the second part of the paper, this theory of budgetary incrementalism appears to be superior to Davis, Dempster, and Wildavsky's.

Because the mandatory request and congressional acquiesence to it can alone account for the descriptive incrementalism of the budget process, in further research attention must be paid to the programmatic portion of the budget. In a sense, this is the only part of the budget that is "variable." It is only in this component of the budget that the "politics" of the budgetary process can be found.

Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior*

PAUL ALLEN BECK
University of Pittsburgh

Introduction

Political parties have been the focus of perhaps as much empirical research as any other actors in the American political process. Yet for all this attention, no empirical theory attempts to explain the activities of the party organizations themselves. Such activities have been utilized frequently as independent variables in the analysis of voter behavior,[1] but only infrequently have they been treated as variables to be explained in their own right—that is, as dependent variables. At the same time it is commonly acknowledged, as a result of research on political machines, that parties are adaptive organisms—that their behavior should be viewed as a response to, rather than a cause of, the conditions or environmental circumstances which they face.[2]

This view of the activities of local party organizations as dependent variables will guide the research reported here. In the following pages, I shall develop a theoretical frame for analyzing the impact of environmental conditions on party activities and then test a number of hypotheses, many of which derive from the findings of previous studies, concerning the relationship of environment to party during the 1964 campaign. The principal goal of this research is to chart the path, both conceptually and empirically, toward an empirical theory of environment and party behavior.

A number of studies have analyzed the impact of the environment on the performance of party activities. Some of them have attempted to explain variations in party behavior by reference to the political features of the environment as reflected in inter-party competition. Crotty and Key found that parties in the South performed their activities better the more competitive they were with one another.[3] The relationships between competition and party behavior reported by Eldersveld for Wayne County, Michigan, however, diverge sharply from those in the South. When Eldersveld's two highest categories of performance are combined, each party performs better where it holds the competitive advantage.[4] The studies of Cutright and Putnam present yet a third set of empirical findings: no significant relationships between the political environment and party performance.[5] Certainly no clear picture of the impact of the political environment emerges from previous research.[6]

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[1] See Phillips Cutright, "Activities of Precinct Committeemen in Partisan and Non-partisan Communities," *Western Political Quarterly*, 17 (March, 1964), 93–108; Phillips Cutright, "Measuring the Impact of Local Party Activity on the General Election Vote," *Public Opinion Quarterly*, 27 (Fall, 1963), 372–386; Phillips Cutright and Peter H. Rossi, "Grass Roots Politicians and the Vote," *American Sociological Review*, 23 (April, 1958), 171–179; Daniel Katz and Samuel J. Eldersveld, "The Impact of Local Party Activities upon the Electorate," *Public Opinion Quarterly*, 25 (Spring, 1961), 1–24; Gerald H. Kramer, "The Effects of Precinct-Level Canvassing on Voter Behavior," *Public Opinion Quarterly*, 34 (Winter, 1970–1971), 560–572; Robert Putnam, "Political Attitudes and the Local Community," *American Political Science Review*, 60 (September, 1966), 640–654; and Peter H. Rossi and Phillips Cutright, "The Impact of Party Organization in an Industrial Setting," in *Community Political Systems*, ed. Morris Janowitz (Glencoe, Illinois: Free Press, 1961), pp. 81–116.

[2] This view of the party as a dependent variable underlies Robert K. Merton's classic discussion of the functions of the party machine. See *Social Theory and Social Structure* (New York: Free Press, 1968), pp. 126–127.

[3] William J. Crotty, "The Party Organization and Its Activities," in *Approaches to the Study of Party Organization*, ed. William J. Crotty (Boston: Allyn and Bacon, 1968), pp. 247–306; and V. O. Key, *Southern Politics in State and Nation* (New York: Alfred A. Knopf, 1949), pp. 386–405.

[4] Samuel J. Eldersveld, *Political Parties: A Behavioral Analysis* (Chicago: Rand McNally, 1964), p. 424.

[5] Phillips Cutright, "Measuring the Impact of Local Party Activity . . . ," pp. 372–386; and Robert Putnam, "Political Attitudes and the Local Community," p. 643.

[6] There are several methodological reasons why these research findings might be so contradictory: (1) both the independent and dependent variables were measured differently in each; (2) different types of party behavior were examined; (3) different levels of analysis were employed (some studies worked at the precinct level while others worked with counties); and (4) variations in the range of the political environment captured by the independent variable, party competition, were quite small in most cases. Whatever the reason, the resultant problem in cumulating these diverse findings into a theory of environment and party behavior underscores the need for truly comparative studies of party performance across a wide range of political environments.

Other studies of environment and party behavior, especially those of that form of party organization known as the "machine," have utilized demographic characteristics of the environment as the independent variables. Party machines have emerged in those areas which contain large concentrations of particularistic voters.[7] They differ from other party organizations in their overarching emphasis on the continuous provision of material benefits and services directly to constituents through local party leaders. When voters are near the subsistence level, their vote is readily exchanged for such rewards as the machine can offer, and more general appeals, in which the anticipated returns are less direct, are likely to be ineffective. As explained graphically by Scott:

> Poverty shortens a man's time horizon and maximizes the effectiveness of short-term material inducements. Quite rationally he is willing to accept a job, cash, or simply the promise of assistance when he needs it, in return for his vote and that of his family. Attachments to policy goals or to an ideology imply something of a future orientation as well as wide loyalties, while poverty discounts future gains and focuses unavoidably on the here and now.[8]

The fuel of the machine, in the past as well as in the present, is the wide range of services it provides for constituents—such material rewards as patronage jobs, legal assistance and protection, and cash to tide the family over during hard times. Performance of service activities alone, though, is rarely sufficient to ensure satisfactory levels of support on election day for even the strongest machine. In order to collect the debts which have been accumulated by continuous servicing of constituents, machine leaders place a heavy emphasis on voter mobilization activities. Performance of these activities is particularly important among the poor because they often lack the motivational force which propels the more affluent or educated voter to the polls. Both adequate servicing of constituents and mobilization, furthermore, depend upon a high level of organizational efficiency, especially at the precinct level "where the voters are."

The relationship between the nature of the constituency and party behavior implied in this description has become increasingly clear with the decline of the party machines. This decline has been traced to a fundamental alteration in the nature of the party's constituency:

> The specific inducements which the machine was organized to supply worked their "magic" on a smaller and smaller proportion of party workers and supporters. Instead, as businessmen and laborers each came to appreciate their broader, longer run interest as a sector of society, they increasingly required general legislation that met their interests in return for political support. Here and there a social context tailored to the machine style remained, but the machine either reconciled itself to the new loyalties—becoming less and less a machine in the process—or was the electoral victim of social change. Parties still continued to offer palpable inducements to voters but the new inducements were more typically embodied in general legislation, where previously they had been particularistic and often outside the law.[9]

That is, as the needs of voters have changed, the party organizations have turned away from service activities toward other kinds of vote-getting methods. Among these new methods has been an emphasis on persuading voters to back the party's candidates through the use of issue and personality appeals. At the same time, as the general level of education among voters grew, the organization could diminish its concentration on mobilizing its supporters. Many of them had become self-mobilized through their increased awareness of and interest in politics and through their internalization of the norms of democratic citizenship. While the activities of the party organization changed with the changing nature of the constituency, the need for a strong organization remained, but the vote-directed activities of that organization were transformed.

The state of our knowledge concerning the relationships between environment and party behavior may be summarized as follows: A number of empirical studies have examined the impact of the political environment (operationalized in terms of interparty competition) and their findings are sufficiently at variance with one another to preclude cumulation into a theory of political environment and party. On the other hand, a theory of the demographic environment and party behavior emerges quite clearly from the literature on machine politics, but this theory lacks the empirical grounding required of scientific theories.[10]

[7] See Talcott Parsons, *The Social System* (New York: Free Press, 1951), pp. 61–63, for the meaning of particularistic. My use of the term in the context of political machines is derived from James C. Scott, "Corruption, Machine Politics, and Political Change," *American Political Science Review*, 63 (December, 1969), 1145–1149.

[8] Scott, "Corruption, Machine Politics, and Political Change," p. 1150.

[9] *Ibid.*, pp. 1148–1149.

[10] A clear distinction should be made between the "theory" concerning the relationship of the demographic environment to party behavior and the "findings" concerning the relationship of the political environment to party behavior. The expectations embodied in the theory were derived from studies of party organizations, particularly the political machine, which, although highly insightful, were not systematically empirical. With the exception of the findings

Several reasons may be suggested for the lack of theory about political environment and the lack of empirical tests of the theory bearing on the demographic environment. Probably the most important reason is that serious problems of measurement equivalency arise in comparing party activities across environments.[11] If party behavior does not vary along a single dimension under different environmental conditions but varies instead in its emphasis on one dimension or another depending upon the environment, then a prerequisite to the comparative analysis of party behavior is the specification of the universe of party activities and the grouping of these activites along certain common behavioral dimensions. Such an approach has not been common in previous studies. A second reason involves the problems in observing party behavior *per se* even if certain common dimensions of performance can be specified. Party activity is a property which cannot be easily measured directly. Instead it must be aggregated from the behavior of numerous party activists. Such an aggregation is intrinsically more difficult than that for other "analytic" properties, such as median income or Democratic vote, because it requires extraordinarily detailed information of the kind difficult to obtain with even the most intensive survey. Finally, few studies of party behavior have viewed parties as dependent variables. As a consequence, little research has focused explicitly on the relationship between the kinds of activities performed by a local party organization and the political and demographic characteristics of the locale.

The principal objective of the research reported in the following pages is to provide empirical grounding for a theory of environment and party by examining the impact of environment on party behavior across the range of environmental conditions extant in the United States. The dependent variable in the analysis is the behavior of a national sample of county party organizations during the 1964 campaign. While many possible determinants of this behavior warrant attention, the analytical focus is restricted to the environmental factors which have been identified as important in previous research: the political and demographic environments. A second objective of this study is the specification of a conceptual frame for the comparative study of party behavior. Construction of such a frame requires both the theoretical identification of separate dimensions of party behavior and the measurement of these dimensions in such a manner as to make them equivalent across environments. It is to these two tasks that I now turn.

Measuring Party Behavior

To develop measures of party behavior which are equivalent across different environments, it is necessary to discard the notion that party activity is unidimensional and that a summary measure of party performance may be constructed by adding performance scores on diverse activities. It is much more reasonable to assume that the types of activities which the party organization performs will vary greatly depending upon the political and demographic conditions of the environment. If this assumption is valid, then party performance must be measured along several different dimensions.

A few studies of party activity have adopted this approach. In his research on party organization in North Carolina counties, for example, Crotty specified four dimensions of activity. Three of the four—intercampaign, campaign, and election-day activities—were differentiated from one another according to their timing. The fourth —party organization—was singled out because it had an important impact on performance of voter-oriented activities.[12] Eldersveld identified somewhat similar types—organization, intercampaign, and campaign—in addition to a fourth type which he called "adaptive."[13] For the purposes of my research, I have differentiated party activities into four general types or dimensions—organizational, service, mobilization, and persuasion. This typology, in contrast to those of either Crotty or Eldersveld, organizes activities according to a single criterion—their function or purpose. Use of this criterion derives from the theoretical focus of my study: it is assumed that the major impact of the environment will be exerted on the purposes of activities rather than their timing. A more detailed discussion of each dimension follows.

The political party is an organization and may be expected, like all organizations, to engage in activities designed to maintain its organizational vitality. It must select leaders, provide for a staff, hold meetings, keep records, etc., if it is to be successful in pursuing its goals. The organizational dimension of party behavior refers to those activities which are directly concerned with organizational maintenance.[14]

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reported in the Key study, on the other hand, the discussion of the political environment's impact relies on systematically empirical studies.

[11] See Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley, 1970), pp. 106–110, for an excellent discussion of the problems of establishing equivalency in comparative research.

[12] Crotty, "The Party Organization and Its Activities," pp. 254–257 and 269–273.

[13] Eldersveld, *Political Parties*, p. 347.

[14] For a good example of the operationalization of this dimension, see Eldersveld, pp. 410–433.

Political scientists have been interested in political parties not because they are organizations, but because parties have an explicitly political *raison d'être*—to win elections and, thereby, control the reins of government. Most of the effort of the party organization is directed toward fulfillment of this goal, and the three remaining dimensions of party behavior reflect the three different approaches to winning voter support.

Service activities are performed in order to build allegiances among voters which can be converted into votes at election time. In this sense, then, they are future oriented activities. The emphasis on the service dimension above all others and the allegedly superior performance of service activities are the hallmarks of American party machines. It would be a mistake, however, to assume that an emphasis on the performance of service activities is a good strategy in all types of political and, especially, demographic environments.[15]

Mobilization and persuasion activities are even more directly concerned with winning elections since, unlike either organizational or service activities, they are practiced heavily during campaigns but very little during the intercampaign period. The division of campaign activities into those concerned with the mobilization of latent support and those concerned with persuading voters to support the party and its candidates has been adopted in several studies of party behavior.[16] Mobilization has been found to be the more successful of the two activities in terms of winning votes, but this does not imply that persuasion is unimportant.[17] The specific activities which fall into the domain of one or the other of these two dimensions are many and seem to be bounded only by the imagination of party leaders.[18]

Whether the various party activities can be represented by these four dimensions is an empirical question. The assumption which underlies my typology is that there is a correspondence between the conceptualization of the analyst and the behavior, even if not the theory, of local party leaders. The extent to which this assumption is valid may be assessed by examining the interrelationships of performance scores for the various activities. Ideally, if this *a priori* differentiation of party behavior into four different dimensions has any validity, party activities should fall into four clusters reflecting organization, service, mobilization, and persuasion. If the data are found to support this assumption, then a measure of each dimension may be fashioned directly from the performance scores of those activities which cluster on it.

The performance scores for specific activities were derived from reports collected by the Survey Research Center at the University of Michigan on the performance of county party organizations during the 1964 campaign. Democratic and Republican chairmen from a national sample of counties were the sources of these reports.[19]

[15] Nonetheless, this assumption underlies some research on party behavior, particularly that which implicitly views the political machine as the "ideal" party organization. For example, see Cutright, "Activities of Precinct Committeemen." In this comparative study of two cities, Cutright uses indicators which tap only the service dimension of party activity—(1) contacts with voters, (2) requests for aid (3) responsiveness to requests, and (4) whether the precinct leader held a patronage job. While a measure of party activity based exclusively on such indicators may reflect party behavior in a "machine" city such as Gary (Cutright's partisan city), it is surely not generalizable to a non-machine and non-partisan city. In other words, the political machine is only one form of party organization and can not serve as the model against which the activities of all party organizations should be measured.

[16] Raymond Wolfinger, "The Influence of Precinct Work on Voting Behavior," *Public Opinion Quarterly*, 27 (Fall, 1963), 387–398; Lewis Froman, "A Realistic Approach to Campaign Strategies and Tactics," *The Electoral Process*, ed. M. Kent Jennings and Harmon Zeigler (Englewood Cliffs, New Jersey: Prentice-Hall, 1966), pp. 1–19; and Kramer, "The Effects of Precinct-Level Canvassing on Voter Behavior."

[17] See Froman, "A Realistic Approach to Campaign Strategies and Tactics"; Bernard Berelson, Paul Lazarsfeld, and William McPhee, *Voting* (Chicago: University of Chicago Press, 1966), p. 171; and Kramer, "The Effects of Precinct-Level Canvassing on Voter Behavior," p. 572.

[18] Richard T. Frost lists a number of different campaign activities which were performed by party organizations in eight New Jersey counties. See his "Stability and Change in Local Politics," *Public Opinion Quarterly*, 25 (Summer, 1961), 221–235.

[19] The county chairmen data were collected as a supplement to the Survey Research Center's 1964 election study and were drawn from both parties in all counties where voters were interviewed. Questionnaires were mailed to the chairman of each party in 129 counties and the District of Columbia. The return rate from these mailings was unusually high (more than 90 per cent) and was not noticeably biased in favor of any particular type of county environment. For most of the analyses which follow, the data were weighted by county population to ensure representativeness of the findings. The District of Columbia was deleted from all analysis.

The use of the county as the level of analysis and county party chairmen as the source for perceptions of party activity seems optimal for my present purposes. Because of the decentralization of American party organizations, several levels of analysis are appropriate to the study of environment and party. Most previous studies have utilized either the precinct or the county. Of the two, it seems to me that the county chairmen may be freer to respond to environmental conditions and more knowl-

Among other things, these chairmen were asked to estimate what percentage of the precincts under their jurisdiction had chairmen and to assess their party's performance of six standard campaign activities:

(1) distributing literature (Lit)
(2) transporting voters to the polls on election day (Polls)
(3) holding candidate affairs (Affairs)
(4) raising money (Money)
(5) publicizing candidates through the mass media (Media)
(6) conducting registration drives (Drives)

They were also asked to rank order these six activities in terms of importance.

The constraints of a secondary source unfortunately preclude complete empirical testing of my theory concerning the multidimensionality of party activity. The original questionnaire contained no items which could be identified with the service dimension *a priori* and only one item—the percentage of precincts with chairmen—which seems to reflect performance along the organizational dimension. As a consequence, no attempt has been made to test for the existence of a separate service dimension, and the organizational dimension had to be measured by the single item which bore some relation to it. In order to denote the restricted nature of this measure of organizational performance, the variable will be referred to as "precinct organization" in subsequent analysis.

The theory-testing injunction was followed, on the other hand, where the mobilization and persuasion dimensions were concerned. On the surface, the six standard campaign activities seem to fall into two distinct groups. Two of the activities, Polls and Drives, are clearly identifiable as mobilization activities in terms of their function. The four others—Lit, Affairs, Media, and Money—seem to be activities which are performed with the purpose of persuading voters. Money merits inclusion as a persuasion activity, in theory at least, because it is a prerequisite to certain other persuasion activities, particularly the use of the mass media.

The assumption that these activities form two distinct clusters empirically was tested using the technique of factor analysis. The results of this factor analysis are presented in Table 1 with factor loadings above .50 underlined for emphasis.[20]

Table 1. Dimensions of Campaign Performance[a]

Activity	Factor I (Mobilization)	Factor II (Persuasion)	Variance Explained by Both Factors
Lit	−.43	.01	.19
Polls	.78	.01	.61
Affairs	.06	.59	.35
Money	.11	.68	.48
Media	−.21	.68	.51
Drives	.74	.01	.55
Variance Explained by Factor	.24	.21	.45

(Number of cases = 234)

[a] Each activity was measured as a trichotomous variable, coded jointly from the chairman's evaluation of performance and, to correct for overestimation, the priority ceded to the activity.

edgeable of party performance at and below their level than any other party leaders. The use of judges' perceptions to measure performance of party activities can not be justified as easily. The inherent difficulties in measuring party performance directly have forced all previous studies to rely upon indirect measurements. Two types of these indirect measures have been utilized. Most studies have depended upon estimates by party leaders, usually precinct captains or county chairmen depending upon the level of analysis. An alternative method has been to compile estimates of party performance by aggregating voter perceptions of party contact from mass samples. (See Putnam, "Political Attitudes and the Local Community," p. 643; and see Kramer, "The Effects of Precinct-Level Canvassing on Voter Behavior," pp. 564–565.) The estimates of party leaders, while still indirect, are clearly preferable to those aggregated from a sample of voters. Not only do they capture a broader range of party behavior, but they are less contaminated by errors caused by selective perception and weak memories or by large sampling variances. Nonetheless, the use of an indirect measure is not wholly satisfactory and more work needs to focus on the development of measures of party performance.

[20] The Varimax solution is presented in Table 1. An eigenvalue of 1.00 was used as the cutoff for the admission of additional factors. This criterion is suggested by R. J. Rummel, *Applied Factor Analysis* (Evanston, Illinois: Northwestern University Press, 1970), pp. 362–364. The matrix which was factored to produce this solution contained coefficients based on equally weighted counties in both the South and the North. This weighting procedure was followed to produce a solution which reflected the behavior of county chairmen as nearly as possible. The results remain substantially the same when the counties are weighted by population size.

Precinct Organization was not included in the factor analysis presented in Table 1 because I expected it to form a separate, yet correlated, dimension of activity. The positive correlations between it and all campaign activities but literature distribution support this expectation. Furthermore, a two-factor Varimax solution for all seven measures of party activity virtually replicates Table 1 and adds Precinct Organization as an important item to each factor. While a three-factor oblique solution seems warranted given my expectations, the

That the solution produced two factors and that the items identified as *a priori* similar were found to load on these factors substantially as expected supports my theory that there are two dimensions of campaign activity. Factor I may be labeled "mobilization" and Factor II "persuasion." Taken together, these factors explain 45 per cent of the total item variance—not a large amount but certainly satisfactory, given the measurement unreliability expected with indirect measures of party performance. Had there been no underlying structure in these activities, the first two factors would have explained only 33 per cent of the item variance, and each would have contained only one item with a high loading.

Only the loading of the literature distribution item (Lit) fails to conform to expectations in that it is not associated with the practice of the other persuasion activities. As expected, however, it is negatively associated with mobilization activities. There is no obvious explanation for this anomalous result, and it clouds the empirical separation of mobilization and persuasion activities. Since the data show that this activity is performed well in almost all counties, though, one could surmise that it is perhaps so universal that it does not constitute an integral part of any campaign strategy.

The results of this factor analysis are important because they provide some empirical support for the theory that there are independent persuasion and mobilization dimensions of campaign performance. These results serve also as the empirical foundations for measures of the two dimensions. If the squared loadings of the important (underlined) items along each dimension are utilized as weights, party performance along each dimension may be represented as the weighted sum of the performance scores on those activities central to the dimension.[21] This factor scoring procedure yields two indices of party performance—Mobilization and Persuasion. The third index of party performance constructed for use in subsequent analysis, Precinct Organization, reflects simply the percentage of precincts in the county staffed with a chairman. Party performance in each county, of course, is measured separately for the two parties.

results of this operation are meaningless because too little variance is left to be explained by the additional factors. More items, and particularly more items which reflect organizational activity, are required before a meaningful three-factor oblique solution can be extracted.

[21] This procedure for calculating factor scores, called the composite method, uses only those items with the highest loadings on the factor in scoring that factor; the factor loadings used here have been underlined. See Rummel, *Applied Factor Analysis*, pp. 441–442, for a discussion of the composite method.

Measuring the County Environment

While there are many possible determinants of party behavior, my analysis is restricted to the two most frequently cited in studies of party activities—the political and demographic environments. The task of this section of the paper is to provide operational meaning for these two environmental conditions.

The political environment. At the conceptual level, it is easy to imagine most of the citizens in an area divided into two partisan camps, with a smaller number of individuals generally unwilling to commit themselves to the partisan fray. The political environment refers to this division of partisan strength. Were it not for the interstate differences in requirements for party declarations at registration time and the unwillingness of many partisans to state a preference for the public record, a direct count of registration rolls might provide an excellent estimate of the political environment. One possible alternative, to derive estimates of this partisan division from a voter survey, is highly impractical because of its cost. Another possible alternative is to measure the political environment from the voting record for several important offices over a relatively long period of time. This is the alternative adopted in this study. Not only are data on elections the only information on the distribution of partisanship readily available at the county level of analysis, but also these election results are perceived as meaningful by party strategists.

The measure of the political environment developed for this study is constructed from the popular vote for three offices—President, United States Representative, and Governor—from 1946 to 1962.[22] Unlike the standard measures of interparty competition, this measure has been constructed so as to estimate the partisan division beyond the sixteen-year interval (see Appendix I). The measure is titled Democratic Concentration because it ranges from a low of no Democrats to a high of all Democrats.

In order to test some of the hypotheses about environment and party, however, another measure of the political environment is required to reflect deviations in either direction from an equal

[22] This time base is long enough to smooth out the irregularities which may result from the operation of short-term forces, yet not so long that it overlaps two electoral eras. (It would have had to extend back to 1930 to cross into another electoral era.) The offices chosen represent three of the four offices for which county-level data are readily available and are diverse enough so that local idiosyncrasies in voting for a particular office will not distort the measure of the general pattern of partisan division.

partisan division. Such a measure, called Competitiveness to denote absolute rather than pro-Democratic or pro-Republican competition, is constructed by simply folding the Democratic concentration measure at its mid-point so that the ends of the continuum are collapsed upon one another. The measure is scored so that its maximum value will be attained when there is an equal division of partisans.

The demographic environment. Data from the 1960 Census are utilized to measure the demographic characteristics of the population in each county. Following the direction of Scott, the concentration of voters with particularistic orientations toward politics is deemed to be most relevant to the behavior of local party organizations.[23] Such orientations are found among poor and dependent voters and those to whom parochial group loyalties are important in voting behavior. Thus, those variables in the Census which reflect the level of poverty in the county and the concentration of parochially oriented groups are utilized to measure the demographic environment. Three variables are indicators of poverty levels—median income, median education, and median home value. Two other variables seem to reflect, again *a priori*, the relative concentrations of groups which have exhibited loyalty to candidates from their groups in their voting behavior—the percentages of nonwhites and foreign born. Both groups have been counted among the clients of urban political machines. Two other county demographic characteristics—percentage urban and percentage rural—are included because they too are known to be strongly related to the concentrations of particularistic voters. (These two variables are not opposites. That is, while they are negatively correlated with one another, that correlation is far from perfect.)

That these seven demographic variables reflect some underlying traits is clear from their high intercorrelations. In order to identify clearly the underlying traits or dimensions, the matrix of intercorrelations was factor analyzed for southern and nonsouthern counties separately.[24] The Varimax solution is presented in Table 2 with factor loadings above .70 underlined for greater emphasis.[25] Both solutions contain two similar factors or underlying dimensions, and the variance in the items explained by these two factors exceeds 70 per cent in both regions. Only 29 per cent of the variance would have been explained had the items been independent.

Table 2. Dimensions of the Demographic Environment

County Characteristic	Factor I	Factor II	Variance Explained by Both Factors
	Northern Counties (Parochialism)	(Poverty)	
Med Income	.34	−.87	.80
Med Home Value	.54	−.31	.38
Med Education	−.15	−.92	.87
% Foreign Born	.78	−.14	.63
% Nonwhite	.83	.10	.70
% Urban	.70	−.59	.84
% Rural	−.57	.64	.74
Variance Explained by Factor	.37	.35	.72
	(Number of cases = 98)		
	Southern Counties (Nonwhite Concentration)	(Poverty)	
Med Income	−.28	−.85	.80
Med Home Value	.28	−.78	.67
Med Education	−.45	−.84	.90
% Foreign Born	−.05	−.69	.48
% Nonwhite	.95	.04	.90
% Urban	−.06	−.90	.81
% Rural	.06	.89	.79
Variance Explained by Factor	.18	.58	.76
	(Number of cases = 31)		

Labeling these factors is difficult because they reflect the clustering of county, not individual, characteristics. Nonetheless, the two-factor solution in each region fits remarkably well the notion that there are two distinct dimensions of particularism—poverty and parochialism—if regional differences are taken into account. In the northern counties, Factor I may be labeled Parochialism because of the high loading of minority group concentrations upon it, and Factor II may be labeled Poverty because of the high loadings of the

[23] Scott, "Corruption, Machine Politics, and Political Change," pp. 1145–1149

[24] This factor analysis was performed in exactly the same manner as that presented in Table 1, except that it was done for the two regions separately. The population compositions of the southern and nonsouthern counties were too different to allow for strictly equivalent measures and a meaningful general factor solution. The counties were weighted according to size in calculating correlation coefficients so that the results would be representative of the universe of county voter environments.

[25] The selection of .70 as a cutoff point for this table in contrast to the .50 used in Table 1 is somewhat arbitrary, but it was felt that this level captured a more meaningful factor structure than the lower level would have.

indicators of poverty/affluence and education on it. In the southern counties, Factor II clearly warrants the title of Poverty. Factor I, on the other hand, is dominated only by the percentage of nonwhites and reflects one of the unique regional characteristics of the South—the heavy concentration of blacks in the old plantation areas called the "black belt." The style of politics in black-belt counties has been oriented towards maintaining white supremacy for almost one hundred years and has been accompanied by a highly restricted "whites only" electorate. While there is a large concentration of parochially oriented blacks (or, at least, potentially so in 1964) in these areas, local party organizations pay no attention to them because they are disenfranchised. Political circumstances in the South, then, lead to different expectations concerning the relationship between the concentration of parochially oriented blacks and party behavior. To reflect the political facts of southern politics, Factor I in this region will be labeled Nonwhite Concentration instead of Parochialism. Two measures of the demographic environment for each region are constructed from the results of the factor analysis by weighting the performance scores on the important (underlined) factors by the square of their factor loadings following the same factor scoring technique discussed above.[26]

Summary. The independent variables in this study —the political and the demographic environments —are operationalized as described in this section. The political environment is measured so as to reflect the division of partisan strength in each county prior to the 1964 election. The demographic environment is measured along two dimensions in the North and South separately. One dimension, Poverty, had the same meaning in both regions. The second dimension, Parochialism in the North and Nonwhite Concentration in the South, is based on similar items in the two regions but requires nominal differentiation because of the unique features of southern politics.

The Research Hypotheses

A number of propositions concerning the impact of the environment on party behavior were derived from the research reviewed in the introduction. The principal task of this section is to reformulate these propositions into hypotheses which can be tested using the measures developed in previous pages.

The research on the relationships of political environment to party behavior, most of it empirical, provides some evidence for each of two contrasting hypotheses:

(1) The more equal the division of partisans in the constituency (i.e., the more competitive the constituency), the better each party will perform its activities.[27]
(2) The more the division of partisans in the constituency favors a party, the better that party will perform its activities.[28]

Each of these hypotheses implies the null hypothesis that no significant relationship exists between the political characteristics of the environment and party performance. Empirical support for this hypothesis too can be found in previous research.[29] Many of these studies fail to recognize the multidimensional nature of party behavior, implicitly assuming that their results can be generalized to include any type of party activity. In my testing of hypotheses derived from the studies, however, each dimension of party behavior has been examined separately so that empirical evidence can be brought directly to bear on this assumption.

Previous discussions of the relationships of the demographic environment to party behavior yield clear expectations concerning the impact of the demographic environment on each dimension of party behavior. These expectations may be formulated into the following hypotheses:

(3) There is no relationship between the demographic characteristics of the environment and performance of organizational maintenance activities (i.e., organizational maintenance is important in all environments).
(4) The greater the concentration of particularistic voters in a constituency, the better each party will perform service activities.
(5) The greater the concentration of particularistic voters in a constituency, the better each party will perform mobilization activities.
(6) The lesser the concentration of particularistic voters in a constituency, the better each party will perform persuasion activities.

These expectations are derived from what may be termed a "theory" of machine politics and have not been tested empirically in a systematic fashion previously. In contrast, hypotheses one

[26] These measures of the characteristics of county constituencies are undeniably crude. In an attempt to estimate the concentration of voters whose orientations to politics are what has been described as particularistic, I have been forced to use proxy measures. Instead of information on the distribution of voter orientations, I have used information on the distribution of voters who are likely, according to "machine theory," to have particularistic orientations —the concentration of minority group members and the median levels of "poverty." A good deal of measurement error must be anticipated in such measures.

[27] Crotty, "The Party Organization and Its Activities," and Key, *Southern Politics*, pp. 386–405.

[28] Eldersveld, *Political Parties*, p. 424.

[29] Cutright, "Measuring the Impact of Local Party Activity," and Putnam, "Political Attitudes and the Local Community."

and two are formulated from the research findings of previous studies—most of which articulate no general theory of environment and party.

Six major hypotheses have been specified for empirical testing. If the first two are expanded into hypotheses relating to each dimension of party behavior, then the total increases to twelve. Three of these hypotheses, unfortunately, cannot be tested with the data from my study because no information on service activities was solicited from county chairmen during the 1964 campaign. The tests of the three hypotheses involving the organizational dimension, furthermore, are to be regarded as only partial because of the restricted measure of this activity dimension. Even though not all hypotheses specified above will be subjected to empirical testing, it is hoped that specification of all of them will guide future research on environment and party. The remainder of the paper is devoted to the empirical examination of the nine hypotheses, including the ones involving precinct organization as the dependent variable, for which both the independent and dependent variables are measured.

Environment and Party Performance: The Empirical Patterns

The empirical analysis reported in this section of the paper is focused on the interrelationships between the three dependent variables—precinct organization, mobilization, and persuasion—and the four independent variables—Democratic Concentration, Competitiveness, Poverty, and Parochialism (or Nonwhite Concentration in the South).

This analysis is conducted for northern and southern counties separately to allow comparisons between what are generally recognized as two distinct political regions.[30] It is also conducted separately for the Democratic and Republican party organizations because the different coalitional bases of the two parties lead one to expect quite different responses to the same environment, particularly where demographic characteristics are concerned.[31]

Northern Counties

Table 3 contains the product-moment correlations between the four environmental characteristics and each of the three dimensions of party performance for northern counties. Three measures are presented for each dimension of party behavior—one for each party and a "relative" measure which reflects the performance differences between the parties. (Relative performance is measured by subtracting the Republican performance score from that of the Democrats and ranges from a high of maximum Democratic advantage to a low of maximum Republican advantage.) All coefficients of .20 and above are significant at or beyond the .05 level and are underlined for emphasis.[32]

Precinct organization. Of all the possible relationships between the different environmental variables and precinct organizational activities for each party, only two attain significance, and both of these involve Republican levels of performance. In support of hypothesis 3, there is no evidence that the demographic characteristics of the environment affect precinct organization for either party. On the other hand, both Competitiveness and Democratic Concentration have clear and strong impact on Republican performance. The more competitive the county, the more precincts have been staffed by the Republicans. The higher the relative concentration of Democratic supporters in the county, the fewer precincts have been staffed by the Republicans. By contrast, the parallel relationships for the Democrats approach zero.

Even though Competitiveness and Democratic Concentration are correlated at −.47 in the northern counties, the hypotheses involving them are sufficiently contradictory to one another that only one of them can be valid. Support for the other is based on a spurious relationship. This spuriousness can be identified through the use of partial correlations involving both independent variables and the dependent variable at the same

[30] Parallel analyses of the two regions are required for two methodological reasons as well as the substantive one. First, important regional differences in the clustering of demographic features have precluded a factorial structure which is common to both regions. Second, the nationwide distribution of the political environment variable is bimodal and, as a result, seriously violates the normality assumption underlying correlation analysis. The regionally-specific distributions of this variable, however, are close enough to normality to satisfy the assumption. The South includes all the states of the Old Confederacy: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

[31] Three additional points require elaboration prior to this analysis. First, party performance was measured for 1964 and the environment was measured prior to 1964 so that party performance could be used as a dependent variable. Second, the hypothesized relationships between the environment and the "dependent" activity variables are linear and have been tested with a measure of linear association. Examination of the bivariate scatterplots uncovers no theoretically meaningful departures from linearity. Third, since this is an analysis which is based on data from the 1964 election, generalizations to other years should be made with caution.

[32] Some criterion is needed to differentiate between those correlations which are meaningful and those which are not. Since a high probability that the relationship has not occurred by chance seems a prerequisite for meaningfulness, significance at or beyond the .05 level will be the criterion used in the analysis.

Table 3. Environment and Party Performance, North[a]

	Political Environment		Demographic Environment	
	Competitiveness	Democratic Concentration	Parochialism	Poverty
Precinct Organization				
Democrats	.00	.04	−.06	.16
Republicans	.43	−.56	−.05	−.11
Relative	−.31	.47	−.01	.22
Mobilization				
Democrats	−.06	.22	.35	−.18
Republicans	.25	−.07	.26	−.23
Relative	−.25	.27	.06	.05
Persuasion				
Democrats	.20	−.27	−.24	.01
Republicans	.15	−.25	−.30	.26
Relative	−.01	−.06	−.01	−.16

(Number of cases = 83 for Democrats, 96 for Republicans, and 81 for Relative)

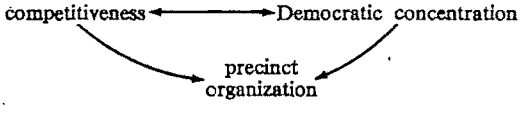
[a] See Appendix II for partial correlations cited in text.

time.[33] The partial correlation between Competitiveness and Republican Precinct Organization controlling for Democratic Concentration is insignificant. On the other hand, the partial correlation between Democratic Concentration and Republican Precinct Organization controlling for Competitiveness is a robust −.41. The inference from these results is clear: *Precinct Organization, at least for the Republicans, is performed better the greater the electoral strength of that party in the county.* That is, for the GOP, strength begets strength.

To the extent to which precinct organization is critical to a party's electoral performance, however, it is the relative edge that one party enjoys over the other which is important. This edge, measured as Relative Precinct Organization, is related to the Poverty dimension of the demographic environment as well as to the two measures of political environment. Additional analysis shows that both the Relative Organization-Competitiveness and Relative Organization-Poverty relationships are spurious. Thus, more empirical support is found for the hypothesis that a party's organization effort is predicated upon its electoral strength. This finding implies that the minority party must overcome two hurdles if it is to become a majority. It lags behind in both electoral support and organizational activity.

Mobilization. Unlike the case for precinct organization, both the political and demographic environments appear to influence performance of mobilization activities. The impact of the demographic environment is basically the same for both parties. The greater the concentration of parochial voters in the county, the higher are Democratic and Republican mobilization performance levels. Where this aspect of demographic environment is concerned, then, hypothesis 5 receives empirical support. The relationships of the Poverty dimension with Mobilization, on the other hand, are opposite to those predicted by the hypothesis—a pattern that will be found rather consistently throughout the analysis and may possibly be explained by the fact that urban counties, even though they contain large numbers of poor and uneducated voters who seem to be "machine" clients, rank relatively low on the Poverty dimension (rural counties rank higher). One conclusion appears amply justified: *Mobilization activities are performed better for both parties*

[33] This test for spuriousness assumes the following causal ordering among the variables:



Since competitiveness and Democratic concentration are highly intercorrelated, as are many other pairs of independent variables in this study, it is clearly possible that the relationships of either of them to the dependent variable may really reflect the influence of the other on both. Thus, the partial correlation coefficients must be used to sort out the influences of the independent variables. When the original zero-order correlation is substantially reduced by partialing, one may conclude that it is the control variable which is responsible for the bulk of the relationship.

the higher the concentration of parochially oriented voters.

This interparty similarity disappears when measures of the political environment are used, and a pattern appears which clarifies even more the responses of the two parties to environmental conditions. The Democrats are found to perform mobilization activities better as their electoral strength increases, while the Republicans perform them better as the competitiveness of the county increases. Democratic Concentration and Parochialism are correlated at .26, though, and partial correlation coefficients must be utilized to disentangle environmental effects for the Democrats. When this is completed, the relationship between Democratic Concentration and Mobilization performance by the Democrats approaches zero, while the partial correlation between Parochialism and Democratic Mobilization remains a high .31. Democratic Mobilization efforts seem to be affected by the concentration of parochial voters only. The relationships between Republican levels of Mobilization and both Competitiveness and Parochialism, on the other hand, remain undisturbed after partials are employed. Republicans, it would seem, make mobilization of parochials conditional upon the existence of close competition.

Persuasion. The environment has had somewhat different impact on each party's performance of precinct organizational and mobilization activities. This is not the case where persuasion is concerned. Not only are the relationships between each environmental variable and the performance of each party in similar directions in all cases, but they are of almost the same magnitudes in three of the four comparisons. Both parties seem to perform persuasion activities less as the electorate becomes more Democratic, as the county becomes more one-party dominated, and as the concentration of parochial voters increases. The Republicans also perform significantly better in the poor counties (which, it should be remembered, are largely nonurban).

Taken together, these findings should be read so as to support the aspects of hypothesis 6 which relate to parochialism—*performance of persuasion activities is low where there are large concentrations of group-oriented voters.* This interpretation is reinforced by examination of the first-order partials. After controls have been imposed, both parties still eschew persuasion activities in those counties where the concentrations of parochial voters and Democrats are high, emphasizing these kinds of activities in rural, Republican-dominated counties. These are the counties where, traditionally, the anti-partisan political culture has been the strongest. It seems reasonable that both parties should emphasize persuasion activities there. Great care must be taken lest this argument be overstated. After all, the coefficients are not high, and in most cases they barely pass the cutoff point for significance. What is most striking about these findings is that they run in the same direction for both parties. Where the political climate seems to encourage persuasive appeals, both parties will respond by relying upon them.

Southern Counties

Table 4 contains the correlations between the four environmental characteristics and each of the three dimensions of party performance for the southern counties. As before three measures are to be presented for each dimension of party behavior. All coefficients of .35 and above are significant at or beyond the .05 level and are underlined for emphasis.

Precinct organization. Unlike the case in the North, the Democrats and the Republicans respond similarly to the environment in their performance of precinct organizational activities. Each comparable pair of coefficients goes in the same direction with differences only in magnitude. Both parties are better organized in the competitive counties of the South and show their lowest levels of organization in those counties with large nonwhite concentrations, where Democratic hegemony has traditionally gone unchallenged. None of the relationships between environment and party performance attains significance for the Democrats, however, although the one involving Competitiveness is near enough to the threshold to warrant attention. By contrast, both features of the political environment and Nonwhite Concentration are significantly related to Republican performance levels.

The three environmental variables which are strongly related to Republican activity are themselves highly intercorrelated,[34] and their contributions must be disentangled through the use of partial correlations if one is to get a clear picture of the impact of the environment on Republican precinct organization. When the partials for each triad of variables are computed, both Competitiveness and Nonwhite Concentration are found to contribute to organizational performance.[35] Re-

[34] Competitiveness and Democratic concentration are almost perfectly correlated at −.94, and this confounds substantive interpretations as will be seen later. The relationship between nonwhite concentration and competitiveness is −.64, while that between nonwhite concentration and Democratic concentration is .63.

[35] The scarcity of counties with pro-Republican political environments is clearly reflected in the near-perfect correlation between Democratic concentration

Table 4. Environment and Party Performance, South[a]

	Political Environment		Demographic Environment	
	Competitiveness	Democratic Concentration	Nonwhite Concentration	Poverty
Precinct Organization				
Democrats	.32	−.24	−.24	−.26
Republicans	.72	−.69	−.71	−.34
Relative	−.27	.30	.32	.01
Mobilization				
Democrats	−.25	.25	.04	.20
Republicans	.31	−.19	−.41	.01
Relative	−.42	.33	.32	.14
Persuasion				
Democrats	.15	.06	−.20	−.16
Republicans	.26	−.23	−.13	−.11
Relative	−.11	.20	.00	.03

(Number of cases = 26 for Democrats, 29 for Republicans, and 24 for Relative)

[a] See Appendix II for partial correlations cited in text.

publicans organize better as competition increases and the concentration of nonwhites decreases. Both findings seem consistent with the conventional wisdom concerning southern politics. There is little incentive for Republicans to be well organized in the precincts in counties where they never receive more than minimal support and where such organization might threaten the "white monopolies" on politics in the black-belt counties.

The pattern of environmental effects becomes more complex, as well as more substantively interesting, when relative performance levels are examined. Democratic performance advantages are found where Democratic electoral strength is high and where nonwhites comprise a substantial portion of the population. Thus, *even though Democratic performance levels are lowest in the "black-belt" counties of the South, they are high enough to surpass the meager performance of the Republicans.* Democratic electoral monopolies in these counties are reinforced by a virtual monopoly on organizational activity. This Democratic performance advantage is overcome by increases in Republican activity as the counties become more competitive. Even so, the Democrats sustain a higher average level of performance in the region with 71 per cent of the precincts organized to 63 per cent for the Republicans.

and competitiveness. Because of this, scores on either variable reflect the other as well. Any inferences made regarding the relative importance of the two political variables rest on those few counties which are pro-Republican. These inferences must be regarded as highly tentative.

Mobilization. The patterns of mobilization performance in the South differ greatly from those in the North. That only one of the coefficients attains significance, the one relating Nonwhite Concentration to Republican performance, underlines the uniqueness of the South as a political region. One would expect mobilization activities to be performed most extensively, *ceteris paribus*, in those black-belt counties with high concentrations of parochially oriented black voters given the findings for the northern counties. Yet other things are clearly not equal. These counties, in response to the threat to white supremacy posed by a large black population, have had the most restricted electorates of any counties in the nation since the Civil War. The last thing that party leaders there want to do is mobilize voters, because in many cases this might mean the mobilization of black voters. Thus, the strong differences in the political cultures of the two regions can be cited to explain the relationship between demographic environment and party performance of mobilization activities in the South.

If attention is focused on relative performance levels, the regional differences appear much less sharp. *The more competitive the county, the greater the Republican edge in mobilization performance.* Again the implication of these findings is not completely clear because of the skewed nature of the political environment continuum in the South. The other coefficients are also consistent with those in the North even though they do not attain the magnitude necessary for significance. Democrats perform mobilization activities better

Conclusion

This research has been designed to make two principal contributions to the study of political parties. First, party behavior was measured within a theoretical framework which recognized the multidimensionality of party activities. Unlike many earlier measures of party behavior, these measures were equivalent across environments and, as a result, were well-suited for use as dependent variables in an analysis of the impact of the environment on party behavior. It is hoped that this new approach will foster additional research on determinants of party activity. Second, the impact of certain political and demographic characteristics on performance along three dimensions of party behavior—Precinct Organization, Mobilization, and Persuasion—was examined empirically using data on county party organizations in 1964. The measures of these dimensions can and should be improved, and other campaigns should be analyzed as well. Nonetheless, the research reported in this article provides empirical evidence on the relationship between environment and party and may serve as a guide, as well as a benchmark, for further exploration.

It is unfortunate that, for all the attention paid to political parties, so few researchers have focused on the interaction of a party organization and its environment. Parties are vote-oriented institutions in modern democracies and, as a result, must reflect the nature of their constituencies if they are to survive. One index of political change may well be the behavior of political parties. Not only may counties or nations be arrayed across a range of environmental conditions at any point in time, but both may be arrayed along a temporal dimension as well. In fact, in the continuing absence of longitudinal data on party behavior, variation at any one point in time can be used as a surrogate for temporal variation by assuming that areas are at different stages of development along a single continuum.[37]

The twentieth century has witnessed massive changes in party organizations. Political machines have declined and, indeed, have been pronounced dead in most American cities. More recently, a new style of political campaigning has emerged with a heavy emphasis on the "marketing" of candidates via the mass media—a technique with the clear purpose of persuasion. Campaigning has come a long way since the 1896 election in which William Jennings Bryan gave more than 3,000 speeches and traveled to the most remote parts of the nation and William McKinley entertained thousands on his front porch. Even the resurgence of personal contact with voters as an important campaign activity, best exemplified by the extraordinary successes of the primary organizations of McCarthy in 1968 and McGovern in 1972, has a new flavor. It has retreated from the old methods of mobilization and continuous service to an emphasis on persuasive appeals. Most students of politics who have attempted to explain these changes have attributed them to the changing nature of the electorate. Yet no one has undertaken to provide empirical foundations for this explanation by examining the relationship between environment, particularly the demographic characteristics of the electorate, and party over time. While the theoretical frame developed in this study has been applied to cross-sectional data here, it seems to have a clear applicability to research focusing on such questions of political change.

APPENDIX I

The measure of political environment was constructed in four separate steps. (1) The vote division for each office in each year was reduced to a single index number by subtracting the Republican percentage from the Democratic percentage and adding one hundred to remove the minus sign. (2) An estimate of the partisan division in each year was computed by averaging the index number for each contest in that year and, where appropriate, the index number for the gubernatorial contest in the previous year. (3) These yearly averages of partisan balance were regressed on time to provide an estimate of the trend in partisanship during the time period. This trend was represented by the slope of the regression line. That is, time was the independent variable, and the index of partisan division for that year the dependent variable. The equation may be expressed as follows:

$$Y_i = a + bX_i + e_i$$

[37] For example, the finding that both parties in the North do not perform persuasion activities very well in those counties which contain large concentrations of nonwhites and foreign-borns suggests that persuasion was a rarely practiced activity in major American cities during the late 1800s and early 1900s. To the extent to which these county characteristics reflect parochial orientations to political life, one may argue also that as group identifications become less salient, persuasion will be practiced more by both parties. Similar inferences could be drawn from the impact of concentrations of these types of citizens on Democratic mobilization activities. There has probably been a decline in the emphasis on mobilization as immigrant groups have been assimilated into the mainstream of American society, although this decline has been slowed partially by the emergence of "new" immigrant groups—blacks from the rural South and Puerto Ricans. As these new immigrants are integrated into American society (if that ever happens) and their places are not taken by new waves of immigrants, performance of mobilization activities will decline and political machines will finally vanish.

America, the vestiges of the machine-style political organization rest on large concentrations of parochially oriented voters.

The theory of demographic environment and party receives much less empirical support in the South, where unique features of southern politics probably render it inapplicable. Only the *absence* of a relationship between the demographic environment and Precinct Organization conforms to the expectations of the theory, and it is dangerous to base even tentative support for a theory on the confirmation of null hypotheses.

When one turns to an examination of the hypotheses involving the political environment and party behavior, it is immediately apparent why previous empirical studies of these relationships have produced seemingly contradictory results. The kind of relationship between political environment and party performance which emerges in any particular case depends not only on the performance dimension analyzed but also on both the party and the region of the country. Thus, in the data analyzed here, there is support for the conclusions Key and Crotty drew concerning the South, those Eldersveld proposed concerning Wayne County, and also those Putnam and Cutright outlined in their studies. Yet, just as is true when one attempts to generalize from these preceding studies, no clear pattern emerges overall in this one. Thus, both parties in the North appear to organize better as their electoral edge increases, but this is not true in the South. The Republicans perform persuasion activities better as their electoral strength grows in the North, but again this is not the case in the Old Confederacy. Only for Republican mobilization activities is the impact of the environment the same in both regions: as the Competitiveness of the county increases, so too does Republican performance along the Mobilization dimension.

One of the most theoretically interesting findings in this study is obscured by the hypothesis-by-hypothesis mode of presentation employed in Table 5. It involves the interaction of the political and demographic environments. In the North, the Democrats perform mobilization activities better as the concentration of Democratic voters increases, while the Republicans perform these activities better as the county becomes more competitive. Also in this region, Democrats perform persuasion activities better as the constituency becomes more competitive, and Republicans perform them better as the concentration of Republican voters increases. These contrasting performance patterns may be explained by reference to the demographic characteristics of each party's basic constituency. Because voters who require mobilization come generally from groups with pro-Democratic orientations, mobilization is more naturally a Democratic activity. On the other hand, because those groups most affected by persuasive appeals are more likely to be pro-Republican, persuasion is more naturally a Republican activity. Each party performs its "natural" activity better as the concentration of voters becomes more favorable to it. Conversely, each party performs the other party's "natural" activity better as the constituency becomes more competitive. One may infer from these findings that, as the challenge posed by the opposition grows, the incentive increases for a party to practice activities which promise to yield relatively small returns. This tendency also appears for Mobilization in the South.

A focus on only those relationships which are significant, moreover, conceals substantial similarity between the North and South. Of the sixteen pairs of variables which were significantly related in the North, only three have the opposite sign in the South, and two of these are near zero in magnitude. In fact, most of the significant relationships for northern counties are paralleled by high, though not significant, relationships in the southern counties. In other words, the clearest differences between the two regions involve the magnitudes of the relationships rather than their directions. It seems likely that the historic lack of interparty competition in the South has attenuated the impact of environment on party there. No matter what the county environment, and it almost always favors the Democrats, it is uniformly embedded in a heavily Democratic state political system. The impact of race on southern politics may also attenuate the environment-party correlations. Preservation of white supremacy has dominated the politics of that region for decades and has left an indelible mark on both southerners and their political institutions.

Since the Second World War, interparty competition has increased greatly in the South, particularly in presidential elections. While most counties in this region were still not competitive in all elections by 1964, it should be only a matter of time before the politics of the South "joins the union." It remains to be seen, however, whether this trend reflects the "southernization" of national politics, with race as the major political cleavage, or the "nationalization" of southern politics. Nonetheless, as recently as 1964, there were still marked differences between North and South in the relationships between environment and party behavior. While the similarity in direction of these relationships, if not their magnitudes, provides some reason to predict a convergence of the South upon the empirical patterns of the North where environment and party are concerned, the South retains its distinctiveness as a political region in this study.

where their electoral edge is greater and where the concentration of nonwhites is higher.

Persuasion. There is some similarity in the response of each party to the environment in the South and between the two regions where performance along the Persuasion dimension is concerned. This similarity hardly warrants discussion, however, because *none of the relationships between the environmental variables and Persuasion approach significance in the South.* The explanation for variations in the performance of persuasion activities is not to be found in the kinds of environmental variables which have been operationalized in this study.

Summary and Interpretation of Empirical Patterns

The empirical patterns reported in the preceding section are most meaningful when used to test the validity of the hypotheses advanced earlier. In this way additional empirical grounding for a theory of environment and party can be laid. Table 5 portrays the results of the empirical testing of each hypothesis. Several different results are possible, and each is represented by a specific symbol in the table.[36]

The most obvious conclusion which can be drawn from Table 5 is that there is support in the northern counties for the theory which links the demographic environment to party behavior. As predicted by this theory, party performance of campaign activities is related to the concentration of parochial voters. Furthermore, as expected considering the pivotal role of organization for all campaign activities, Precinct Organization does not vary with the demographic environment.

The failure of the Poverty dimension to show any relationships to party performance, however, was not expected and requires that the theory be revised. The poorest counties in the North are rural, as can be seen from Table 2. Political machines have been rare in these counties in the past, and the types of activities which characterize the political machine are not practiced there in the present. One can only conclude from this finding that relative poverty alone is not sufficient to stimulate the development of machine-style party organizations, with their twin emphases on service and mobilization. Perhaps this poverty has to fall closer to the subsistence level than it does in these counties for machines to emerge. One may also speculate that political machines will emerge only where there is a demand for large-scale governmental activity. Where government performs few services, as is the case in sparsely settled rural areas, the kind of "profits" necessary to sustain a machine may be absent. Whatever explanation for the unimportance of the poverty dimension proves valid, one thing is apparent: in modern

Table 5. The Results of Hypotheses Testing

	Hypothesis 1 (competitiveness)	Hypothesis 2 (Demo. concentration)	Hypotheses 3–6 (machine theory)
	NORTH		
Organization			
Demo.	R_n	S*	S
Rep.	R_s	S	S
Service			
Demo.	U	U	U
Rep.	U	U	U
Mobilization			
Demo.	R_n	R_s	S
Rep.	S	S*	S
Persuasion			
Demo.	R_s	R_o	S
Rep.	R_n	S	S
	SOUTH		
Organization			
Demo.	R_n	R_n	S
Rep.	S	R_s	S
Service			
Demo.	U	U	U
Rep.	U	U	U
Mobilization			
Demo.	R_n	R_n	R_n
Rep.	S*	R_n	R_o
Persuasion			
Demo.	R_n	R_n	R_n
Rep.	R_n	R_n	R_n

KEY: U = untested
S = supported
S* = supported using relative performance measure
R_n = rejected because no relationship was found
R_s = rejected because only a spurious relationship was found
R_o = rejected because relationship did not go in the hypothesized direction

[36] The hypotheses involving the service dimension went untested as is indicated by the letter "U." Some hypotheses were supported in the analysis using the party-specific performance measures as is indicated by the letter "S." The few hypotheses which did not receive empirical support in the party-specific analysis but did receive it where the relative performance measures were utilized are indicated by "S*." Rejection of the hypothesis is symbolized by the letter "R," and each R is followed by a subscript to indicate the reason for rejection. Where rejection was warranted because no *significant* relationship was found, the subscript is "n." Where a significant relationship was found which vanished when partials were employed, the hypothesis was rejected due to a spurious relationship as is indicated by the letter "s." Where the hypothesis was rejected because the relationship was significant but not in the direction predicted by the hypothesis, the subscript "o" was used.

Conflict in the Community: A Theory of the Effects of Community Size

GORDON S. BLACK
University of Rochester

Introduction

The decade of the 1960s has been a decade of urban conflict and violence, and, as a result, the study of community conflict has suddenly emerged as an important subfield in the discipline of political science. After years of neglect, the study of conflict, especially violence and urban conflict, has become fashionable, but like so many fashions, the "styles" of inquiry have been dictated more by the concerns of the moment than by a desire to build toward more general theories of conflict.[1]

Although it is certainly understandable that the violence in our great urban centers would attract intensive study, the academic rush to supply the public with "scientific" understanding of such phenomena has led to several problems with the study of conflict. One problem has been the tendency to focus rather exclusively on *violent* forms of conflict. The difficulty is, of course, that violence is only a single form of conflict (and certainly not the most common form of conflict in the urban setting), and an exclusive focus on violence will almost certainly lead to a rather one-sided view of conflict.

Another problem has been the equally understandable tendency to focus the violence in the large cities. For the most part, both the smaller cities and the cities in which riots did not occur have been excluded from study. This is unfortunate from two perspectives. On the one hand, the almost exclusive emphasis on the large city provides us with little information about whether the dynamics of community conflict operate differently in other kinds of communities. On the other hand, we have almost no information about the effects of nonviolent conflict—effects that might include the political instability of decision-makers, opinion polarization, and a host of other phenomena.

Older traditions in this area, however, examine both the nonviolent dimensions of conflict and the effects of conflict under various environmental circumstances. One such tradition involves a theory of intra-group conflict that links both the incidence and the intensity of conflict within a group to the size of the group. Proposed first by Georg Simmel and later elaborated by Lewis Coser, the theory suggests that the *frequency* of intra-group conflict is positively related to the size of a group while the *intensity* of conflict is negatively related to the size of the group.[2] The implication is, of course, that large groups should tend to have frequent conflicts, but of low or moderate intensity, while small groups should tend to have infrequent conflicts, but conflicts that tend to become severe when they occur. Although it was originally developed for groups, this theory may be applicable for communities as well; and one variant of this theory will be examined in this paper.

There is a second and more recent approach to the study of conflict that also has relevance to this analysis. This approach suggests that conflictual behavior can best be understood by assuming that the actors who are party to a conflict are attempting to behave in a rational manner; that conflict, even severe conflict, is not the product of some set of blindly irrational forces within man, but the result of people trying to do the best for themselves given the circumstances in which they must operate.[3] This is not to suggest that politics is entirely free of lunatics; there have been too many assassinations for that. The primary point is that conflict, and certainly community conflict, is the outcome of differences in preferences and tastes rather than the inevitable consequence of the "dark" motives that supposedly lurk within people.

The setting for this study comprises the 89 cities of the San Francisco Bay Area.[4] The data,

[1] There are a number of exceptions to this generalization, but these "theories" of conflict were developed before the decade of riots. Cf. Kenneth E. Boulding, *Conflict and Defense: A General Theory* (New York: Harper and Row, 1962); James S. Coleman, *Community Conflict* (New York: The Free Press, 1967); Lewis Coser, *The Functions of Social Conflict* (New York: The Free Press, 1956); Ralf Dahrendorf, *Class and Class Conflict in Industrial Society* (Stanford: Stanford University Press, 1959); Thomas Schelling, *The Strategy of Conflict* (Cambridge, Mass.: Harvard University Press, 1960); and Anatol Rapoport, *Fights, Games, and Debates* (Ann Arbor, Mich.: The University of Michigan Press, 1960).

[2] Georg Simmel, *Conflict and the Web of Group Affiliations*, trans. by Reinhard Bendix and Kurt Wolff (Glencoe, Ill.: The Free Press, 1955); Georg Simmel, *The Sociology of Georg Simmel*, trans. and ed. Kurt Wolff (Glencoe, Ill.: The Free Press, 1950); and Coser, pp. 73–80.

[3] Every author cited in the first footnote adopts this perspective.

[4] The larger project of which this analysis is a

collected under the auspices of the City Council Research Project, include election statistics covering approximately a ten year period in each city, demographic information obtained from United States Census reports, and interview material obtained in an extended interview with 435 of the city councilmen in the area.[5]

A Theory of Community Conflict

Three Propositions. The Simmel-Coser theory of group conflict rests on three general hypotheses about the characteristics of groups of differing sizes. The first is a simple proposition about the distribution of preferences within a group: the larger the group, the more likely is that group to contain diverse social elements.[6]

The second proposition describes the relationship between the size of a group and the character of the interaction that occurs among group members. The small group, Simmel and Coser suggest, is characterized by face-to-face relationships in which the interaction between group members is frequent, direct, affectively charged, and diffuse. In secondary associations, by contrast, interaction between group members tends to be less frequent, more indirect, emotionally neutral, and specific. In the former case, for example, they argue that relationships are likely to involve emotional ties between group members that tend to encompass the total personalities of the individuals, whereas in the latter the relationships tend to be functionally specific, i.e., the interaction of group members is limited to the group roles they occupy and the interests they represent.[7]

Simmel and Coser's third hypothesis is that the involvement of the members in group activities is likely to be greater in small groups than in larger associations. Coser, however, criticizes Simmel because he failed analytically to separate group size and the involvement of group members.

> Simmel tends to equate size of group with degree of involvement of its members. Although there is much evidence to suggest that the smaller the group the greater the involvement of the participants, since interaction among a few members tends to be more intense than interaction among many, we cannot assume that this relationship holds invariably. . . . We must therefore distinguish between two aspects of group structure: (1) the numerical size of the group and (2) the degree of involvement of group members—although generally we can expect a simultaneous variation of these two factors.[8]

Neither Simmel nor Coser, however, provide any firm theoretical reason for the greater involvement of members of small groups as compared to the members of larger groups, and neither author delineates the principal factors that might lead to more intensive involvement by group members in general. We will turn to an analysis of this question in a moment, but for now we will accept the hypothesis that group size and the involvement of group members are positively related.

The Implications for Community Conflict. From these hypotheses, Simmel and Coser proceed to formulate a theory about the frequency and intensity of conflict in groups of varying sizes. In the small group, the members interact frequently and face-to-face, and they tend to be more highly involved in group life than the members of larger associations of the same type. Coser initially argues that the intensive interaction in the small group provides many *occasions* for conflict,[9] an idea he later modifies by suggesting that: "Close relationships, though providing frequent occasions for conflict, exhibit tendencies toward the supression of these conflicts. If, however, conflicts occur despite suppression, they tend to disrupt the relationship because they are likely to assume peculiar intensity due to the total involvement of the personality and the accumulation of suppressed hostilities."[10]

Thus, although there may be frequent *occasions* for conflict in small groups, the actual *frequency* of conflict is lessened by a supposed tendency for the suppression of conflict in the small group. At the same time, the closeness of the interpersonal relationships and the tendency for hostilities to go unexpressed intensifies those conflicts *that do occur.*

Conflicts are less frequent in small groups not only because they are suppressed, however, but also because the interests of group members are likely to be less divergent. Interaction will produce conflict only when there is something to fight about.

The general argument for large groups is the

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part, the City Council Research Project, is sponsored by the Institute of Political Studies, Stanford University, and is supported by the National Science Foundation under contract GS 496 and GS 1898.

[5] The interviews are with 435 out of a possible 490 subjects. San Francisco was not included because it has a local government which combines city and county functions and structures. *All of the cities are nonpartisan.*

[6] Coser, *Functions of Social Conflict*, p. 144; Simmel, *The Sociology of Georg Simmel*, p. 397.

[7] Coser, chapter 4; Simmel, *The Sociology . . .*, p. 320.

[8] Coser, p. 97.

[9] *Ibid.*, p. 73.

[10] *Ibid.*, Coser also states (p. 79): "Yet, frequent *occasions* do not necessarily eventuate in frequent conflicts. It is precisely the closeness of the relationship and the strong affective mutual attachment of the participants which may induce them to avoid further intensification of the conflict once it breaks out."

converse of the argument for small groups. In the large group, the diversity of interests creates numerous conflicts, but contrary to having a disrupting effect, these conflicts are less intense than those within smaller groups.[11] In large groups, as the argument goes, the involvement of members in group life will tend to be low, interaction among group members will tend to be less frequent and more specific, and the conflicts will tend to be less intense.[12]

Drawing on this theory by Georg Simmel and Lewis Coser, I have examined the data from the cities in this study to see if they reflect two general propositions inferred from Simmel and Coser: (1) the larger the city, the more *frequent* will be the incidence of conflict and contention, while (2) the smaller the city, the more *intense* will be the conflict or contention.[13] I will return to these propositions after exploring an alternate approach to community conflict.

Electoral Structures and Electoral Costs: the Rationalistic Theory

Pluralism and the Number of Minorities: An alternative theoretical approach, involving *costs* and *risks*, may prove to have greater generality than the theory of Simmel and Coser. The initial hypothesis in this theory is the same as the first hypothesis for Simmel and Coser; namely that the larger a community, the more heterogeneous the attitudinal distributions within the community.[14]

But the relationship between city size and heterogeneity goes beyond a simple, linear increase in attitudinal diversity. Attitudinal differences are linked to group differences, and groups provide the foundation for coalition formation in city politics. Racial, ethnic, occupational, and religious groups tend to vote as groups, but they also have associations and representatives who speak "for" the group in the process of community coalition formation.[15] Size, therefore, tends to be associated with the *number* of minorities in a community as well as with the simple increase in the overall diversity in political preferences; and the number of minorities in a community is likely to have a profound effect on the whole process of coalition formation.[16]

The importance of the number of minorities in a community derives from the effects that the number of minorities has on two different types of political calculations: the costs that individuals and groups must sustain in attempting to mobilize other groups for political action and the risks that are associated with the various possible outcomes of community controversy. The first factor will be referred to as the *political mobilization cost* whereas the second factor will be described as the

[11] Coser argues: "Loosely structured groups and open societies, by allowing conflicts, institute safeguards against the type of conflict which would endanger basic consensus and thereby minimize the danger of divergences touching core values. The interdependence of antagonistic groups and the crisscrossing within such societies of conflicts, which serve to "sew the social system together by cancelling each other out, thus prevent disintegration along one primary line of cleavage," p. 80.

[12] This is not a new idea; and it can be found in a large number of places. Peter Blau, for example, says about the same thing: "Without cross affiliations, conflicts tend to be cumulative as many involve the same split in the community, and the predominant communications within each opposition camp may lead to intense hostility and endeavors not merely to defeat the opposition but to destroy it." Peter M. Blau, *Exchange and Power in Social Life* (New York: John Wiley and Sons, 1964), p. 306; see also, Coleman, pp. 21–23; Robert A. Dahl, *Pluralist Democracy in the United States* (Chicago: Rand McNally and Co., 1967); William Kornhauser, *The Politics of Mass Society* (Glencoe, Ill.: The Free Press, 1959), pp. 80–81; and Sidney Verba, "Organizational Membership and Democratic Consensus," *The Journal of Politics*, Vol. 27 (August, 1965), 468–470.

[13] The Coser/Simmel theory was propounded for groups, not for cities, and it remains questionable whether Simmel's and Coser's ideas can be applied to the cities of this study. Their analysis is based on the qualitative character of relationships in small groups; groups which are characterized by intense, face-to-face interaction, where the relationships are personal, affective, and relatively stable. This kind of relationship is contrasted with large groups in which relationships are affectively neutral and distant and lack intensity. Although all of our cities would, on the face of it, seem to fall into the category of the large secondary association, the *politically active stratum*, within which conflicts are fought out, is much smaller and may have all the characteristics of a small group. Take for example a city with 30,000 people. The number of registered voters in such a city will be around twelve thousand, and in any given election one might expect a turnout of about five to six thousand. If 5 per cent of the voters are active on a regular basis, a generous figure, that means the city will have a "politically active stratum" of about 300. Politically active people interact with great regularity, and that number is not too large to have many of the characteristics of the Coser/Simmel group.

[14] For extensive empirical evidence of such a relationship between city size and social diversity, see Jeffrey K. Hadden and Edgar F. Borgatta, *American Cities: Their Social Characteristics* (Chicago: Rand McNally, 1965).

[15] Cf. Michael Parenti, "Ethnic Politics and the Persistence of Ethnic Identification," *American Political Science Review*, 61 (September, 1967), 717–726; Raymond E. Wolfinger, "The Development and Persistence of Ethnic Voting," *American Political Science Review*, 59 (December, 1965), 896–908.

[16] Parenti, pp. 720–23; Robert C. Wood, *Suburbia: Its People and Their Politics* (Boston: Houghton Mifflin Co., 1958), p. 178; Scott Greer, "Catholic Voters and the Democratic Party," *Public Opinion Quarterly*, 25 (Winter, 1961), 624; and Stanley Lieberson, "Suburbs and Ethnic Residential Patterns," *American Journal of Sociology*, 67 (May, 1962), 673–681.

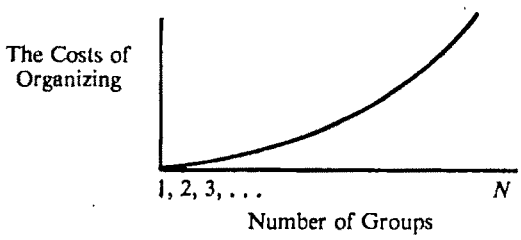


Figure 1. The Costs of Organizing as a Function of Number of Groups

expected political risk. Both these factors are directly related to the number of minorities in a community.

The Cost of Political Mobilization. The first, and perhaps the most difficult, problem of political life is that of organizing a coalition to pursue political action. Organizing a coalition involves a sizeable expenditure of time, effort, and money; and the more factions or minorities in a community, the greater will be the cost of mobilizing one's coalition for a conflict. Because the number of factions or minorities in a community is partly a function of the size of a community, the average costs involved in political mobilization also will be related to the size of a community. Such costs undoubtedly have a dramatic effect on the degree to which conflicts escalate into community battles.[17]

In the small community, where mobilizational costs are relatively low, the participants in a conflict can, in their search for additional allies, easily involve most of the interested individuals and groups in the community. In the large community, by contrast, mobilization is both expensive and difficult, and the costs associated with total community mobilization probably inhibit such attempts except under unusual circumstances.[18]

The problems of mobilizing the community are compounded when the informational level and sense of political involvement of the public are low. Problems that arise in a large community will tend to be more complex, involve more bureaucratic and institutional elements within the city, and involve greater numbers of individuals and groups. Thus, the informational requirements for effective political action are likely to be higher while the informational level of the average citizen is likely to be lower.

[17] For some empirical evidence to support this point for these cities, see Gordon S. Black, "A Theory of Political Ambition: Career Choices and the Role of Structural Incentives," *American Political Science Review*, 66 (March, 1972), 147–148.

[18] Coleman describes the process of searching for allies in some detail; pp. 5–9.

Information is generally costly to acquire, and a higher informational requirement for effective participation is likely to mean that fewer people will take the trouble to acquire the information necessary for successful participation. Thus, in larger communities, one sees the participants to conflicts engaging in the ritual of "informing the public," a pattern of behavior seldom seen in smaller communities.

In addition, if the voter evaluates the prospect of voting partly in terms of the probability that his vote might have some effect on the outcome of elections, then we might expect that the citizen of the small community would be more likely to vote and to participate in community affairs (all other things being equal).[19] The reason is simple (and usually put forward as a defense of the small community): where there are fewer voters, the individual may feel that he has more of a chance of affecting the decisions that are made in his community. This may account for greater citizen involvement in the small community, if greater involvement is in fact the case.

All three of these arguments can be summarized in slightly different language:

Proposition I: The larger the population of a community, the greater the number of distinct groups, factions, and minorities that will be found within the community's boundaries.

Proposition II: The expected costs of organizing for collective action will be an increasing function of the number of groups, factions, and minorities that are found in the community.

A. If the average level of information of the citizenry in a community is a decreasing function of the size of a community, then the cost per unit of organization will be marginally increasing.[20]

B. If the average level of citizen involvement in a community is a decreasing function of the size of a community, then the cost per unit of organization will be marginally increasing.

The combined function of Proposition II, A & B, is shown graphically in Figure 1, with a curve that is sloped in a marginally increasing fashion.

This argument suggests that there are diseconomies of scale in the mobilizing of coalitions such that the cost of organizing a winning coalition increases exponentially as a function of community size. Thus mobilizational costs constitute a barrier that discourages the formation of politi-

[19] William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March, 1968), 25–42.

[20] The hypothesis about "marginal increases" in costs is supported by the data from these cities which show that there is a dramatic increase in costs as one moves from the smaller cities to the larger cities, an increase which is not a simple linear function of city size.

cal coalitions and inhibits the expansion of such coalitions once they form.[21]

The Rewards and Risks of Community Conflict. The number of minorities may also be related to the kinds of *risks* that different groups face when they seek to achieve their goals through the political process. Specifically, it seems likely that a group's response to a potential conflict is a function of two factors (in addition to the cost of mobilization): (1) the net difference between the value of "winning" and the cost of "losing" for the group (which will be called the differential value of the outcome); and (2) the probability that the participation of the group will alter the likelihood that the group will obtain its preferred outcomes. If the differential value for the group is small or if the group's participation is of little consequence, the group will tend to sit on the sidelines and ignore whatever contest exists. Conversely, one should expect conflict to intensify when groups perceive that political outcomes involve large scale value differences and when groups perceive that by participating they can affect the likelihood that their values will win out in the conflict. Both these variables tend to be related to the number of groups in a community and correspondingly to the size of a community.

Groups, like individuals, are likely to feel that they have more opportunity to affect political outcomes when they face few competitors rather than many. Assuming that groups are of relatively equal size, the importance of any given group declines as the number of groups increases.[22] Where many groups are present, as in almost every large city, one is likely to find that political contests involve both a smaller proportion of the total group life in the community (even though many groups may be involved) and correspondingly a smaller proportion of the total population than in the smaller communities.

At the same time, the value of the "stakes" of a political contest is also likely to rest partially on the number of groups around which the contest takes place. Stakes are most "valuable" when two groups, of relatively equal size, confront each other over a wide range of issues about which they are in bitter disagreement. When this occurs, there is little room for compromise because the groups lack a common ground on which to negotiate their positions. Such groups will tend to fight it out on a winner-take-all basis.

A second factor is equally important, however. If a third group is introduced into this situation, a different pattern of behavior is likely to emerge. For one thing, the third group will tend to have somewhat different preferences from either of the two groups, and this difference sets the stage for a bargaining process that centers around the competition of each of the groups for a coalition partner. It also establishes a basis for a bilateral set of agreements that modifies the more extreme demands made by either of the two original groups. The result is compromise, and compromise tends to reduce both what the groups might expect to gain from the conflict and what they might expect to lose. Each of the coalition partners must modify at least some of his demands in order to forge a coalition with some other partner, and the resultant coalition is likely to be less divergent from the third group than was the case earlier. Each additional group increases both the opportunity for bargaining and the likelihood of compromise, so that the expected outcome for each group tends to be both potentially *less profitable* and potentially *less costly* than when the political contest was polarized. With N groups the situation is analogous to an electoral competition with N voters, and the dominant coalition is likely to be one that can appeal to groups lying near the center of the political spectrum.[23]

The implication of these arguments, an implication which is consistent with the cross-pressure theory of overlapping groups, is that the intensity of a conflict should be inversely related to the number of distinct "parties" to the conflict.[24] A large number of groups, however, will not eliminate conflict, but the addition of other groups should encourage coalition formation, bargaining, and compromise; and compromise will tend to mute the risks to the various groups. These argu-

[21] This is a very important part of Mancur Olson's theory of collective action. Mancur Olson, Jr., *The Logic of Collective Action: Public Goods and the Theory of Goods* (Cambridge, Mass.: Harvard University Press, 1965), Schocken Books, 1968), chapter 1; see also, James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: The University of Michigan Press, 1962), pp. 63–84; Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), chapter 14, pp. 260–276; Robert H. Salisbury, "An Exchange Theory of Interest Groups," *Midwest Journal of Political Science*, 13 (February, 1969), 1–32; Charles R. Adrian and Charles Press, "Decision Costs in Coalition Formation," *American Political Science Review*, 62 (June, 1968), 556–563.

[22] The probability of affecting the outcome is a partial function of the number of participants to the collective decision, whether those participants are individuals or groups.

[23] Downs, pp. 114–142; Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review*, 64 (June, 1970), 426–449.

[24] For an excellent review of the cross pressure theory, see Verba, "Organizational Membership and Democratic Consensus," *The Journal of Politics*, Vol. 27 (August, 1965), 467–469.

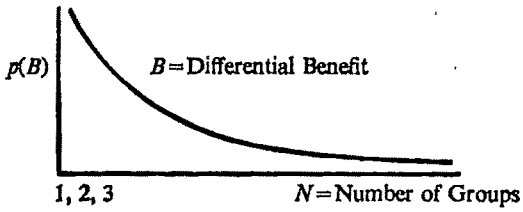


Figure 2. The Benefit from Winning as a Function of the Number of Groups

ments can also be codified in formal propositions and they are shown graphically in Figure 2.[25]

Proposition III: The expected differential value to a group in a conflict is a decreasing function of the number of groups, factions, and minorities that are found in the community.

Proposition IV: The probability that any group will have a noticeable effect on community outcomes is a decreasing function of the number of groups in a community, all other things being equal.

Community Conflict and Electoral Competition

Although some kinds of community conflict undoubtedly do not find their expression through the electoral system, it seems likely that most of the political conflict in a community eventually revolves around who will make decisions for a community. Thus, the election presents us with a useful arena for examining both the character and the effects of community conflict in different kinds of cities, and it also provides us with a set of data with which to examine the relative validity and usefulness of the two different theories outlined above.

In particular, both theories suggest a simple typology of community and electoral conflict that can serve as an organizing device for the subsequent data analysis.[26]

The Pluralistic City: Mobilizational Politics. Both the Simmel/Coser theory and the rationalistic theory of community conflict suggest that the style of political conflict in the large community will be shaped by the necessity of investing great effort in the organizing of political coalitions. Almost all large communities are sufficiently heterogeneous to insure regular electoral competition, but the difficulties of organizing coalitions will retard the development of most conflicts, keeping them at a moderate level of intensity.

In addition, both theories suggest that neither the potential benefits nor the potential losses from a conflict are likely to be particularly large in a city characterized by great diversity. Simmel and Coser argue that the cross cutting cleavages in large groups tend to reduce the intensity with which conflicts are fought out. In the rationalistic theory of conflict, the argument is that the addition of new groups will tend to reduce the differential benefit that groups can expect from a conflict; and as the difference between the value of winning and the value of losing declines, the motivation to intensify the struggle will decline as well. Both theories thus tend to have a similar view of conflict in large communities. In the rationalistic theory, however, this effect is compounded by the presumed ability of individuals and groups to discern that their participation "makes a difference," and this inability undermines the rationale for political action.

In the rationalistic theory, the costs of organizing play a central role in explaining why political actors might choose either to intensify or to avoid a conflict. Those costs include the cost of mobilizing a coalition, the cost of attracting votes, the cost of informing the public, etc., and each of these costs must be paid by the individuals en-

Chart 1

SIZE AND SOCIAL PLURALISM: STYLES OF ELECTORAL CONTESTING

Size	Social Pluralism: Heterogeneous	Social Pluralism: Homogeneous
Large	*City Type:* Pluralistic *Electoral Style:* Mobilizational: Frequent competition, but Mod-intensity	(Does Not Exist)
Small	*City Type:* Fragmented *Electoral Style:* Discontented: Frequent competition, and high intensity	*City Type:* Communal *Electoral Style:* Affirmative: Infrequent competition, Intensity irrelevant

[25] All of these arguments are cast in terms of continuous variables which are continuous in character; and although this typology is being presented for purposes of exposition, the hypotheses that have been stated must be tested independently. The purpose of the typology is to summarize what has been stated earlier; that the variables under consideration create a pattern of relationships that tend to go together, and these patterns constitute the typology.

[26] An example of the "pluralistic city" would be New Haven, Connecticut, studied by Robert A. Dahl and his associates, in *Who Governs?* (New Haven: Yale University Press, 1961). Coleman cites numerous examples of the second type of community, the "fragmented city" in his study, *Community Conflict.* The best description of the "communal city" is in Robert C. Wood, *Suburbia: Its People and Their Politics* (Boston: Houghton Mifflin Co., 1958).

gaged in the conflict. The theory by Georg Simmel and Lewis Coser, by contrast, does not discuss these costs at all. Rather, they stress the relatively neutral character of interpersonal relationships in groups involving large numbers of people, a factor which the second theory ignores almost entirely.

The Communal City: The Politics of Affirmation. At the other end of the scale of communities is the "communal" city. The communal city is small and homogeneous, and the dominant style of political activity is the "politics of affirmation."

Simmel and Coser argue that conflict is less frequent in the small group because it is suppressed by other group members, suppressed because interpersonal conflict is particularly destructive in groups whose members are on familiar terms with each other. In the rationalistic theory, however, the *incidence* of conflict in the small group is simply a function of the diversity of interests that abound in a group, and because some small communities are quite homogeneous, these communities are unlikely to demonstrate a high level of conflict over time. Elections in communal cities should be decidedly tranquil affairs, so tranquil that the character of such elections is not so much a competition for leadership as it is an *affirmation* of the existing order of things.

The Fragmented City: The Politics of Discontent. The third class of cities, those that are relatively small yet heterogeneous, are the critical cities for both theories. Both the Simmel/Coser theory and the rationalistic theory imply that a pattern of politics should be considerably different from the political behavior exhibited in the other two sets of cities. In these cities, which will be referred to as *fragmented* communities, social and political diversity produces many opportunities for conflict, but when the conflict erupts it tends to become more severe than in the other two types of cities. For this reason, I will refer to the electoral style in this kind of city as the "politics of discontent."

The test of these two theories rests on a series of implied empirical hypotheses that can be examined using the data from the cities of this study. Although the empirical indicators that are employed here are not direct measures of the central theoretical propositions outlined above, they are closely enough related to those phenomena so that they should tend to follow the pattern of relationships that comprise the two theories.

Analysis

The Predisposition to Participate. A hypothesis central to both theories is the contention that the willingness of people to participate voluntarily in community life is a function of the size of the community in which they live—the larger the community, the less inclined are citizens to participate. In the Simmel/Coser theory, this relationship is asserted without explanation, but one assumes that the involvement is due to the closeness of relationships within the community. In the rationalistic theory, the willingness to participate declines because people believe that their participation counts for less in the larger community; i.e., neither they nor others can perceive a change in the probabilities of various outcomes as a result of their participation. Although the willingness to participate is obviously affected by factors other than the individual's impact on outcomes, the expected variation of participation with city size is central to both theories.

Table 1. The Relationship Between the Size of the Population of a City and the Turnout in Municipal Elections

Turnout in Elections	Population: Greater Than 30,000 %	Population: Less Than 30,000 %	
Above 50%	16	38	
Between 50% and 40%	32	34	
Below 40%	53	28	
	101%[a]	100%	
Cities	(19)	(68)	(87)

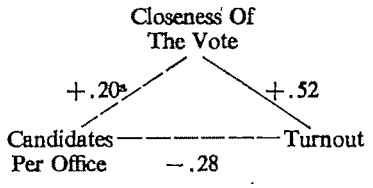
[a] Rounding Error.

The data bearing on this relationship are clear: the large cities are about half as likely to have an average turnout in elections of more than 50 per cent of the registered voters and about twice as likely to have an average turnout of less than 40 per cent. The citizens of the larger communities apparently feel less inclined to participate in elections than do the citizens in the smaller communities. This relationship would be even stronger if the five small communities with uncontested elections were removed from the sample. These five cities have the lowest turnout rates of all (all less than 15 per cent), but the lack of citizen participation in the electional process in these communities is simply due to the complete lack of choice. Where a choice is available, the turnout is considerably higher in the smaller communities. Voting, of course, is not the only form of citizen participation, but these data do not include other forms of participation.

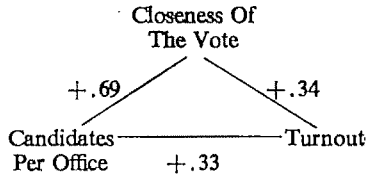
A second issue, however, can be explored with these data. The rationalistic theory suggests explicitly that the declining participation in the

Table 2. Intercorrelations Between the Closeness of the Vote, the Candidates Per Office, and the Turnout Among 'Large' and 'Small' Cities

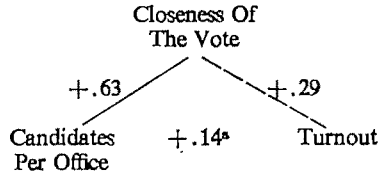
LARGE CITIES



SMALL CITIES



ALL CITIES



$r_{ab} = \pm .30$ (———)
$r_{ab} = \pm .20$ (— — —)

[a] Not significant at the .05 level; all other *r*'s significant at or below .05 level.

larger communities is due to the feeling that people cannot affect outcomes as readily in the larger communities. If the lower rates of participation in the large communities are due to what one might call this "probability effect," the *data ought to also show that this effect is mitigated by other variables which would increase one's estimate that his vote might make a difference.* Moreover, the effect of any such variable should be more pronounced in the larger cities where the base level of participation is less than in the smaller cities.

A variable which should obviously affect such probability estimates is the closeness of the vote in elections. Riker and Ordeshook have already shown that the voter's estimate that the election will be close increases his likelihood of voting in national elections, and it seems reasonable to expect that the actual closeness of votes in a community will have a comparable effect.[27]

[27] One wonders what the precise relationship might be between the number of voters and the sense one has that one's own vote counts for something. Undoubtedly, the relationship reaches a saturation point such that increases in the number of voters has no appreciable effect on one's subjective feeling of the importance of one's vote. Also, it is probably true that voters systematically overestimate the importance of their vote. See Riker and Ordeshook, pp. 38–40.

Table 2 shows the intercorrelations among three variables central to the electoral process: the closeness of the vote in city elections, the number of candidates seeking office, and the turnout of registered voters. Each of the variables is an average for each city based on ten years of electoral data.

In the first place, the closeness of the vote is significantly related to turnout both when all the cities are examined and when the cities are divided according to size. Moreover, when the cities are divided according to size, the relationship is substantially stronger among the large cities than among the small cities ($r^2 = .28$ to $r^2 = .12$). The pattern, therefore, is precisely what was predicted: i.e., the relationship exists among all cities but it is stronger in the large cities.

A second set of relationships lends further support to the rationalistic interpretation of the electoral process. When one compares the relationship between the number of candidates per office and turnout in both the large and the small cities, one finds a complete reversal in the relationship. Among the smaller cities, the greater diversity of choice associated with more candidates seems to be somewhat related to a higher turnout in election. Among the larger cities, however, the more candidates seeking office, the less likely are people to vote.

Although this finding might seem surprising, it is consistent with the discussion of information costs in the rationalistic theory. One of the problems for the voter is that he requires some information on which to base a voting decision, and the acquisition of information costs the voter time and effort. These costs for the average voter probably increase with the size of the city, particularly in these cities which lack a strong mayor and where public attention is spread across a number of city councilmen. In the smaller cities, where many people are likely to know some if not all of the candidates, the presence of additional candidates would do two things: it would give the voters a greater range of choice and each candidate could appeal to a slightly different sector of the public that was already well acquainted with the candidate. In the large city, by contrast, a proliferation of candidates would increase the informational problems for the voter, dissipate the clarity of a two candidate choice, and force the media to disperse its attention too broadly. The net effect would be to discourage the voter by making his choice difficult and by forcing the

voter to pay more attention to the campaign than he might like. In other words, voter turnout should decline as the number of candidates increases.

The relationship between the number of candidates per office and the closeness of the vote is also consistent with this interpretation. In the large cities, the closeness of the vote is apparently only slightly affected by the number of candidates, whereas in the smaller cities the two variables are closely related. Because the measure for the closeness of the vote is a measure of dispersion (the *mean deviation* of the vote), the strong relationship among the smaller cities means that additional candidates do tend, in fact, to attract rather sizeable blocs of voters, thus reducing the dispersion of the vote among the various candidates. In the large communities, on the other hand, this logic does not appear to hold, and one reason for the lack of a relationship is that many of the candidates in a multiple candidate race cannot establish a sufficient political identity with the public because they are relatively unknown.

All three sets of relationships tend to support the participation propositions in both theories, but there one begins to see some difference in the explanatory power of the two approaches. The Simmel/Coser theoretical approach really does not imply anything directly about the interrelationships in Table 2; but the rationalistic approach clearly does provide an explanation for these findings that is consistent with the central propositions of the theory discussed earlier. One can validly argue, of course, that the implications mentioned here are not derivable in a formal, mathematical sense, but whether or not the explanation offered here is fully acceptable, the pattern of relationships strongly supports the existence of a "size effect" in electoral politics. A more thorough examination of these questions can wait until other related topics have been examined.

The Emergence of Electoral Competition. Conflict in a community may erupt over a wide range of issues, but one of the most likely places for conflict to be expressed is in the competition for political offices. Elected office holders are ultimately held responsible for the decisions from which the seeds of controversy are sown, and the primary mechanism for holding them responsible is the election. Although electoral competition and conflict are obviously not the same phenomenon, they share many similar characteristics that are pertinent to this study. For one thing, they both involve a contest in which two or more "parties" are struggling to obtain a scarce resource. Similarly, the individuals involved have an incentive to mobilize support for their goals with the understanding that such support will aid them in their struggle to obtain those goals. Finally, both conflict and competition are rooted in the differences that exist in the opinions and values of citizens, and neither could exist in a community in which all individuals felt alike about community decisions.

For these reasons, theories of community conflict should also serve to describe much of the behavior that occurs in the arena of electoral competition, and the data on elections should enable one to test many of the central propositions of theories of conflict. The application of conflict theory to elections, however, does not imply that conflict and competition are exactly alike; it only implies that the two kinds of phenomena have a great deal in common. The intent here is to explore some of these common elements.

Both theories of community conflict suggest that the central factor in the emergence of conflict is the presence of social and political diversity in a community. Diversity breeds disagreement, and disagreement breeds both conflict and electoral competition. Larger communities, because they are almost inevitably heterogeneous, should tend rather uniformly to have competitive elections, particularly in communities such as these which lack the dominance of political parties. The smaller communities, by contrast, range from being quite heterogeneous to a small number of upper class suburbs which are quite homogeneous. Among the smaller communities, the competitiveness of local elections should vary rather strongly with the extent of the social and political diversity within the community.

This in fact seems to be the case. In research reported elsewhere, several measures of social diversity were correlated with the closeness of the vote in city elections, and they were all found to be related to competition both when all the cities were considered and when only the smaller communities were examined. At the same time, social diversity was found generally unrelated to competition among the larger cities.[28]

Political Mobilization. Another of the expected differences between the large and the small cities derives from the problem of mobilizing voters and groups in elections. The last two tables have already shown that the voters of small communities seem more predisposed to participate than do the citizens in larger communities, and differences in electoral competition have been shown to have less impact on turnout in the small than in the large cities. These data support the participation hypothesis in both theories.

The problem with this inference, however, is

[28] Gordon S. Black, "Patterns of Politics in Small Democracies: An Analysis of Community Politics" (doctoral dissertation, Stanford University, 1970), p. 123.

Table 3. The Mobilizational Effects of Size and Competition

Personal Campaigning	Large Cities: Last Election Closeness-of-Vote High %	Large Cities: Last Election Closeness-of-Vote Low %	Small Cities: Last Election Closeness-of-Vote High %	Small Cities: Last Election Closeness-of-Vote Low %
Fairly Hard	82	52	51	39
Some or Not Too Much	18	48	49	61
Respondents (*N*)	(82)	(21) (103)	(116)	(123) (239)

	Large Cities: Last Election Closeness-of-Vote High	Large Cities: Last Election Closeness-of-Vote Low	Small Cities: Last Election Closeness-of-Vote High	Small Cities: Last Election Closeness-of-Vote Low
Average Group Support Per Candidate	1.38	1.24[a]	.48	.50[a]
Respondents (*N*)	(82)	(21) (103)	(132)	(141) (273)

	Large Cities: Hard Campaigning	Large Cities: Some & Little Campaigning	Small Cities: Hard Campaigning	Small Cities: Some & Little Campaigning
Average Group Support Per Candidate	1.54	1.00[b]	.65	.44[b]
Respondents (*N*)	(82)	(32) (114)	(126)	(166) (292)

[a] Not significant.
[b] Significant at .05 level.

that the intervening role of the candidate is ignored. The voter is not only responding to the closeness of the vote, he is also responding to the efforts of candidates to mobilize his vote; and one can assume that the mobilizational efforts of prospective office holders are partly determined by how close they expect the vote to be. Although the evidence on this variable is somewhat fragmentary, it all points to the fact that neither the certain winner nor the certain loser invests very much time and effort in his campaign.[29]

Two hypotheses here are relevant to the earlier analysis. First, the mobilizational efforts of candidates should be a function both of the size of a community and of the degree of electoral competition, large communities requiring more electoral effort than small communities. Second, one should be able to demonstrate that the efforts of the candidates actually increase the extent to which groups participate in elections. If these hypotheses can be confirmed, it will lend support to the argument that the central problem for the candidate in the large community is that of mobilizing his constituency, a problem that is not nearly as severe in smaller communities.

The first two hypotheses have been confirmed elsewhere and need not concern us here.[30] Electoral campaigning is strongly related to city size and electoral competition, and the candidates in the large communities are nearly three times as likely to campaign extensively as the candidates from the smaller, less competitive cities and about twice as likely as those candidates from the small competitive cities.

The second hypothesis suggests both that group mobilization is more extensive in the larger communities and that it is primarily a response to the campaign efforts of candidates.

The first part of the table clearly shows that the campaign efforts of candidates are structured by the size of the community and by the closeness of the vote in elections. Even the three less competitive large cities require as much effort as the more competitive small cities. The other two parts of the table demonstrate the importance of the efforts made by the candidates. Electoral competition is slightly related to an increase in group activity among the large cities, but it is quite unrelated to group activity among the smaller cities. When, however, one examines the impact of the candi-

[29] Cf., Alexander Heard, *The Costs of Democracy* (Chapel Hill, University of North Carolina Press, 1960); Herbert E. Alexander, *Financing the 1960 Election* (Princeton, N.J.: Citizens Research Foundation); Alexander, *Financing the 1964 Election* (Princeton, N.J.: Citizen's Research Foundation, 1966; and Alexander, *Financing the 1968 Election* (Boston: Heath, 1971).

[30] Black, "A Theory of Political Ambition: Career Choices and the Role of Structural Incentives," p. 148.

Table 4. Candidate Hostility by the Closeness of the Vote, by City Size and by the Closeness of the Vote Controlling for City Size

Candidate Hostility	Population: Large %	Population: Small %	N	Closeness-of-Vote: High %	Closeness-of-Vote: Low %	N
Much	16	16		20	9	
Some	29	30		35	22	
None	55	54		44[a]	69	
Respondents	(82)	(224)	(306)	(186)	(120)	(306)

	Large Cities: Closeness of the Vote			Small Cities: Closeness of the Vote		
Candidate Hostility	High %	Low %	N	High %	Low %	N
Much or Some	49	—		60	32	
None	51	—		40	68	
Respondents	(69)	(13)[b]	(82)	(117)	(107)	(224)

[a] Rounding Error.

[b] This table was collapsed into two categories on the variable of interpersonal hostility because of the small number of cases in this column. Even collapsing the table, however, does not increase the number of cases in the cells sufficiently to allow an interpretation of this column of the table.

date's own efforts—efforts that are strongly influenced by electoral competition—it can be seen that the campaign efforts can substantially increase the number of groups supporting candidates. Moreover, the group activity among the large cities is considerably higher than that found in the smaller cities, and the level of that activity apparently can be sharply increased by the candidates.

These findings, coupled with those discussed earlier, tend to confirm several of the hypotheses in both theories of community conflict. Electoral competition, our surrogate for conflict, seems to arise as a response to social and political diversity; and the large cities, being more heterogeneous, are also more competitive. At the same time, citizen involvement does appear to be less in the larger cities, but electoral participation, our measure of involvement, tends to increase with the competitiveness of the milieu and the mobilizational efforts of the candidates. Among the small cities, on the other hand, citizen involvement is higher and less dependent upon the mobilizational efforts of candidates. These findings support the contention that the central campaign problem in the larger communities is the problem of mobilizing a constituency, but it does not confirm the contention that the electoral style of competition tends to be more intense among the smaller cities. For that we must turn to additional data.

Additional Evidence on the Intensity of Electoral Conflict

Electoral Conflict. One measure of the intensity of conflict in elections is the extent of hostility between the candidates. Some hostility between candidates should be expected whenever there are contested elections simply because someone has to lose. If hostility between candidates were merely a function of candidates losing, however, then we might expect that the greatest hostility would exist in the "large" cities where the candidates stand to lose the greater investment they must make in elections because of the higher costs of campaigning. If, however, the size of a community affects the conflictual level of a campaign in the manner discussed above, then we should find that intercandidate hostility is greater in the "small" and competitive cities. Table 4 shows the relationships between city size and the closeness of the vote, and the extent of the interpersonal hostility between candidates at the time of the candidates' last election.

When the variables of city size and the closeness of the vote are employed independently, only the closeness of the vote is related to the level of interpersonal hostility among candidates. When city size is employed as a control variable, the predicted pattern emerges. The councilmen from the "small" and competitive cities are most likely to indicate that the hostility existed between the candidates, while the councilmen from the "small" cities with less competitive elections are least likely to indicate that such hostility existed.

A more direct measure of what I have termed the "politics of discontent" is the perception of the extent to which incumbents are defeated in a city. Such data are, of course, subject to a high

Table 5. Frequency Incumbents Defeated by City Size, by Closeness of the Vote to the Closeness of the Vote Controlling for City Size

Frequency Incumbents Defeated	Population: Large %	Population: Small %		Closeness of the Vote: High %	Closeness of the Vote: Low %	
Sometimes	37	35		44	25	
Seldom	63	65		56	75	
Respondents (*N*)	(99)	(232)	(331)	(190)	(141)	(331)

Frequency Incumbents Defeated	Large Cities: Closeness of the Vote High %	Large Cities: Closeness of the Vote Low %		Small Cities: Closeness of the Vote High %	Small Cities: Closeness of the Vote Low %	
Sometimes	39	30		48	24	
Seldom	61	70		52	76	
Respondents (*N*)	(79)	(20)	(99)	(111)	(121)	(232)

degree of judgment error; but this analysis will be supported with the actual incidence of incumbent defeat in a moment.

In Table 5 both city size and the closeness of the vote have been correlated with the councilmen's perceptions of how frequently incumbents are defeated in their cities; and then size is employed as a control variable while closeness of the vote is correlated with the perception of incumbent defeat. Again, as before, city size is not related to the perception of incumbent defeat, while the closeness of the vote does show a relationship with that variable. But, as before, the predicted pattern emerges when city size is employed as a control variable. The "large" and competitive cities tend to fall between both sets of "small" cities, and 39 per cent of the councilmen in those cities indicate the incumbents are sometimes defeated in their cities as compared to 48 per cent of the councilmen in the "small" cities with closely contested elections and 24 per cent of the councilmen in the "small" but less competitive cities.

Both of the variables employed above seem to suggest that a qualitative difference does exist in at least some aspects of the expressive style of electoral politics in the small cities. Two variables, however, are not enough to establish that such a difference in style exists, even though the relationships are in the predicted directions.

A third indicator of differences in the expressive style of electoral competition is the extent to which councilmen disliked some group during the campaign. Intergroup hostility is a phenomenon often discussed by students of intergroup relations, including Simmel and Coser, and such hostility is usually expressed as some function of intergroup conflict. Interpreting this relationship, however, is somewhat more difficult than with the previous two variables, because the relationship is less obvious.

We know from Table 3 that there are more than twice as many groups active in elections in the "large" cities as in the "small" cities; the average number of groups named per councilman in the "large" cities is 2.73 times as great as that average for the councilmen in the "small" cities. That table also shows that the average number of groups per councilman is not associated with the closeness of the vote in the "small" cities and only slightly related to the closeness of the vote in the "large" cities.

In order to determine whether the disliking of groups is proportionally greater in the "small" cities with closely contested elections, we need to examine three different hypotheses. One hypothesis is that the incidence of disliking groups is *strictly* a function of the number of groups active in elections; the more groups, the more likely that councilmen will indicate that they particularly disliked a group. In this case, the proportion of councilmen indicating that they disliked groups should be related to the size of the communities from which they come, and this relationship should follow roughly the same pattern as that found in Table 3. A second hypothesis is that disliking groups is *strictly* a function of the closeness with which elections are contested, and the increase in the number of councilmen indicating an especially disliked group *that is a function of competition* should be about the same among both the "large" and the "small" communities.

The third hypothesis, the one of importance for our analysis, is that there exists a special "size effect" among the "small" communities—a *stronger* relationship between the closeness of the vote and the disliking of groups among the

Table 6. Groups Disliked by City Size, by the Closeness of the Vote, and by the Closeness of the Vote Controlling City Size

Groups Disliked	Population: Large %	Population: Small %		Closeness of the Vote: High %	Closeness of the Vote: Low %	
Yes	47	24		39	0	
No	53	76		61	80	
Respondents (*N*)	(102)	(246)	(348)	(200)	(118)	(348)

	Large Cities: Closeness of the Vote			Small Cities: Closeness of the Vote		
Groups Disliked	High %	Low %		High %	Low %	
Yes	49	40		31	17	
No	51	60		69	83	
Respondents (*N*)	(82)	(20)	(102)	(118)	(128)	(246)

"small" communities than among the "large" communities. In Table 6, the councilman's dislike of groups is correlated both with the size of his city and with the closeness of the vote in city elections; and in the latter part of the table, the closeness of the vote is correlated with the disliking of a group, again controlling for city size.

In the first part of the table, both city size and the closeness of the vote in the councilman's election are related to the proportion of councilmen indicating that they disliked some group.

By employing city size as a control variable, it is possible to examine the relative validity of the three hypotheses. Compared to the "large" cities, the "small" cities in the second part of the table show a much greater increase in the proportion of councilmen who disliked a particular group that is associated with the closeness of the vote in city elections, *even though from Table 3 we know that there is no increase in the average number of groups present when one moves from the "small" cities with less closely contested elections to those "small" cities that do have closely contested elections.* Among the "large" cities, however, there is only a 9 per cent increase in the proportion of councilmen indicating that they disliked a particular group when one moves from less competitive to the more competitive "large" cities. This increase is proportionally about the same as the increase in group support across the two sets of cities, as reported in Table 3.

Although this table tends to support the first hypothesis, i.e., that the disliking of groups is partly a function of the number of groups active in elections, it also suggests that there is a "size effect" operating among the "small" cities. The second hypothesis that the disliking of groups is strictly a function of electoral competition is not confirmed because the relationship between the closeness of the vote and the disliking of groups is much weaker among the "large" cities than among the "small" cities.

Thus, among the "small" cities, the incidence of group dislike appears to be accentuated by the "smallness" of the communities. Community conflict, translated into the electoral arena through electoral competition, appears to engender animosity towards the groups that participate in elections in the "small" cities, at least a higher incidence of animosity than one would expect simply on the basis of the number of groups present.

These three tables all tend to support the contention that elections in the "small" and competitive communities provide an avenue through which discontent is expressed in communities. The data for these tables, however, were based on interviews with the councilmen, and we need not rely exclusively on their judgment about the quality of the electoral process in their communities. The following tables are based on electoral statistics gathered in each city, and they reflect the actual extent to which councilmen seek re-election and the actual frequency with which incumbents are defeated in these cities.

Incumbents Seeking Re-election. Up to now I have suggested that among the "small" communities a "size effect" operates to produce an expressive style of politics that is characterized by high intensity and widespread discontent. At the same time, however, a second "size effect" is associated with the larger communities and is related to the mobilizational requirements of the larger communities. The original evidence that such an effect might occur was the finding discussed earlier

Table 7. Incumbents Seeking Re-election by City Size, and by Closeness of the Vote

Percentage of Incumbents Seeking Re-election[a]	Population: Large %	Population: Small %	
70% or Greater	63	43[b]	
Less than 70%	37	57	
Number of Cities (*N*)	(19)	(69)	(88)
	Average Closeness of the Vote: High	Average Closeness of the Vote: Low	
Percentage of Incumbents Seeking Re-election	%	%	
70% or Greater	45	47[c]	
Less than 70%	55	53	
Number of Cities (*N*)	(49)	(34)	(83)

[a] These figures are calculated from election materials obtained from the various cities; in most cases representing the average from the five previous elections. The calculation for this average is quite simple: Total number of incumbents seeking re-election divided by the total number of incumbents.

[b] Significant at .05 level.

[c] Not significant.

that the closeness of the vote was highly associated with increases in turnout and group participation in the larger communities whereas these same relationships were much weaker in the smaller communities. This suggested that electoral competition might be strongly related to the electoral costs facing an incumbent in the "large" communities but substantially less related to those costs in the "small" communities.

At the same time, however, incumbents in the "large" communities have already made a rather sizeable investment in obtaining the offices they hold. They got to be incumbents by paying a higher initial electoral cost than the incumbents in the smaller communities, and for this reason they are more reluctant to retire from office.

These two factors might be expected to operate in the following manner. In general, incumbents from the "large" communities should be more likely to run for re-election than incumbents from the "small" communities because their original investment in obtaining their office was greater than was the case for incumbents in the "small" communities. At the same time, however, when faced with the proportionally greater costs of running for re-election in a highly competitive (and risky) "large" city, they would be less willing to pay the cost of winning re-election. If this is the case, we should expect to find that city size is positively related to the tendency of incumbents to seek re-elections but that electoral competition is negatively associated with incumbents seeking re-election in the "large" communities.

Among the "small" communities, on the other hand, the costs of seeking re-election are not closely associated with the degree of electoral competition, and consequently electoral competition should not affect the tendency of incumbents to seek re-election among these cities. In Table 7 city size and the closeness of the vote in city elections are correlated with the percentage of incumbents who seek re-election in these cities.

One can see from Table 7 that city size is somewhat related to the tendency of incumbents to seek re-election, and, as expected, incumbents tend to seek re-election somewhat more in the "large" cities than in the "small" cities. The measure of the closeness of the vote, however, is unrelated to the tendency of incumbents to seek re-election, but the reason for this will be clear in Table 8.

In Table 8 the variable of city size is employed as a control variable, and, among the "small" cities, the closeness of the vote is not related to any decline in the percentage of incumbents seeking reelection. Because the number of cities in one column are so few, it isn't possible to interpret the relationship between the closeness of the vote and incumbents seeking re-election among the "large" cities.

When the dividing point for the closeness of the vote variable is lowered from a mean deviation of .13 to one of .11, however, the predicted relationship emerges and it is very strong. Almost all of the "large" cities with closely contested elections in Table 9 have an average of less than 70 per cent of the incumbents seeking re-election

Table 8. Incumbents Seeking Re-Election by Closeness of the Vote Controlling for City Size

Percentage of Incumbents seeking Re-election	Large Cities: Average Closeness of the vote: High %	Large Cities: Average Closeness of the vote: Low %	Large Cities: *N*	Small Cities: Average Closeness of the Vote: High %	Small Cities: Average Closeness of the Vote: Low %	Small Cities: *N*
70% or Greater	50	—		43	42	
Less than 70%	50	—		57	58	
Number of Cities	(14)	(3)	(17)	(35)	(31)	(66)

(there is one deviant case), whereas all of the cities with less closely contested elections have an average of more than 70 per cent of their incumbents seeking re-election. Thus the closeness of the vote, as predicted, appears to be strongly related to the tendency of incumbents not to seek re-election among the "large" cities, but the closeness of the vote is *not* related to incumbents seeking re-election in the "small" cities. This finding seems to confirm the argument made above that competition increases the mobilizational requirements of candidates in the "large" communities and thereby increases the costs of seeking re-election in these cities. This increase in costs induces incumbents to decline the opportunity to seek an additional term. At the same time, however, the lack of a relationship between the closeness of the vote and the tendency to seek re-election in the "small" cities may occur because competition does not significantly increase the costs of running for office in these cities.

Incumbent Winning Re-election. The argument concerning the defeat of incumbents has been made earlier. The variable of city size should be unrelated to the extent to which incumbents are defeated when they seek reelection while the closeness of the vote should be related to the incidence of incumbent defeat in cities. The results from the interview data follow this pattern and the same pattern should occur here.

The critical hypotheses as discussed earlier are: (1) the closeness of the vote should be strongly related to the incidence of incumbent defeat among the "small" cities but not in the "large" cities, and (2) the incidence of incumbent defeat in the "large" cities should be higher than among the "small" cities with less contested elections and lower than among the "small" cities with closely contested elections.

The relationships in both parts of Table 10 are the same as those we examined in Table 5. City size appears to be unrelated to the incumbent win ratios for the cities, while the closeness of the vote is negatively related to those ratios. Table 5 indicated that councilmen from the cities with closely contested elections were more likely to perceive that incumbents were often defeated in their cities than was the case for the less competitive cities, and indeed the actual incumbent win ratios are lower in the cities with closely contested elections. The critical hypothesis, however, is that the "small" cities with closely contested elections should have, on the whole, lower incumbent win ratios than any other set of cities. In Table 11 the closeness of the vote is correlated with the incumbent win ratios for the cities while controlling for the size of the cities.

These tables give evidence on several points. In the first place, the "large" cities with closely contested elections have a distribution of incumbent win ratios that falls exactly halfway between the two sets of "small" cities. As was predicted, the "small" and competitive cities tend to have, on the whole, the lowest incumbent win ratios (i.e., incumbents are defeated more often in these cities) while the "small" cities with less closely contested elections tend to have the highest incumbent win ratios (i.e., incumbents are defeated least often in these cities).

In addition to these findings the closeness of the election in the "small" cities appears to be strongly related to the frequency of incumbent victories in

Table 9. Incumbents Seeking Re-election by Closeness of the Vote in "Large" Cities Only[a]

	Large Cities		
	Average Closeness of the Vote		
Percentage of Incumbents Seeking Reelection	High %	Low %	
70% or Greater	13	100	
Less than 70%	87	0	
Number of Cities (*N*)	(8)	(9)	(17)

[a] In order to show the relationship for the large cities, it was necessary to reduce the cutting point for the measure of the closeness of the vote from a mean deviation of .13 to a mean deviation of .11.

Table 10. Relationship of Incumbent Win Ratio to City Size and to Closeness of the Vote

	Population		
Incumbent Win Ratio[a]	Large %	Small %	
Above 90%	29	26	
70% thru 90%	41	47	
Below 70%	29[b]	26[b]	
Number of cities	(69)	(19)	(87)

	Average Closeness of the Vote		
Incumbent Win Ratio	High %	Low %	
Above 90%	17	41	
70% thru 90%	48	35	
Below 70%	35	24	
Number of Cities	(48)	(34)	(82)

[a] This index is constructed by dividing the total number of incumbents who win re-election in a city by the total number of incumbents who seek re-election.

[b] Rounding Error.

Table 11. Incumbent Win Ratio by Closeness of the Vote Controlling for City Size

	Large Cities			Small Cities		
	Average Closeness of the Vote			Average Closeness of the Vote		
Incumbent Win Ratio	High %	Low %		High %	Low %	
Above 90%	21	—		15	45	
70% thru 90%	57	—		44	36	
Below 70%	21[a]	—		41	19	
Number of Cities	(14)	(13)	(17)	(34)	(31)	(65)

	Large Cities		
	Average Closeness of the Vote[b]		
	High %	Low %	*N*
Above 80%	38	44	
Below 80%	62	56	
Number of Cities	(8)	(9)	(17)

[a] Rounding Error.

[b] This part of the table employs the same measure of the average closeness of the vote as Table 9; the cutting point is a mean deviation of .11 as opposed to the figure of .13 used above. The number of categories of the dependent variable, the incumbent win ratios, has been reduced because of the limited number of cases in this table.

these cities. In fact, the relationship between the closeness of elections and the incumbent win ratios is stronger among the "small" cities than among the whole set of cities.

As in a previous table it is necessary to lower the cutting point for the closeness of the vote when examining the "large" cities because there are too few cases in one column of the table. When that change is made, the table that results suggests that the closeness of the vote is unrelated to the incumbent win ratios in the "large" cities. The perceptual data in Table 5 suggested that the closeness of the vote was largely unassociated with the councilmen's perception of incumbent defeat, and that finding is now supported with aggregate electoral statistics from the cities.

The parallel results from two different data sources is striking and suggest that the character of electoral competition is quite different in the "small" cities. Whereas in the "large" cities electoral competition appears to deter many incumbents from seeking re-election, electoral competition in the "small" cities appears to be associated with incumbents losing their bids for re-election. At the same time, electoral competition is largely unrelated to the incidence of incumbents winning in the "large" cities and unrelated to the incumbents' decision to seek re-election in the "small" cities. Thus, electoral competition appears to have a quite different character in the two sets of cities; in the "large" cities it forces extensive political mobilization while in the "small" cities it is related to the expression of discontent with incumbent decision-makers. If these patterns are really divergent in character, that is to say, if there are really *two* separate effects on electoral politics caused by community size, then we should find that the incumbent win ratios for the cities are unrelated to the tendency of incumbents to seek re-election. Table 12 shows the relationship between these two measures.

In Table 12, the tendency of incumbents to seek re-election is only very slightly related to whether or not incumbents are defeated in those cities. Where incumbents seek re-election, they are both slightly more likely to win in some cities *and* slightly less likely to win in other cities. The relationship is not strong enough to warrant any speculation.

Conclusions

This analysis has explored two different, and competing, theories concerning the patterns of politics that one should expect in cities of varying sizes. Both the Simmel/Coser theory and the rationalistic theory suggest that conflict should become more intense in heterogeneous small communities than in larger cities; and the data presented above support that assertion. In particular, the greater frequency of incumbents' losses in such cities, coupled with the high levels of electoral hostility, indicates that elections serve to express discontent with incumbent decision-makers, a discontent which is less visible both in the larger

communities and in the more homogeneous smaller communities. Thus, in the most general sense, the data support the central conclusions of both approaches to the study of community conflict.

That question that arises immediately, of course, is how we can choose between two theories, based on somewhat different internal constructs, which account for the same phenomena? One point of difference between the two theories arises from the *criterion of generality;* or which theory accounts for the most findings. On this score, the rationalistic theory accounts for more of the findings presented in this analysis. In particular, the rationalistic theory suggests explanations for incumbents not seeking re-election, for the relationships between the number of candidates and electoral turnout, and for several other factors; and these cannot easily be subsumed within the Simmel/Coser approach. The concepts associated with interpersonal relationships, so central to Simmel and Coser, cannot logically be extended to cover these additional phenomena. The rationalistic approach, using concepts of information costs and mobilizational costs, does permit an extension of the theory to cover these phenomena.

A second major point of difference is the precision with which the central terms of the two theories can be expressed. The Simmel/Coser approach rests on assumed relationships between group size and the interpersonal relations with the group. Such relations are subjective, and they are affected by such factors as the age of the group, the intensity of group interaction, the nature of the interaction, etc.—all of which modify the central propositions in their theory. The rationalistic theory, while resting on propositions which cannot be easily examined directly, has a degree of specificity that the Simmel/Coser theory cannot match. While the concept of "cost" is difficult to measure precisely, its theoretical and empirical meaning is both simpler and clearer than any of the terms normally employed to describe the difference between the primary group and the secondary association.

A third point of difference is the manner in which inference is derived in the two theories. With Simmel and Coser, conflict is the product of a clash of interests and personalities in a relationship which is "close." The conflict intensifies because initial conflicts are suppressed, for such conflicts may threaten the relationship (or the relationships within the group as a whole.) The problem is that the essential assumptions about the behavior of actors remain unexpressed, and it is not clear whether conflict intensification is a function of "suppression," "interpersonal closeness," or "threat." In the rationalistic theory, by contrast, the empirical implications are directly inferred from a set of relationships based on an underlying set of assumptions concerning rational behavior. The patterns emerge because the relationships described covary with city size. Thus, it does partially establish the conditions under which severe conflicts can arise.

Table 12. Incumbents Seeking Re-Election by Incumbent Win Ratios

	Incumbents Seeking Re-election		
	70% or Greater %	Less than 70% %	
Above 90%	33	24	
70% thru 90%	36	49	
Below 70%	31	27	
Number of Cities	(42)	(45)	(87)

Both theories, however, accurately suggest that the *size* of a community dramatically affects the character of the conflict, competition, and contention that occurs within the community. The popular view of small communities has always been that they provide a haven from which the weary can escape from the conflicts of larger communities. To some extent, this view is true, but it is only true for those small communities which are so homogeneous that controversies cannot erupt into full-scale community warfare. If one looks at the community conflicts of the past two decades, many of the most bitter conflicts involving fluoridation, tainted books in libraries, leftist teachers, and civil rights tended to occur in small towns, not the large cities.

There is now a movement afoot to decentralize our cities, to produce smaller units of government that maximize participation. The implication of this paper is that smaller units *will* encourage participation, but the result of that participation may be severe conflict. While I personally approve of such attempts for other reasons, there are dangers involved and these dangers are largely wrapped in ignorance. These data and the analysis demonstrate that the virtues of "smallness" may be greatly overstated, particularly because the proponents of "smallness" usually assume that "smallness" implies homogeneity.

COMMUNICATIONS

TO THE EDITOR:

Any analysis of a system, whether it is a city real estate market, a valley's hydrological system before and after a dam or drainage system is installed, the ecological system of a dune area, the traffic system of a city, is a theory about the world. One reason to call such theories "systems analyses" rather than, respectively, urban land economics, hydraulic engineering, ecology, or traffic engineering, is because they are fraudulent as scientific theories, and could not stand up to scientific inspection and debate among professionals. If a theory of biological balances in the dunes can be inspected by other systems theorists, instead of by people who know something of entomology, hydrology, microclimatology, the effects of different plant covers on the evaporation rate, and so on, then the systems analyst can get away with being an expert in a subject of which he knows nothing. My purpose here is to suggest some symptoms in the work of a systems analyst which very often indicate such scientific fraud, to help the practical bureaucrat determine for himself whether he is being counseled by an astrologer or by a sage. He may then choose to follow astrological advice, but at least he need not think astrology a science.

(1) References to Scientific Disputes on Sizes of Parameters. In no system which is too complicated for intuitive comprehension are scientists in agreement about the size, and range of variation in size, of crucial causal parameters in the system. If the report on a systems analysis finds no such controversy, it is usually fraudulent. If its analysis of this controversy does not fairly represent both sides of the controversy, and convey to the user what is at stake in the controversy, it is probably misrepresenting the degree of scientific validity it has.

(2) Reports on Data Sources. Many data used in practical analysis are of low quality. Even cost accounts of routine repetitive mechanical production processes are well known, to accountants who work with them, to have serious sources of error. More complicated and erratic systems with less professional attention than cost accounting systems have correspondingly worse data. If the main sources of data are not reported, and the probable sources of error in the data are not analyzed in technical notes, the analysis is probably scientifically fraudulent.

(3) Sensitivity Analysis. Since almost all systems analyses are partly made up of guesses or of choices of estimates that are in controversy, a range of parameter values are possible. Because one generally does not use systems analyses if he understands the whole affair already, the consequences of such possible errors are not obvious. If the systems analyst has not conducted studies of how sensitive his recommendations are (that is, how much the crucial results would vary) to errors in various assumed parameters, it is probably scientifically fraudulent.

(4) Clarity of Definition and Exposition. The assumption of the scientific community is that other experts can read an account of an investigation or "analysis," and tell exactly what was done and on what reasoning processes the argument depends. It is characteristic of scientific discourse, therefore, that the closer one comes to the specific details, the clearer everything becomes. It is the overarching theory that is problematic, not whether parameter p was estimated by regression analysis of census table 3.14-P 31. It is characteristic of scientific fraud that the opposite is the case, that the overarching theory has a specious concreteness and persuasiveness, but exactly what was done at specific steps in the argument disappears into the fog. Some reasonably good indicators of such specificity of detail are:

a) A conveniently organized list of variables in the model, with specific definitions.

b) An appendix describing estimation procedures and probable errors for the crucial equations of the system.

c) A conveniently organized presentation of the notation used, both in the mathematical presentation of the theory (if there is no mathematical presentation of a mathematical model, and all systems analyses are mathematical theories, then of course it cannot be regarded as serious) and in the program.

d) A documentation of the computer programs used in the analysis, sufficiently detailed so that an experienced programmer can find out for you exactly how a particular part of the process was calculated.

e) The use of clear language, and a recognition that the report is directed to nonexperts. This is actually a processual matter, for all experts talk in a jargon that must be translated, and do not realize they are doing it. But an expert can define his terms or tell you where to get a reference where they are defined. A fraud usually

defines one abstract and woolly term by another equally abstract and woolly, so that one cannot even tell *what* he is reasoning about, let alone *how* he is reasoning about it. One must expect real experts to be impatient and abrupt with nonexperts. But they got to be experts by being nonexperts who had experts explain things to them, and so ultimately can be driven to be specific. If one feels on examining a purported expert that the more one asks what the expert is talking about, the woollier it becomes, he is probably a fraud. One experienced in the frauds of a particular field in a particular culture will become attuned to clichés which indicate fraudulent and unconsidered reasoning. For instance, in the United States, in this particular field of systems analysis, a man who is not going to give you an analysis of anything in particular will almost always, at some point or other, urge upon you an "open system" rather than a "closed system" point of view. Unfortunately the dull clink of such linguistic coins that marks a counterfeit has to be learned for each of the linguistic currencies of each field, and I suppose in each language. All I can suggest in this regard is that you make your own judgments of which systems analysts are fake and which are not, and keep track of how the fakes talk so you can tell more quickly the next time.

(5) Computer Mysticism. One of the favorite clichés in the computer business is that a computer can only do what you tell it to. While I do not think this is exactly true, it is true for the kind of work systems analysts do. What this means in turn is that anything *intelligent* that the systems analyst has had the computer do was understandable *in the analyst's mind.* This means that *if* the analyst has understood the structure of the problem and the structure of the method of solution, then it is simple enough for a mere human mind to understand. For example, the mathematicians that design the hours-long huge computations that estimate the orbits of satellites can describe to anyone with elementary differential equations what they are up to, and the general structure of the method of computation. Whenever somebody gives you a lecture on how the computer has enormously increased our capacity to analyze complex systems (a true statement, as witness the orbit computations) without being able to describe to you what he proposes to have the computer do, it is suspicious. That is, what the orbit computation does is to approach a known scientific goal with known means, but means requiring tremendous capacity.

What many systems analysts do is to store one guess after another about how things might work in a computer, connect them together by further guesses about how the system might work, let it run for enough hours to be impressively costly, and call wherever they have happened to get to a scientific goal. One of the symptoms of this procedure is that they can't quite explain what the computer is doing, so it seems that the computer is the magical connection between their disorganized ideas to start with and the remarkable clarity of their conclusions. It is true that whatever magic was done was done by the computer, rather than by the mind of the analyst.

As a practical matter, if an overall summary in reasonably sensible form about what, generally speaking, the computations are for, what goal is being approached by what means in the computations, is not provided, but instead the computer is represented as being able to combine things too complex for us to understand, the combination is likely to be scientifically fraudulent.

(6) Objective Referees. It is characteristic of scientific work that a judgment on the merits of a scientific theory or investigation (and I repeat that a systems analysis is nothing other than a scientific theory of a particular system) ought to be made by experts who have no stake in the outcome. Five out of six of the papers submitted to the *American Sociological Review* are rejected as of not enough scientific value to be worth publishing. If the ratio of work worth reading is as high as one-sixth in systems analysis, I would be surprised. The way we protect ourselves against fraud in a science, then, is to systematically provide for professional level critiques by independent disinterested authorities. I would say that an ordinary science involved in reasonably complicated systems employs about a sixth of its research resources in such independent professional review. As a rule of thumb, then, if less than about 10 per cent of the budget of a system analysis is devoted to independent professional review (with the result of the review submitted to the client rather than the analyst), it is probably fraudulent.

The above six criteria are, I believe, hardly ever met by reports that call themselves systems analysis. I think they are routinely met by scientific papers in the fields that analyze systems: ecology, sociology, economics, geography, metereology, and so on. There is no inherent reason that government officials should not get scientifically valid analysis of policy alternatives. But a man does not become a scientist serving policy makers by proclaiming that he is one. He would become a systems analyst for policy purposes by following well established standards for how to analyze systems, established in the scientific disciplines that know how to do it.

Why then is fraudulent science so prevalent in

the field? I believe that three factors explain the prevalence of fraud.

The first, and most important, is that true science is expensive, fraudulent science is cheap. A policy maker with a million-dollar decision in hand cannot accept the answer that it will cost him three million dollars to find the right answer. But if someone will fake an answer, and spend $100,000 painting the cardboard and chicken wire to look like science, the policy maker can afford it. The second factor is the analyst's self-deception, self-deception institutionalized in the scientific community. It is so much fun to play with computers that it must be valuable. It would be so nice to have a scheme of analysis that makes us all geographers as well as economists as well as sociologists as well as traffic engineers, that general systems theory must be possible. And the gilded coin of general systems theory is so pretty that maybe we can ignore the dull clink of counterfeit.

The third reason is internal to public bureaucracy. Consider the social function of an astrologer. A king gets worried that he might not be doing the right thing in a certain problem area. It is time to engage in search behavior for a new policy. But we are faced with a traditional royal bureaucracy, knowing exactly how to do things according to royal edict, in triplicate, with seven seals. A function of saying, more or less at random, that things would be better if they were changed, is a valuable social function. An astrologer says, more or less at random, that policy ought to be changed. An intelligent king uses this random input in strategic places. An innovator inside an education bureaucracy, for example, who wants to shake things up a bit may not much care whether the man from outside on whom he calls says "Saturn in Pisces" or "student in a general system," as long as the bureaucrat gets from it some legitimation for change. Something that poses as science is particularly good for legitimating change. One can clearly understand why many successful kings consulted astrologers, rather than setting up research projects to study the effects of the planets on the affairs of men.

In short, astrology is cheaper than astronomy, more fun than astronomy, and more useful in bureaucratic politics than astronomy. It is only in that rare case when it matters whether one has the right answer, that one would be ill-advised to consult a systems analyst or an astrologer.

ARTHUR L. STINCHCOMBE
Netherlands Institute for Advanced Study and University of California, Berkeley

TO THE EDITOR:

There is some irony in defending the importance of studying state and local political systems in the pages of the *American Political Science Review*. But the thrust of Professor Douglas D. Rose's recent article, "National and Local Forces in State Politics," *APSR*, 67 (December, 1973), 1162–1173, is that studies of policy differences among the states are wrongheaded. Rose argues that variance among states is less than variance among individuals, and that as a consequence, studies of state politics are "severely limited in their usefulness" (p. 1173). He also offers the observation that national forces impact state politics and that as a consequence "states cannot usefully by viewed as political systems" (p. 1169).

It is, of course, a *non sequitur* to argue that because variance among states or other aggregates is less than the variance among individuals, the study of state politics is useless. Professor Rose documents (to the fourth decimal point) the fact that standard measures of variance for the whole U.S. population are greater than similar measures for policy variables for the fifty states. But this fact has no relation to the importance of developing explanatory theory about variations among state political systems. Using Rose's logic, one might conclude that the study of comparative government is "severely limited in its usefulness" because the variance among individuals in the world population is less than the variance among nations. It is not the size of the variance that makes a topic theoretically interesting; scholars study state politics because they are interested in explaining differences among states, however modest these differences may be in relation to differences among individuals.

It is also a *non sequitur* to argue that states "cannot usefully be viewed as political systems" because federal policy affects the activities of states. Even if Rose's hunch that the most influential determinants of state policies are national forces rather than characteristics of states turns out to be correct, this would not render systematic models of state and local government useless. Indeed, systematic models are probably essential in testing Rose's untested assertions on this point. And it is noteworthy that the relevant literature on the impact of federal policy in the states treats states as political systems responding to national policy.[1] Of course, most of this research has been done by economists, and perhaps

[1] See Seymour Sacks and Robert Harris, "The Determinants of State and Local Government Expenditures and Intergovernmental Flows of Funds," *National Tax Journal*, 17 (March, 1964), 75–85; Roy W. Bahl and Robert J. Sanders, "Determinants of Changes in State and Local Government Expenditures," *National Tax Journal*, 18 (March, 1965), 50–57; Jack W. Osman, "The Dual Impact of Federal Aid on State and Local Government Expenditures," *National Tax Journal*, 19 (December, 1966), 362–372; Louis J. James, "The Stimulation and Substitution Effects of Grants-In-Aid: A General Equilibrium Analysis," *National Tax Journal*, 26 (June, 1973), 251–265.

we should not have expected Professor Rose or the *Review* editors to be familiar with it. But economists have managed to learn a great deal about states and municipalities, and the nation as well, by employing models whose assumptions are not fully congruent with reality.

The political science literature on state and municipal government activity is not as advanced as the related literature in economics. Of course, if we accept Professor Rose's contention that studies of state policy differences are "trivial" (p. 1166), then this should not really concern us. But we have made considerable progress over the last decade in developing a systematic understanding of the behavior of state and local governments. That understanding was developed by employing, explicitly or implicitly, models which viewed state and local governments as political systems. The test of the utility of a model is its contribution to understanding, not its isomorphism with reality.

THOMAS R. DYE
Florida State University

TO THE EDITOR:

In disputing my conclusions, Professor Dye misrepresents my position. It is not my position that states cannot usefully be viewed as political systems simply because of federal policies. Nor is it my position that the study of state politics is useless or trivial. In attributing these arguments to me, Professor Dye so considerably jumbles my reasoning that I wish to make clear a few of my basic points.

A variety of evidence—over time, cross-section, individual, county, state, region, national, social, economic, electoral, governmental, policy and policy impact—indicates that the systematic causes of state policy occur at many levels. If we consider only state-level variables and relations, then we ignore the major causes of state policy and most of the policy differences; because this selection process controls for the major causes of policy, we cannot make inferences from what is left to what has been omitted and thus cannot make valid statements about state policy as a whole. Even if Professor Dye is only interested in the state-level causes of state policy differences, he must consider other levels to arrive at reliable conclusions because causes at other levels determine some of the state-level relations: his results will depend on how he treats national and local forces.

The problem is not that state systems models are somewhat unrealistic but that they misconstrue relations in a way which produces confusion and error. Econometric reasoning, the part of the economic literature which has proved most adaptable to political science, lays out the difficulty: strong associations dominate weak associations and produce spurious correlations, and the only way to deal with the spurious correlations is to take the strong associations into account. States are governmental units within a national society; this fact requires the study of state policy to consider non-state factors and also allows the study of states to make distinctive contributions to our understanding of politics.

DOUGLAS D. ROSE
Tulane University

TO THE EDITOR:

"Political ethos" is one of the most significant concepts to have emerged in the theoretically barren terrain of ethnic politics.[1] Until recently, the empirical basis for this concept rested solely on aggregate data—with their attendant structural and inferential limitations. Wilson and Banfield have currently sought to reinforce both the conceptual and empirical buttressing by presenting their own test of the concept against individual-level data.[2]

We contend, however, that the individual-level data analysis presented by Wilson and Banfield constitutes the strongest evidence yet accumulated for *rejecting* the concept of political ethos. This assertion stems from the discovery that the percentages on which their evidence is based are generally, although not always, calculated with the independent and dependent variables reversed. When proper distinctions are made between the independent and dependent variables and the tables are appropriately calculated, the results disconfirm the basic tenets on which the conceptual structure rests. This reconsidered evidence, coupled with the criticisms leveled in other recent studies,[3] appears to go a long way toward rejecting

[1] The ethos theory was first articulated in Edward C. Banfield and James Q. Wilson, *City Politics* (New York: Vintage Books, 1963, reprint), esp. chaps. 3, 16. Shortly thereafter, the thesis appeared in slightly altered form: James Q. Wilson and Edward C. Banfield, "Public-Regardingness as a Value Premise in Voting Behavior," *American Political Science Review*, 58 (December, 1964), 876–887. For an early controversy over the application of the theory, *cf.* Raymond E. Wolfinger and John Osgood Field, "Political Ethos and the Structure of City Government," *ibid.*, 60 (June, 1966), 306–326; and James Q. Wilson and Edward C. Banfield, "Communications," *ibid.*, 60 (December, 1966), 989–999.

[2] James Q. Wilson and Edward C. Banfield, "Political Ethos Revisited," *American Political Science Review*, 65 (December, 1971), 1048–1062.

[3] Roger Durand's test of the concept against individual-level data has raised serious doubts about its empirical validity. "Ethnicity, 'Public-Regardingness,' and Referenda Voting," *Midwest Journal of Political Science*, 16, 2 (May, 1972), 259–268. And a cogent and well-reasoned analytical incision by Timothy Hennessy has raised serious doubts about ethos theory's conceptual validity. "Problems in Concept Formation: The Ethos 'Theory' and the Comparative Study of Urban Politics," *ibid.*, 14, 4 (November, 1970), 537–564.

Table 1. Characteristics of Voters with "Holistic" and "Localistic" Conceptions of the Local Political Process (Wilson and Banfield's Table 2)

Characteristics	Holistic (*n*-475)	Local-istic (*n*-315)	Non-descript (*n*-269)
**Income			
Under $5,000	13%	26%	23%
$5,000–7,499	17	33	29
$7,500–9,999	24	17	18
$10,000 and over	40	15	25
NA	4	9	7
**Schooling			
Less than high school	27	50	37
High school	30	36	37
Some college	17	8	13
College graduate	26	6	13
NA	0	2	1
**Ethnicity			
Negro	8	17	15
Jewish	24	11	17
Italian	7	18	12
Polish	8	13	8
Yankee	10	2	3
Irish	8	15	11
Other	35	25	34
**Religion			
Protestant	27	21	27
Catholic	44	67	49
Jewish	26	11	18
Other, NA	3	1	5
Immigration generation			
Immigrant	19	27	22
First generation	45	39	45
Second generation or more	32	29	29
NA	4	5	4
Religiosity			
**Strong religious feeling	27	37	32
**Very religious upbringing	47	61	53
**Attend church weekly or oftener	47	67	45

** Percentages significant at the .005 level in the chi-square test.

the concept of political ethos as a means of explaining the impact of sociopolitical forces on the shape and substance of American political life.

As students of ethnicity our primary concern revolves about the ethnic basis of the concept of political ethos. Consequently, our attention will be directed at the istributions of ethnicity and social class and their incorporation into the conceptual structure. We will also demonstrate that the improper calculation of per cents has led Wilson and Banfield to draw erroneous conclusions.

A View from Wilson and Banfield's Individual-level Data

In their most recent article, Wilson and Banfield define each ethos in terms of three attitudinal components, and then describe the incidence of each as it appeared in a sample of voters in a large city a few years ago. One of their purposes is to demonstrate the proportion of the voters having each attitudinal component, and especially the proportion having the "right" combination of attitudes that constitutes an ethos. A second goal is to identify the attributes (especially ethnicity, religion, income, and years of schooling) of voters having the relevant attitudes or combinations of them.

In the execution of the second objective, the causal linkage is lost. It is not political cultural responses that produce sociocultural characteristics; rather, it is sociocultural characteristics which produce political cultural responses. Or, translated directly into the framework of analysis used by Wilson and Banfield: the question is not what proportion of those possessing the attribute came from various groups, but rather what proportion of those in various groups possess the attribute. They note:

> We should expect holists [a term now used to denote "public regardingness"] to be drawn mostly from the upper-middle class and localists [a term now used to denote "private regardingness"] to be drawn mostly from the lower-middle and working classes. In income and schooling, then, holists should be clustered at one end of the scale and localists at another. We should also expect localists to be disproportionately Catholic and Negro. As can be seen from Table 2, [our Table 1] these expectations are in general borne out by the data. Holists have more schooling and higher income than localists. They are five times more likely than localists to be Yankees and twice as likely to be Jewish. Localists on the other hand are twice as likely as holists to be Italian, Irish or Negro. Contrary to what we should expect differences associated with immigrant status are not statistically significant.[4]

We have no quarrel with the authors' reading of their own table, per se. But we emphatically argue that their Table 2 (our Table 1) has put the cart before the horse in reversing the temporal and

[4] Wilson and Banfield, "Political Ethos Revisited," p. 1051. The conclusion based on immigrants is drawn from the lack of statistical significance. We are compelled to call attention to this all-too-common error of using significance tests on data gathered with nonsimple random sample designs. *Cf.* Leslie Kish, *Survey Sampling* (New York: John Wiley & Sons, 1965), pp. 582–593. As for the distinction between immigrant generations, ignoring the issue of calculation, the differences seem to be substantively trivial.

causal sequences of the variables, and as it stands is substantively meaningless.

If the per cents in Table 1 are recomputed in the proper direction, giving due consideration to the causal sequence, then the conclusions reached by Wilson and Banfield, which sustain the ethos theory, are not supported, although some general trends persist. Let us view Wilson and Banfield's data as they appear when properly recalculated in Table 2.

Catholics, contrary to the implications of Wilson and Banfield's expectation and conclusion, have as high a probability of being holistic as they have of being localistic. This fact is far more meaningful than saying, as Wilson and Banfield have done, that localists are disproportionately Catholic and Negro, without even establishing an appropriate frame of comparison by controlling for the proportion of Catholics and Negroes in the population. It is true that Protestants and Jews have a greater tendency to be holistic than localistic. And, if Catholics are compared to Protestants and Jews, they have a lesser tendency to be holistic. But localism per se hardly seems to be a Catholic trait, as Wilson and Banfield imply.

When we move from religion to ethnicity, we find that the Poles and Irish, contrary to Wilson and Banfield's belief, are as likely to have holistic attitudes as localistic orientations. The remaining groups do manifest feelings congruent with the theoretical predictions (although some questions will be raised below about even this). Moreover, it is one thing to note that the Poles and Irish fall below Yankees and Jews in their manifestation of holistic attitudes. It is quite another to argue that sociocultural forces produce a distinct political cultural response when the groups in question are no more likely to manifest one type of political cultural response than the other. If localism is part of what, until the most recent depiction of the theory, has been associated with the immigrant ethos, how does one account for the fact that the immigrant generation itself is just as likely to be holistic as localistic? There is nothing in the recomputed data that suggests that localistic attitudes are a product of a sociocultural force generated by America's experience with latter-day immigrant culture, as the ethos theory would have us believe.

What is most troubling about the Wilson and Banfield analysis is to be garnered from the positive association between income and schooling on the one hand and holistic attitudes on the other. The linear relationship is now observable in the recomputed distributions, and a gamma for the two associations yields +.32 and +.49 respectively.

Consequently, it becomes apparent that any group possessing disproportionate amounts of the two attributes so positively associated with the dependent variable will show a strong holistic response. In such cases, it is not sociocultural forces but income and schooling that contribute to a political outlook that is ostensibly directed toward the interests of society as a whole. The varying distributions of such demographic characteristics between groups is well known, and some partition

Table 2. Characteristics of Voters with "Holistic" and "Localistic" Conceptions of the Local Political Process (Recalculated)

Characteristic	Holistic	Localistic	Nondescript	*N*
Income				
Under $5,000	33%	38%	29%	215
$5,000–7,499	31	40	30*	263
$7,500–9,999	53	25	22	216
$10,000 and over	63	15	22	304
NA	29	42	29	66
Schooling				
Less than high school	33	41	26	386
High school	40	32	28*	355
Some college	57	18	25	141
College graduate	70	11	20*	178
NA	0	66	33	9
Ethnicity				
Negro	29	41	30	132
Jewish	58	18	24	195
Italian	27	47	26	121
Polish	38	41	22*	101
Yankee	77	10	13	62
Irish	33	41	26	115
Other	49	24	27	336
Religion				
Protestant	48	25	27	267
Catholic	38	38	24	552
Jewish	60	17	23	207
Other, NA	47	10	43	30
Immigrant Generation				
Immigrant	38	36	25*	234
First Generation	47	27	26	458
Second Generation or more	47	28	24	321
NA	10	84	6	188
Religiosity				
Strong Religious Feeling	39	35	26	331
Very Religious Upbringing	40	34	26	558
Attend Church Weekly or Oftener	40	38	22	555

* Does not sum to 100% due to rounding errors.

control procedures for these effects are performed by Wilson and Banfield in the course of their analysis. However, the partitioning is of limited value, for the definition of ethnic groups confounds class and ethnicity in the case of Yankees, and the samples of ethnic Catholics and Negroes are restricted in class terms. "Yankee" is defined as a respondent who "describes himself as a Protestant, was born in the United States with both of his parents and at least three out of four of his grandparents born in the United States, had an Anglo-Saxon last name, lived in the Beacon Hill area of Boston, and earned more than $10,000 a year."[6]

While such definition might be appropriate, it sorely confuses the analysis, for one is unable to determine if the decidedly holistic responses of the Yankees are due to an ethnic cultural tradition, to class, or to both, in some interactive setting. On a set of valued normative characteristics which are positively related to income and schooling, the juxtaposition of upper-class Protestants against a socially heterogeneous constituency of other ethnic groups appears to predetermine the results One might inquire whether such analyses unobtrusively say more about the value orientations of the researchers than about sociocultural factors and their impact on American political culture. Indeed, one might ask if we similarly restricted the sample of Poles, Italians, Irish, and Blacks and collected the White Anglo-Saxon Protestants from the varied social strata of the community, might we not have determined that the political culture associated with the impact of the WASP ethos was localistic rather than holistic?

While the authors are able to impose partition design controls for education and income for part of their data and reveal that Catholics are less holistic in attitude than Yankees or Jews, the samples of Negroes, Poles, Italians and Irish are so skewed to the bottom end of the education and income continua that there are not enough respondents for meaningful comparisons with Yankees and Jews. This further illustrates that when the Negro, Italian, Irish or Pole is compared to the Yankee or Jew, it is not simply ethnicity that is being compared but education and income as well. As to what is influencing the response on the dependent variable, one is at a loss to comprehend, for the experimental design is confounded with multiple treatments.

Community Serving vs. People Helping; Good Government vs. Benefits

Although considerations of space preclude a detailed analysis of the other two attitudinal components of the public and private regarding ethics, we might briefly note that these are also handled by Wilson and Banfield with the dependent and independent variables reversed. Recomputing the distributions for community serving vs. people helping does not markedly change the original results. There is, however, a clearer demarcation, once the variables are properly ordered, of the relationship between income and schooling on the one hand and community serving on the other. Gammas for this pair of relationships are +.35 and +.55 respectively.

Focusing on the recomputing of the percentages for good government vs. benefits, we note some important differences with the original version. The original version saw no relationship between ethnicity and attitude, except for the Jews and Italians, who were benefit minded. The recomputed version shows ethnicity is related to attitude, but in a fashion that differs from the results deduced from ethos theory. Poles tend to be incredibly good-government minded, almost as much so as Yankees. The Irish follow suit, and the Negroes are very close to them in attitude preference. Moreover, Wilson and Banfield's conclusion that the benefit minded are disproportionately Italian is untenable when the data are recalculated.

The assault on the ethnic component of the theory, already manifest in the above distributions, is reinforced by the patterns for the immigrant generation. More than 60 per cent of the immigrant generation is good government minded. Consequently, it is difficult to see how ethos theory is a depiction of the impact of an immigrant-based, sociocultural force on the political environment.

Ethos and Attitudes

The next step of Wilson and Banfield's analysis is to combine the three attitudinal components that comprise the two ethics—holistic vs. localistic, community serving vs. people helping, and good government vs. benefits—and look at the demographic distributions over the total index. We not only fault this procedure because it once again confuses the dependent and independent variables, but it also assumes that the three attitudinal components are conceptually related when only face validity has been established. In fact, the authors' correlations between holism and community serving is only +.135 and between holism and good government is a trivial +.058. This is hardly the kind of consistency that warrants an index, and argues rather substantially in favor of the conclusions presented by Hawkins and Prather. Their factor analysis demonstrated factorial separability, with private regardingness, localism, intolerance of conflict and opposition to good

[6] Wilson and Banfield, "Political Ethos Revisited," p. 1050.

government loading on independent factors.[6] Rather than follow the logical conclusions of their own correlation coefficients, which mesh with Hawkins and Prather's multivariate analysis, and accede to the guidance of the data, Wilson and Banfield refuse to acknowledge that the three attitudinal components do not share the same conceptual space and, hence, do not constitute an index. Instead, they incredibly throw out 88 percent of their 1,059 respondents and look (again backwards) at the characteristics of those voters for whom there is some sharing of the conceptual space for the three attitudes.

Of course, we are not too surprised that Table 3 (Wilson and Banfield's Table 8) demonstrates the Yankees and Jews to be disproportionately public regarding (or unitaristic as it is now called), and the immigrant Catholics and Negroes to be disproportionately private regarding (or individualistic), and the theory of political ethos is confirmed. This, however, is sheer nonsense. When Table 3 is properly recomputed (see our Table 4) it sharply underscores the notion that Yankees and Jews have a unitary ethos while the "newer races" (as the authors, taking a leaf from James Curley, refer to the immigrant Catholic groups) and Negroes have an individualistic ethos. Let us look first at the one case which is most distorted by the authors' miscalculations. This is the case of the Polish respondents. Although none of the Polish respondents demonstrated an individualistic ethos and every single Polish respondent did manifest a unitary ethos, the erroneous calculation of percentages cause the authors to conclude, "Table 8 [our Table 3] shows that the unitarists include a disproportionate number of Yankees and Jews and also of the well-schooled and well-off; it shows also that the individualists include a disproportionate number of Irish, Italian and Polish Catholics, as well as the less well-schooled and the relatively low income. . . ."[7]

Looking at the data when they are properly computed one can see that Wilson and Banfield are incorrect in their assessment not only of the Poles but of the Irish and Negroes as well. Two-thirds of the Negroes and two-thirds of the Irish demonstrate the unitary ethos, and the Jews are no more likely to be unitarists than are the Irish or Negroes.

Another crack in the theoretical structure is exposed when the distributions of immigrants are viewed. The private regarding (individualistic) ethos is supposed to be characteristic of the impact of the sociocultural forces of latter-day immigration on the political structure. But how can this be the case if 60 percent of the immigrant

Table 3. (Wilson and Banfield's Table 8) Characteristics of Voters Who Are "Holistic/Community-Serving/Good Government-minded" or "Localistic/People-Helping/Benefit-Minded"

Characteristics	Holistic/Community-Serving/Good Government Minded ($n=97$)	Localistic/People-Helping/Benefit-Minded ($n=26$)
Income		
Under $5,000	12%	27%
$5,000–7,499	8	15
$7,500–9,999	17	19
$10,000 and over	59	31
NA	4	8
**Schooling		
Less than high school	12	27
High school	22	54
Some college	19	8
College graduate	46	8
NA	1	4
**Ethnicity		
Negro	4	8
Jewish	14	35
Italian	3	23
Polish	9	0
Yankee	24	0
Irish	8	15
Other	37	19
**Religion		
Protestant	42	15
Catholic	38	46
Jewish	18	35
Other, NA	2	4
*Immigrant generation		
Immigrant	11	27
First generation	35	54
Second generation or more	47	15
NA	6	4
Religiosity		
Strong religious feelings	24	23
Very religious upbringing	44	54
*Attend church weekly or oftener	49	54

* Percentages significant at the .05 level in the chi-square test.

** Percentages significant at the .005 level in the chi-square test.

[6] Brett W. Hawkins and James E. Prather, "Measuring Components of the Ethos Theory: A First Step," *Journal of Politics*, 33, 3 (August, 1971), pp. 642–658.

[7] Wilson and Banfield, "Political Ethos Revisited," p. 1057.

Table 4. Characteristics of Voters who are "Holistic/ Community-Serving/ Good Government-Minded" or "Localistic/People-Helping/Benefit-Minded" (Recalculated)

Characteristics	Holistic/ Community-Serving/Good Government Minded ($n=97$)	Localistic/ People-Helping/ Benefit-Minded ($n=26$)	N
Income			
Under $5,000	63	37	19
$5,000–7,499	67	33	12
$7,500–9,999	77	23	22
$10,000 and over	88	12	65
NA	67	33	6
Schooling			
Less than high school	63	37	19
High school	60	40	35
Some college	90	10	20
College graduate	90	4	47
NA	50	50	2
Ethnicity			
Negro	67	33	6
Jewish	61	39	23
Italian	33	67	9
Polish	100	0	9
Yankee	100	0	23
Irish	67	33	12
Other	88	12	41
Religion			
Protestant	91	9	45
Catholic	76	25*	49
Jewish	67	33	27
Other, NA	67	33	3
Immigrant Generation			
Immigrant	61	39	18
First Generation	71	29	48
Second Generation and more	92	8	50
NA	86	14	7
Religiosity			
Strong Religious Feeling	79	21	29
Very Religious Upbringing	75	25	57
Attend Church Weekly or Oftener	77	23	62

* Does not sum to 100% due to rounding error.

generation already possess a public regarding (unitary) ethos?

The recomputed table runs strongly counter to the argument sustaining the political ethos theory; however, we must not lose sight of the criticism we leveled when we opened this section. Specifically, the amalgamation of the three items does not constitute an index, and by performing the analysis on the twelve per cent of the sample displaying a consistent pattern among the three items, the authors appear to be measuring the attitudinal structure of those respondents who have been able to form a consistent cognitive pattern. Such respondents are atypical,[8] and perhaps this accounts for the pattern which emerges within each category of Table 4 and across the entire table. Namely, public-regarding (unitaristic) responses occur disproportionately within virtually every group, and 79 percent of the respondents demonstrate this response. Consequently, Table 4 may well be saying far more about the political cultural patterns of people capable of cognitive consistency than about the relationship between sociocultural forces and political attitudes. *For it is rather intriguing that people with consistent cognitive patterns are disproportionately unitarists regardless of income, schooling, ethnicity, religion, immigrant generation or strength of religiosity.*

Conclusion

The general impression the reanalysis imparts is that the ethnic basis of political ethos is not sustained by individual-level data marshalled in its behalf by its creators. Looking at the reanalysis in the light of existent theoretical and empirical criticism, it is clear that the concept of political ethos has led to an unwarranted oversimplification of the influence of sociocultural characteristics on political behavior.[9] Indeed, it is not inappropriate to ask whether or not such basic methodological flaws as are evident in "Political Ethos Revisited" are a function of the authors' preconceived impressions of the political cultural manifestations of America's experience with unrestricted European immigration. Instead of the data guiding the analysis, it appears that the

[8] A substantial body of literature has been developed which indicates that highly differentiated, wide-ranging, and coherent political belief systems of the type Wilson and Banfield refer to are generally restricted to the upper SES echelons of the populace. *Cf.* e.g., Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. David E. Apter (New York: The Free Press, 1964), pp. 206–261; Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, 1960), pp. 168–265; Herbert McClosky, Paul J. Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus among Party Leaders and Followers," *American Political Science Review*, 54 (June, 1960), 406–427; and Herbert McClosky, "Consensus and Ideology in American Politics," *ibid.*, 58 (June, 1964), 361–379.

[9] *Cf.* Herbert Kaufman, "Review of *City Politics*," *American Political Science Review*, 58 (June, 1964), 422.

authors' theoretical impressions have guided the data.

ABRAHAM H. MILLER
STEPHEN E. BENNETT
University of Cincinnati

TO THE EDITOR:

Like others before them, Professors Miller and Bennett have rushed in where they should fear to tread. They write, correctly, that one of our objects was to identify the attributes (ethnicity, religion, and so on) of voters having certain attitudes or combinations of them. They then find that in the "execution of this objective the causal linkage is lost": we failed to address ourselves to the proper question, namely, what proportion of those in the various attitudinal groups possess the listed attributes? By calculating percentages to sum across rather than down the tables they claim to have restored the "causal linkage" and disconfirmed our findings.

Apparently Professors Miller and Bennett either think that we asserted the existence of a causal linkage between attributes and attitudes or that whenever two variables are found to be associated there *must* be such a linkage. Whichever they think, they are mistaken. We did not assert such a relation; on the contrary, throughout the article we were at pains to speak of the variables only as "associated." Any text on statistics will explain, most probably in the introductory chapter, that association does not entail causality.

Our critics wish to impose on us the logic of causal analysis, to insist that ethnicity and other attributes be considered the independent variables, and to require that our theory be rejected if a *majority* of any given ethnic (or religious, or income, or educational) group does not display the predicted attitude. This is not our view. We think that the relationships among attributes and attitudes are so complex and our knowledge so incomplete as to make any choice of an "independent" variable arbitrary, and we reject as unscientific—indeed, as silly—the claim that the association among variables is meaningless unless a majority of respondents having a particular attribute have the expected attitudes.

Professors Miller and Bennett seem unaware that some social science is causal-analytic and some historical-descriptive. Once or twice we stepped close to the line and we may have stepped over it momentarily when we referred to a "quasi-logical" relationship among attitudes, but in all our writings we have repeatedly made it plain that our theory of the association of attitudes is historical rather than causal. We have, for example, remarked that the association of certain attitudes with certain ethnic groups appears to have weakened over time in consequence of social mobility, mass education, the decline of the reform movement, and other historical developments. This being the sort of theory that we have offered, it is entirely proper for us to ask: "What are the social characteristics of individuals who hold particular attitudes?"

If our theory is properly understood, the criticisms that Professors Miller and Bennett make of our tables are all irrelevant.

Let us, however, pursue the (incorrect) assumption that the theory is a causal-analytic one. Would it follow that, when the tables are recalculated and displayed with the percentages summing across, the data provide evidence for rejecting the theory? Professors Miller and Bennett answer yes on the grounds that in the groups which we hypothesized would display individualistic attitudes, more than half the respondents failed to do so. The speciousness of this claim is obvious. Suppose we had hypothesized that sickle-cell anemia is associated with being a Negro and, on the basis of a random sample of the whole population, had found that only 10 per cent of Negroes have the disease. Professors Miller and Bennett would say, "Sickle cell anemia per se hardly seems to be a Negro trait," but they would be wrong if our data showed the incidence to be significantly greater among Negroes than others. It is clear from our analysis (but obscured in their recalculated tables) that this is the case with the groups that we hypothesize will display individualist attitudes: they *do* display them more frequently than do the groups we hypothesize will display unitarist ones.

Take another example, perhaps closer to the collegiate heart. It can be shown that the proportion of persons saying that the penalty for using marihuana should be less severe increases with higher levels of education (Erickson and Luttbeg, *American Public Opinion*, p. 171). At no educational level, however, does a majority of respondents favor the reduction in marihuana penalties. Furthermore, there is no way of being certain which variable—education or attitude toward marihuana—is "independent." Marihuana smokers may be more likely to go to college, college students may be more likely to learn to smoke it, or college graduates may learn tolerance for marihuana though they do not use it themselves. Perhaps all three relationships hold. But whatever the relationship, surely Professors Miller and Bennett would not deny the association between education and attitudes toward marihuana smoking simply because a majority of college graduates do not favor reducing the penalties.

Professors Miller and Bennett should not be so concerned with the absolute percentages. We have never claimed that a *large* proportion—say a

majority—of this or that group has this or that ethos. Instead we have said that we expect to find this or that ethos more closely associated with some groups than with others. Even when rearranged, the data support this hypothesis.

We acknowledge that we misinterpreted Table 8 with respect to Poles and Jews, but the matter is clarified in our discussion of Table 9, and the major conclusions of the article remain unaffected.

One other criticism has some—but not much—merit. In footnote 4, Professors Miller and Bennett say that our significance tests are inappropriate because our data were not gathered by means of a simple random sample. It is true that a simple random sample of homeowners was supplemented with a quota sample of certain ethnic groups. To be methodologically beyond reproach we should have weighted the supplemental sample so that the incidence of relevant characteristics would be the same in it as in the simple random sample. We doubt that our failure to do so materially affected the measures of significance, however, and, in any case, the idea of statistical significance was of small importance in our argument. Indeed, not being uncritically enamoured of tests of significance, we included them in these tables only at the insistence of this *Review*. We should add, lest some readers be unaware, that tests of significance and such measures of association as *gamma* are not affected by the direction in which a table adds to 100 or by how the table is "recomputed."

We feel obliged to call attention to the unpleasant implications of the next to last sentence in Professor Miller and Bennett's comment. If they are suggesting that our views on ethos are the product of a preconceived hostility toward European immigrants, they are wrong, and no fair reading of our works could possibly support such a view.

EDWARD C. BANFIELD
University of Pennsylvania
JAMES Q. WILSON
Harvard University

TO THE EDITOR:

Among the many interesting points at issue in the Katz-Stokes debate on components of variance in congressional elections (*APSR*, 67 [September, 1973], 817–34), one seems susceptible of solution. I refer to the bias in Katz's estimates due to small sample size.

The Katz method is based on regression analysis. In the first step, a congressional district's Republican vote percentages in five elections are made the dependent variable, and the corresponding national vote percentages serve as the lone independent variable. The resulting R^2 in each district is then taken as an estimate of the per cent of the variance "explained" by national forces, and these are averaged to form a nationwide estimate.

Such an estimate, as Stokes points out, is biased upward. In the case where the residuals are normally distributed, A. P. Barten, "Note on Unbiased Estimation of the Squared Multiple Correlation Coefficient" (*Statistica Neerlandica* 16 [1962], 151–63), has provided an improved estimator, unbiased to order n^{-1}. The new estimator, R_1^2, is defined as:

$$R_1^2 = 1 - (1 - R^2) \cdot \left[1 + \frac{K - (1 - R^2)(1 + 2R^2)}{T}\right],$$

where R^2 is the uncorrected squared multiple correlation coefficient, T is the number of observations, and K the number of independent variables, including the constant term. This formula allows us to calculate unbiased estimates of the "variance explained" for any uncorrected estimate of R^2 obtained from a regression analysis. With $T=5$ and $K=2$, as in Katz's case, some representative values are as follows:

Uncorrected R^2	Corrected R_1^2
1.00	1.000
0.90	0.866
0.80	0.741
0.70	0.623
0.60	0.510
0.50	0.400
0.40	0.290
0.30	0.177
0.20	0.059
0.15	0.000

In the case of Katz's nationwide estimate of the national component of the variance, his uncorrected estimate is 0.546. Use of Barten's formula shows that an unbiased estimate is 0.451, meaning that, on Katz's assumptions, 45.1 per cent of the variance should be attributed to national forces. This corrected estimate falls approximately halfway between Katz's uncorrected value and Stokes's earlier estimate, which was based on different techniques and assumptions.

Actually, even 45.1 per cent is too high an estimate, since 0.546 is an *average* of uncorrected R^2's. Because Barten's formula is nonlinear in R^2, applying it to the uncorrected average will not generally yield the desired quantity, namely, the average of the *corrected* R^2's. To obtain this latter value, Barten's formula would have to be applied *before* averaging Katz's constituency-level R^2's. The result should be a few points lower,

yielding an unbiased estimate of the per cent of the variance explained by national forces in Katz's model of perhaps 42 per cent or 43 per cent. By contrast, Stokes's somewhat simpler model employs the usual techniques for estimation of a variance, and his basic estimates are unbiased. (However, if serial correlation is present, as seems possible with this times-series data, both Stokes and Katz are subject to an additional upward bias.)

Of course, this correction is a technical one in what remains a fresh and significant generalization of Stokes's work. And none of Katz's substantive conclusions are affected.

CHRISTOPHER H. ACHEN
The University of Rochester

TO THE EDITOR:

Two observations are, I think, in order with regard to Achen's suggestion. The first is that it only applies if one accepts the assumption that the elections analyzed are a random sample drawn from a large universe. If they are a "better" than random sample, or no sample at all, then the bias resulting from small sample size will be considerably smaller than the correction proposed. Nonetheless, this correction may offer an increase in comparability when data from two series of different lengths are to be analyzed.

The second observation is that even after the substantial correction which Achen suggests has been made, the national variance component which my method yields is still considerably larger than that which Stokes reports, as is only to be expected considering the differences in our models. In this respect, Achen's correction underlines the fact that, quite aside from limitations of methodology, different theories will give different results.

RICHARD S. KATZ
Queens College, CUNY

TO THE EDITOR:

Peter Eisinger's article, "The Conditions of Protest Behavior in American Cities" (*APSR*, 67 [March, 1973], 11–28) points out that the correlation between the number of protests and the population of cities is positive (.46). This is not surprising, since one would expect to find more protests where there are more people to protest. The dependent variable of interest would be the number of protests per capita, or perhaps the number of protesters per capita. Unfortunately, if its use yields significantly different results in the above-noted correlation, the correlations of other variables with the raw number of protests are matters of indeterminate meaning whenever these other variables are correlated with population. Quick calculation reveals that the correlation between protests per capita and city size in the cities examined in the article is *negative*—approximately −.28. This figure might be interesting and suggestive in some way to students of protest behavior and city government, but, sadly, unless the other variables examined are uncorrelated with city size, it probably indicates most of the article is invalid. I don't have the data to ascertain this with certainty, but Dr. Eisinger will probably be able to do the necessary testing rather easily. There are other matters in the article which might provoke discussion, but it is senseless to raise them in the face of this problem.

KEN ALLEN
University of Maryland Baltimore County

TO THE EDITOR:

I think that the point of Ken Allen's criticism is that most of the correlations upon which I base some of my argument may reflect the effects of city size, thus making the relationships I discuss in my article spurious. By controlling for city size, however, we find that in most cases the relationships between the different independent variables and the protest incidents still hold, as the table shows. In a number of cases—notably where black protests are concerned—the relationships are stronger when size of city is controlled.

Both the zero-order and the partial correlation coefficients are small, as I recognize in my article.

Table 1. Zero-order Correlations (r) and Partial Correlations Controlling for City Population Between Protest Incidents and Selected Independent Variables

	All protest incidents		Black protest incidents	
	r	Partial *r*	*r*	Partial *r*
Percentage black	—	—	−.02	.43
Number of blacks	—	—	.14	.38
Model Cities grant	.26	.16	.23	.35
Number of Black Elected officials	—	—	.07	.34
MPO ratio	−.30	−.33	−.28	−.33
Percentage of minority police	.20	.28	.03	.31
Mayor-Council government	.35	.25	.42	.14
Ward Aldermanic elections	−.01	−.07	.08	.02
Partisanship	.07	.17	.11	.17
Incidence of riots	.23	.07	.22	.07
Black representation ratio	—	—	.30	.54

Nevertheless, set in the theoretical context of my argument, they make considerable sense. I make quite clear that I have used the statistics in a cautious way. I think the reader is not required to suspend belief.

In regard to other parts of the letter, I suppose I agree with Professor Allen that the number of protests per capita might be interesting in itself, but I do not see how it fits into my argument regarding opportunity structures. If he has suggestions, I would have appreciated them. All he has done is to assert that the number of protests per capita is "the dependent variable of interest." For whom is it interesting and why?

Second, I find it odd that the writer would dismiss "most" of the article on the basis of a negative correlation involving a variable he does not relate to the argument. I not only manipulate my data in other ways which support the argument, but I suggest a perspective on protest in a discussion which comes prior to the data analysis and, I think, probably merits some discussion. Professor Allen takes an easy and, perhaps, irresponsible route when he claims that it is "senseless" to discuss other provocative matters in the article in the face of the negative correlation he finds.

PETER K. EISINGER
University of Wisconsin, Madison

TO THE EDITOR:

In her review of my book *The Private Nuclear Strategists* (*APSR*, 67 [December, 1973], 1377–78), Anne Cahn states that it lacks feeling for its subject and is mechanically sloppy. I feel rather the same way about her review, since her specific criticisms are misguided, and she totally ignores the book's content, focus, and conclusions.

One major criticism is that the book represents "blind or crude empiricism." The example cited is a factor analysis with factors which do not seem to make much sense; Cahn thinks I should have explained them. In fact I concluded that there was no clear pattern, so it would be absurd to analyze the separate factors. A related charge is that a large number of tables are included which are not statistically significant. My rule of thumb was to include such tables when they contradicted hypotheses which had been advanced elsewhere.

Cahn points out that the book does not deal extensively with the RAND Corporation or the Summer Study-Daedalus-Brennan volume. This is true. But chapter five studies how people employed by different sorts of institutions, including non-profit research institutes, differ from one another. (The differences tend to be small, producing more of the "not significant" tables which Cahn dislikes but which I find substantively interesting.) More importantly, Cahn misses the point. The book is not an intellectual history, although it naturally draws on the work of Lyons and Morton, Smith, Brodie, Kidd, Green and Gray in this genre.

Four examples are cited in support of the opinion that "senseless data manipulation is here compounded by carelessness." While the book is not particularly easy to read, the carelessness in several of these problems is Cahn's rather than mine. As explained on page 94, 14 per cent of the respondents could not be usefully classified and were therefore omitted from part of the analysis; thus in Table 31 the percentage figures do total 86 per cent and Table 38 cites a total N of 145 even though the columns add up to 125. In the same table the significance figure of .02 concerns the difference between the disciplinary affiliation of highly influential respondents and the rest of the sample; it is therefore calculated with a N of 177, not 9. The fourth point is more substantial but is a data presentation problem, stemming from my attempt to reduce the sheer amount of data in the book; I will be glad to send the figures to anyone interested.

More important than these particular points, however, Cahn's review manages to totally ignore the substantive questions discussed in the book, such as: who is influential within the field, the role of ex-military personnel, motives for entering the field, whether the motives are fulfilled, differences among individuals employed by different types of institutions, the role of academic disciplines, part-time vs. full-time work, corporateness, moral responsibility and research contracts, future supply and demand, and the significance of the private nuclear strategists for American democracy. None of these is even mentioned in this review. If one test of a book review is that it accepts a book on its own terms and evaluates how well it accomplishes its author's goals, this one is a clear failure. Ole R. Holsti demonstrates that this can be done with this book in his review in *Science*, September, 1972.

ROY E. LICKLIDER
Douglass College
Rutgers University

TO THE EDITOR:

Professor Licklider's first few paragraphs elucidate the arguments of my review better than the review itself did. As for the substantive questions he feels the review should have been addressed to, a few quotes from his book might illustrate why they weren't.

Q: Who is influential in the field?
A: p. 165—"Because of the nature of the study, the amount of influence the private nuclear strategists have had on American policy

in strategy and disarmament cannot be determined."

Q: The role of ex-military personnel?

A: p. 51—"It would be unwise to overestimate the magnitude or significance of the differences between the ex-military and the remainder of the population."

Q: Motives for entering the field?

A: p. 54—"The motives for entering the field are undoubtedly as numerous as the population."

Q: Differences among individuals employed by different types of institutions?

A: p. 75—"Differences in motivation between individuals employed by different institutions are unimportant."

ANNE H. CAHN

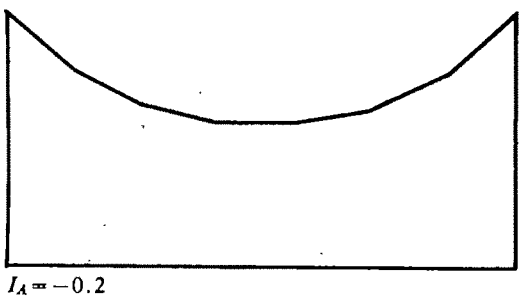
Harvard University

TO THE EDITOR:

How can Peter Taylor (*APSR*, 67 [Sept., 1973], 948) claim that the number of reflex angles of a closed shape cannot be larger than the number of nonreflex angles? Consider a district of which three sides are like three sides of a rectangle, and the remainder of the boundary approximates a semicircular indentation (Fig. 1). Such a figure clearly has four nonreflex angles. If the remainder consists of more than five line segments, there will be more reflex than nonreflex angles, and Taylor's proposed I_A will drop below zero—in principle, arbitrarily far below zero.

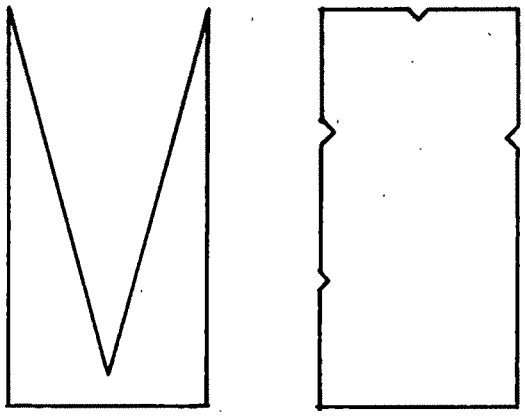
Such a shape presents a special case of problems arising because I_A is insensitive to distinctions among angles beyond the dichotomy of reflex and nonreflex. I_A therefore evaluates a district with one deep indentation (e.g., Fig. 2A) more compact than one with several shallow indentations (e.g., Fig. 2B). I_A also suffers as a summary measure of compactness because it is insensitive to elongation and noncontiguity, listed by Taylor as criteria of noncompactness. Neither a very long ellipse nor two circles have more indentedness than one circle.

On the other hand, I question whether puncturedness is necessary as a criterion of noncompactness, as Taylor proposes. Enclaves within a district either represent unalterable boundaries, in which case they need not be considered, or are detached portions of another district, in which case they already affect measures of that other district's compactness, or are entirely enclosed districts such as Colorado's First Congressional District (Denver), in which case they still affect measures of compactness for the surrounding district based on other criteria, including Taylor's I_A.



$I_A = -0.2$

Figure 1



$I_A = 0.6$ $I_A = 0.5$

A B

Figure 2

In light of the above, I doubt not only that I_A satisfactorily measures compactness generally (which Taylor does not claim), but even that it satisfactorily measures indentation. Although my objection might be met by taking into account the magnitude of the angles, the ratio of lengths of indented sides to total perimeter, and similar quantities, to do so would render the measure complex. One might also measure indentation (and compactness generally) by the proportion of the area of the smallest circumscribed circle lying outside the district (Fig. 3). But it is not clear to me that measures of aggregate divergence from center of gravity, which Taylor mentions, are not equally satisfactory indicators of compactness.

A simpler and more elegant measure than any of these is the ratio of a district's area to its perimeter. This quantity is minimized in the circle, the most compact shape possible. Since the area of a circle, A, equals πr^2, and its circumference, C, equals $2\pi r$, it follows that $A/C = r/2$. Therefore, for a circle, $2A/rC = 1$. For any other shape, calculate the radius of a circle of the same area as the shape: $r = \sqrt{A/\pi}$. Then, letting P = perimeter, $2A/rP$ approaches 1.0 as the shape approximates a circle, and 0.0 as it becomes less compact. Thus the maximum of this measure is



Figure 3

one and its minimum zero, as Taylor desires, and the measure is sensitive to all his criteria of noncompactness.

If a mathematically natural measure of compactness is desired, this ratio is the one I would unhesitatingly propose. On the other hand, it may be felt that a measure of compactness in politics should evaluate not geometric relations, but the locations of inhabitants of the district. After all, politics is concerned with people, as Taylor recalls to us by saying, "very often the shape of districts has been associated with attempts to produce areas within which contact is relatively easy. This contact in turn may foster a sense of community and identity" (p. 948). The geographic appearance of a district on a map is merely a surrogate for such considerations. In increasing order of appositeness to political concerns, a measure of compactness could express the average distance of citizens from: a) center of gravity; b) center of population; c) business or civic center (or central polling place, if any); d) each other.

Concern for community could be further taken into account, and the effects of indentation implicitly recognized, by measuring the relevant distances not by geometric straight lines, but over existing roads lying entirely within the district. Appropriate simplifications could be introduced to make the calculations manageable (e.g., calculating only distances among centers of subcommunities). Finally, such a measure would have to be standardized to the geographic extent of the district, for which density of population might be an appropriate criterion.

RICHARD S. BETH

Boston University

TO THE EDITOR:

I read the comments on the shape measure article with interest. This reply is not so much a rejoinder, but rather an explanation and suggested solution. Of course Figure 1 clearly shows that I_A, as defined in *APSR*, 67:3, 948, can fall below zero and as such is quite unsatisfactory. I must thank the author for pointing this out to me. Quite simply there was a hidden assumption in my reasoning which should have been made explicit. In all the shape experiments carried out in this work, irregular shapes were approximated by polygons made up of straight lines *of equal length*. In this special situation it is correct to say that the number of reflex angles cannot exceed the number of nonreflex angles and $I_A > 0$.

When we generalize an electoral district boundary as a series of straight line segments between base area junctions, the above assumption of equal lengths is normally violated and so the inappropriateness of I_A in this context still obtains. However, now that we have uncovered this assumption, it does lead us to a solution to the problem. Quite simply we define a *weighted* I_A value where each angle is weighted by the total length of the two sides which define it. If we define l reflex angles and m nonreflex angles and measure the total length of sides defining each angle (W_i) then

$$R = \sum_{i=1}^{l} W_i$$

$$N = \sum_{i=1}^{m} W_i$$

We can now define a weighted measure

$$I_A = \frac{N - R}{T},$$

where $T = N + R$ as before.

We can illustrate this weighted measure of indentation by using Figure 1. In this case there are six reflex angles each of which are defined by two lines of total length 0.8 inches. Hence $R = 4.8$. The four nonreflex angles are weighted 1.65, 3.5, 3.5, 1.65 so that

$$N = 10.3. \qquad T = 15.1;$$

$$I_A = (10.3 - 4.8)/15.1 = 0.36.$$

This weighted measure of indentation also solves the criticism of unweighted I_A illustrated in Figure 2. Using the weighted approach these shapes now produce values of $I_A = 0.59$ and $I_A = 0.75$ for diagrams A and B respectively.

Finally, let me reiterate the purpose of the original article. It was to produce a simple measure of compactness in terms of divergence along an indentation scale. Thus elongation and other shape concepts are not measured by I_A. The above modifications, and those suggested in the comments above, complicate this measurement

exercise so that it can no longer be carried out by inspection from maps. However, even in its weighted form I_A is a relatively easy measure to compute and work with.

PETER J. TAYLOR
University of Newcastle upon Tyne

TO THE EDITOR:

Just a note to let you know that the highly unprofessional review of my *The Higher Circles* in your journal last year by Earl Latham did not go unnoticed. At the time it appeared, though, I thought it better to ignore such a nasty little diatribe. Lately, however, I have been in a more combative mood toward critics, and I would like to say a few words in response to the most unfair of Latham's many unfair remarks. These remarks have to do with my alleged affinity for right-wingers.

Latham gets in many little digs on this subject in his original review, but his most tasteless remark along these lines is in his reply to the criticism of his review by Daniel Hoffman. In this reply he says I differ from ultra-conservatives only in that I think they "go too far when they talk about an 'international Communist-Jewish conspiracy'." But my chapter on ultra-conservatives shows I think the ultra-conservatives are totally wrong conceptually even though they have more or less pinpointed the "who" of the power elite. On page 286, early in my essay, I say I am going to show how I differ from them by analyzing their root concept, "the international Jewish-Communist conspiracy." I then analyze each aspect of this concept and conclude on page 307 that "In my view, then, there is nothing like an international Jewish-Communist conspiracy, although I do not dismiss the notion out of hand because it is supposedly the emanation of a particular style which is said to be more associated with some ideas than others and with more bad causes than good ones." This is clearly not a matter of their going "too far." I differ totally from them in my theory, and Latham goes beyond the boundaries of fairness in his distortions.

Indeed, a main point of the chapter on ultra-conservatives was to show the utterly irresponsible, ad hominem way in which pluralists have treated ultra-conservatives. I had come to realize the nastiness of this psychologizing because of the fact that I was being treated in the same way by pluralists, and I thought that a serious essay on the ultra-conservatives might jolt pluralists into an awareness of just how badly they behave towards anyone who dares to disagree with them. In the case of Earl Latham, my plan obviously did not work.

Now I want to turn to a gross distortion in Latham's original review. In this instance he accuses me directly of being like an ultra-conservative: "Like the McCarran committee also, Professor Domhoff is suspicious of the academy, and in a piece of insinuation worthy of *his predecessors* [my italics; GWD] in the early 1950's he says, 'While I do not believe for a minute that the power elite dictate to those scholars [who participate in CFR activities] as to what they should say, it should be clear that members of the power elite see no reason to discontinue their support of such efforts'." The reader should reread the brackets in that sentence, which were added by Latham, for I will show that they are a distortion of the "scholarly" activities to which I am making reference.

In any event, this one sentence by Latham contains so much malice that I hardly know where to begin. First, he does not let his readers know that the sentence followed a brief discussion of secretly financed books that scholars write for the CIA, producing one version for the CIA and a "sanitized" version for the general public! Thus, his readers did not know that my readers at that point would be thinking: does the CIA dictate what these scholars write? If these scholars are so compromised that they do not openly present all their results to the public, have they taken the next step and written what they have been told to write? I answer the implicit question by saying No, and I wrote the sentence Latham quotes in preparation for a quote from none other than David Easton, who expresses my view perfectly on this matter. But Latham does not let his readers know that he is taking on David Easton when he mocks my view. For what followed the sentence Latham ripped out of context was the following very reasonable sentence: "The relationship has been explained by political scientist David Easton: '. . . a deeper social reason for the failure of political science to transcend its limitations . . . lies in the proximity of political research to social forces that determine social policy. . . . Entrenched power groups in society, those who have a firm hold on a particular pattern of distribution of social goods, material and spiritual, have a special reason to look askance at this probing into the nature and source of their social positions and activities. They are prone to stimulate research of a kind that does not inquire into the fundamentals of the existing arrangement of things' " (pp. 127–128). If I can still read the English language, Easton has said in a nice way that political science is a superficial discipline which supinely avoids research into anything its corporate masters don't want studied.

So, consider. Professors are lending themselves to the undercover purposes of the CIA, David Easton says that political science doesn't probe very deeply into the existing arrangement of

things. And reviewer Latham concludes that I am a McCarranite who unfairly attacks professors!

But there is more. Latham not only left out sentences, he distorted the sentence he quoted, for he inserted brackets into my sentence in which he says my remarks refer to Council on Foreign Relations activities. That is a complete distortion, and I think Latham owes me a personal apology for making it. To repeat, my sentence was in the context of professors who write the "real" thing for the CIA and a "sanitized" version for the public, not in the context of the open relationships to CFR and its study groups. By making this false insertion into my sentence Latham takes the spotlight off the underhanded behavior of orthodox liberal professors and tries to put it on me as an alleged McCarranite. Latham is too preoccupied with saving our "pluralistic democracy" to worry about the fact that our "liberal" colleagues are busily undermining the values of American academia and, indeed, as I show in a later chapter of the same book, the open nature of the society. Despite all the startling revelations about the academy between the years 1965 and 1969 (the years before the book appeared in 1970), Latham can only think of my cynicism about the alleged highmindedness and decency of my fellow social scientists in terms of the ultraconservative attacks of the 1950s.

I will not in this letter try to respond to Latham's many distortions and mockeries concerning my work on social class, class cohesion, and policy formation. For those of you who are interested in seeing for yourselves whether or not Latham is a fairminded critic of my efforts, you can read my most recent and brief statement of them in the third and final chapter of *The Bohemian Grove & Other Retreats: A Study in Ruling-Class Cohesiveness* (Harper and Row, 1974). You also can read my brief chapter in a book edited by former Senator Fred Harris which is scheduled to appear in 1974 with Grossman. I think that people who take the trouble to read either of these chapters with an open mind will find that I have a clear and cogent set of disposition concepts that I have empirically supported by a variety of operations and indicators.

G. William Domhoff
University of California, Santa Cruz

To the Editor:

In the phrase of John Marshall, it is an "absurdity too gross to be insisted on" for Professor Domhoff to suggest that a review printed in 1972 about a book published in 1970 must have been unfair in 1972 because of something that Professor Domhoff said in 1974, or was about to say in 1974. Perhaps this confusion about what time it is explains his very belated response to the original review, a response littered with shards of accusation ("tasteless," "unfair," "nasty," "unprofessional," "distortion," "malice," etc.). Good manners require us all to overlook these lapses of self-control, and charity will surely enfold them into the forgetfulness that much of his book deserves.

The gravamen of Professor Domhoff's complaint is evidently that he is not now nor has he ever been an ultraconservative ("my alleged affinity for right-wingers"), and he thinks that I have not made this clear. Now, it is pure tedium to engage at any length in a "He said—But I said" exchange over a few sentences or phrases, especially when the complaining party is as often off point as a ballerina in ski-boots. But in the interest of accuracy, posterity should note that I did not say in the missive to which Professor Domhoff refers, that I thought he differed from ultraconservatives "only" in that he thinks that they go too far in talking about an international Communist-Jewish conspiracy. I have no doubt that there are other differences, but he *does* do them honor when he agrees with them, that "they have more or less pin-pointed the 'who' of the power elite." And, as to the "conspiracy," he repeats the statement in his book that he "does not dismiss the notion out of hand." I find this to be a very curious admission, and I find no clarification in the rest of the sentence from which the phrase is quoted. He says that "it" (the "notion" of an international Communist-Jewish conspiracy) is not dismissed out of hand because it "is supposedly the emanation of a particular style which is said to be more associated with some ideas than others and with more bad causes than good ones." I do not think that this clot of words is biodegradable. In any case, I had a little trouble finding it again because the page reference in his letter is wrong.

The confessions in the third paragraph of Professor Domhoff's letter are interesting, but only faintly. He came to realize how nasty the "pluralists" were to the ultraconservatives because he "was being treated in the same way" by the "pluralists," another rather odd association of himself with ultraconservatives. He intended then "to jolt pluralists into an awareness of just how badly they behave towards anyone who dares to disagree with them." This presumptuous scenario was certainly an ambitious one—shock therapy at a distance to correct the patient's manners, as much, perhaps, to promote the doctor's peace of mind, as to benefit the patient's health. I am sorry that this "plan" for the improvement of my behavior "obviously did not work," but there are bound to be disappoint-

ments. Maybe, like B. F. Skinner's plan for man, Professor Domhoff's will work better with pigeons than with people.

As to the fourth paragraph of his letter, I was not likening Professor Domhoff to ultraconservatives. I was comparing his approach to that of the McCarran committee; and his insinuation that the power elite does not dictate to scholars but that members of the power elite see no reason for discontinuing their support *is* an insinuation worthy of the McCarran committee.

The fifth paragraph of Professor Domhoff's complaint suggests the need for remedial writing. In his book, he talks about the relationship of "corporate-controlled foundations, think factories, and university research institutes" (p. 126). So—he says that Gabriel Almond offers thanks to the Carnegie Corporation for the money that made *The Appeals of Communism* possible; Carnegie supplied the funds for Almond and Verba's *Civic Culture;* Thomas Schelling wrote *The Strategy of Conflict* while at the Rand Corporation; Herman Kahn did most of the research for *On Thermonuclear War* while at the Rand Corporation; Lucian Pye had the help of Carnegie money for *Aspects of Political Development;* W. W. Rostow wrote *Stages of Economic Growth* and had Carnegie money; and Harry Eckstein had Carnegie money to help with the editing of *Internal War*. None of this has anything to do with the private and "sanitized" versions of the same work that Professor Domhoff refers to in his letter, and the CIA is not mentioned at all. What Professor Domhoff did say after reciting the list of works indicated was that "Eckstein's own contribution, 'On the Etiology of Internal Wars'" had appeared in an earlier version in *Social Science and National Security*—"a book," he says, "which had government circulation only." (*Internal War*, incidentally, contains no such contribution by Eckstein, or by any other author.)

It is at this point that Professor Domhoff then says, "Apparently such secret books are not unusual" (p. 127), and he cites a secondary source for the proposition. That's about all there is on the "secret books." If he thought that he was talking about "secretly financed books that scholars write for the CIA, producing one version for the CIA and a 'sanitized' version for the general public," he never said so. If, however, he is now saying that all of the works mentioned were secretly financed books written for the CIA in two versions, one private and one public, he has supplied no evidence. Indeed, it seems to me that if this is his present position, there are many questions without answers. What kind of a secret is it if he knows about it? Is a Carnegie grant evidence of CIA influence? Whatever made Gabriel Almond spill the beans when he offered thanks to the Carnegie Corporation?

He then confronts me with David Easton or, rather, "none other than David Easton." I did not let my readers know, he suggests, that I was really "taking on David Easton." Such an undertaking should dismay all but those of the most abandoned and malignant heart, and I should long have hesitated before entering upon such a trial. Happily, it was not then necessary to do so, nor is it now, for two reasons. The first is that I have no disagreement with David Easton in the matters cited by Professor Domhoff; and the second is that Professor Domhoff's elision from Easton's text misrepresents Easton's position.

First, there is no disagreement because it is obvious that political science does concern itself with the most contended area of human experience, namely, the basic distribution of power in society. As Easton says, "psychology, sociology, or economics" are "less intimately connected with revealing the actual locus of power in the community," or the channels through which social policy is made, than is political science. I certainly agree that political science is a dangerous subject, and it is clear that the possibility of reprisals for probing may be inhibiting and that they may, in fact, have been inhibiting. Second, Easton says that however "inadequate its success" in transcending its limitations in this respect, "political science is reaching towards an understanding of the very things that men consider most vital"—their differences over what Easton describes as the authoritative allocation of values. And one who knows the literature must agree that both before Easton, and afterward, there have been many fine studies of who gets what, why, when, and how; what the political structure is; where power resides; and so on. But the remark of Easton just quoted is what Professor Domhoff left out. By leaving it out, he makes inane, and therefore false, the opinion he attributes to Easton, to wit, "that political science is a superficial discipline which supinely avoids research into anything its corporate masters don't want studied."

In his seventh paragraph, Professor Domhoff fusses about the "context" in which he made his remark about private and "sanitized" versions of the same study, and says that he was not talking about the CFR. He says he was talking about the CIA. But I have already shown that he does not mention the CIA at all in the "context" in which he now thinks he was talking about the CIA. In fact he was talking about Eckstein, but if we would really wish to know the context in which he actually mentioned the CIA, it has to be the larger universe of the CFR, CED, international research centers at various universities, Rand, Carnegie, Rockefeller, and so on. His "context"

then does include both the CFR and the CIA. He says, for example, that CIA leaders and foreign policy leaders "interact at CFR study groups" (p. 339). He says that the CIA and major foundations supported the Center for International Studies at MIT, which produced the Millikan-Rostow report with which other reports done by Brookings and the National Planning Association were in harmony which was to be expected because the NPA in 1958 had interlocking directors with the CFR and the CED (p. 146). So, the CIA, CFR, CED, NPA, and MIT are all a thick swirl in Professor Domhoff's higher circles.

The fact that Professor Domhoff uses the Easton quotation betrays his confusion about what context he is in. Easton was not talking about writers who publish private and public versions of the same work. He was talking generally about the relation—to use Professor Domhoff's revision—between a "superficial discipline" and its "corporate masters," which is quite a different thing.

Finally, like a previous critic on the exciting subject of my opinions, Professor Domhoff goes in for mind-reading. I am "too preoccupied" with saving our pluralistic democracy, he says, to "worry about" the fact that liberal colleagues are undermining academia; and I "can only think" of his "cynicism" about the "alleged high-mindedness and decency" of his "fellow social scientists." What I am preoccupied with, and worry about, and can only think, is that an author identified by his champion as "a psychologist by training who specialized in personality theory" is simply out of his element in the higher circles of sociology and politics.

EARL LATHAM

Amherst College

EDITORIAL COMMENT

Tempest. We have, for our sins, received a brotherly communication from Professor Arnold Rogow, the editor-in-chief of *Comparative Politics*, hotly disputing certain of the figures in a recent editorial comment (December 1973) concerning the deplorable lack in the *APSR* of articles on China. These figures seem to show—and were so interpreted—that articles on China appear very seldom in any of the main political science journals. Our brother Rogow does not dispute these figures, or indeed anything else bearing on the point of the editorial comment. He does complain, however, that articles purportedly on Latin America appearing in *Comparative Politics* were undercounted, and that the proper number is 7, not 4. For South and Southeast Asia their figures are 11 and ours 4. And for Canada their figures are 3 and ours 0. (For "underdeveloped areas" they had 8 and we counted 10.) Professor Rogow further points out, "The *New York Times* regularly has a column of 'corrections' even if the statement corrected is rather insignificant. I recall a case when a *Times* story referred to someone as the 'brother' of a deceased person. In a subsequent edition the statement was corrected to read 'brother-in-law.' Is the *APSR* less responsible than the *New York Times*?"

Heaven forbid. We are happy, therefore, to print his correction, and we trust our brother-in-law Rogow will rest content.

Unwritten Rules. It has not yet ceased to amaze us to see the extent to which the *Review* is read cabalistically, rather than straightforwardly, by political scientists. People frequently try to figure out how the *Review* does its business by inference rather than by inquiry. For instance, given our backlog, it is easy enough to discern why we would rather print short articles than long ones. Nevertheless, if a long article makes the grade, and more than a few have over the last couple of years, we go ahead and print it. The fact that we do this should be plain enough to any readers of the *Review*.

Yet for some reason people occasionally tell us that they assume the *Review* has some sort of page limit on manuscripts regardless of quality. In recent weeks we have had to assure two different authors of moderately lengthy manuscripts that this was not so, and that we would not reject a manuscript out of hand for reasons of length alone.

Likewise, we do not discourage the submission of different manuscripts either by authors who have recently been published by the *Review* or by those recently rejected. Fortunately, somebody asked us about that one. We take the same view as seems to have been taken by the editor of *Annalen der Physik*, who in the year 1905 was pestered by manuscripts from a twenty-six year old Swiss civil servant named Albert Einstein no less than four times. The published articles that resulted[1] did not particularly advance the career of the editor who accepted them, but three of them turned out to be significant landmarks in the study of modern physics.

Nobody knew ahead of time where and in what ways 20th century physics would develop. Owing to the institutional structure of the time, a forecaster could be confident only that if it was going to develop at all, it would have to do so through the medium of scholarly exchange, and predominantly through the journals.

Although the idea of an embryonic Einstein lurking in our midst is nice to contemplate, it is unnecessary for there actually to be one for us to pursue the *Annalen der Physik* policy of encouraging scholars who have two things to say to say them both. Consequently, every new manuscript, from whatever source, is treated separately, and except in the rare case where a preliminary screening discloses its inappropriateness for the *Review*, it is assigned referees, and proceeds through our editorial process, just like every other new manuscript. Nobody gets a free ride—and as a matter of fact nobody has asked for one. Nobody is turned away without consideration on the merits. That is what we are here for.

On Correcting the Record. Speaking as an author, the three worst things an editor of a scholarly journal ever did to me were: (1) Solicit a book review, which was duly supplied, and then reject the review on the stated grounds that it evaluated the book differently from published reviews the editor had seen. Instead, this editor asked permission to condense the review into a few sentences and run it as an anonymous book note. Permission was refused, whereupon the editor did it anyway. (2) Accept a review-essay for pub-

[1] As readers will observe from their titles, these articles were not even written in acceptable English: "Über einen die Erzengung und Verwandlung des Lichtes betreffenden heuristichen Gesichtspunkt," *Annalen der Physik*, Series 4, Vol. 17 (1905), pp. 132–148; "Über die von der molekularkinetischen Theorie der Wärme geforderte Bewegung von in ruhenden Flüssigkeiten suspendierten Teilchen," *ibid.*, pp. 549–560; "Zur Elektrodynamik bewegter Körper," *ibid.*, pp. 891–921; "Ist die Trägheit eines Körpers von Seinem Energieinhalt abhängig?," *ibid.*, Vol. 18 (1905), pp. 639–641.

lication and then edit it into partial unintelligibility without informing me, and printing the resulting mess under my name. (3) Summarily refuse to print a letter offering a correction of an article that had flagrantly misquoted my work. This happened to be about a subject on which the editor himself had in his writings adopted the position of the article I was endeavoring to correct.

These three editors have a special place in my own personal hall of fame. No one who, after experiences of this sort, himself becomes an editor, can help wondering how many halls of fame he inhabits. This may assist in explaining why we conscientiously strive to minimize at least these three classes of mistakes at the *APSR*. We do not knowingly print material contrary to the expressed wishes of authors. We give authors ample opportunity to monitor all the editorial work we perform. And we bend over backward to accommodate most persons who claim to have been misrepresented by material printed in the *APSR*. There is a risk involved in this last policy, and we do try to discriminate between what appear to us to be reasonable complaints and people merely seizing an occasion for self-advertisement, a problem that crops up with some frequency in the book reviews. Any unfavorable review is likely to be perceived by the author of the book in question as unfair. Yet not all authors of panned books write in. Of those that do, how many have a legitimate claim on our scarce space and our readers' valuable time? Opinions are bound to differ on this point. Some readers have shared with us the view that most of the complainants in our letter columns are merely serving themselves and not the cause of scholarship. Yet if we were to turn a deaf ear to all such complaints, some serious misrepresentations, we are convinced, would go uncorrected. Every reader will have to decide for himself how successfully we walk this particular thin line.

Errata. In Brian D. Silver's "Social Mobilization and the Russification of Soviet Nationalities," a line of type has been transposed: the third line on page 65 (March 1974) is missing, and turns up intact as line 23. The original sentence should read: "But two long-term trends working together favor Russification: not only are natives becoming more urbanized, and thereby more available for Russification, but the probability that the urban resident will be Russified also appears to be growing."

In Gregory Henderson's review of *Korean Development: The Interplay of Politics and Economics* (March 1974), a line of type on page 298 has been transposed; what should be the 9th line is set as line 2. The relevant section should read: "Although the Park government has, in effect, ceased even such attempts as it once made to be genuinely popular and although repression renders the degree of support or opposition alike immeasurable, this economic concentration and its fruits surely continue to give the government whatever degree of genuine stability it has; and, for the moment, this degree appears still to suffice. The story of how this took place, told with some clarity by two men who observed its details, is of great value.

In Ted Robert Gurr's review essay, "The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science" (March 1974), the sentence at the end of page 243 and at the beginning of page 244 should read "The compilers respond by resorting to cross-checking against multiple sources, double coding, and sometimes defensive codicils. A third common feature of the handbooks is their 'institutionalized' origin."

In Table 3 (page 1217) of "Party and Incumbency in Postwar Senate Elections" by Warren Kostroski in the December 1973 issue, the column totals for the Senate should read 195, 171, and 87.7. The textual reference just above the table should also be changed accordingly.

Articles Accepted for Future Publication

C. Arnold Anderson, University of Chicago, "Conceptual Framework for Political Socialization in Developing Societies"

Neal Andrews, Wayne State University, "Integration and Community in Communist Theory"

Robert L. Ayres, University of California, Berkeley, "Development Policy and the Possibility of A 'Liveable' Future for Latin America"

Richard Allen Chapman, University of Montana, "*Leviathan* Writ Small: Thomas Hobbes on the Family"

John P. Clark, III, City College, Loyola University, "On Anarchism in an Unreal World: Kramnick's View of Godwin and the Anarchists"

Claude S. Colantoni, Terrence J. Levesque and Peter C. Ordeshook, Carnegie-Mellon University, "Campaign Resource Allocations Under the Electoral College"

Andrew T. Cowart, University of Iowa, Tore Hansen and Karl-Erik Brofoss, University of Oslo, "Budgetary Strategies and Success at Multiple Decision Levels in the Norwegian Urban Setting"

Geoffrey Debnam, University of Otago, "Nondecisions and Power: The Two Faces of Bachrach and Baratz"

Douglas Dobson, Northern Illinois University and Douglas St. Angelo, Florida State Uni-

versity, "Party Identification and the Floating Vote: Some Dynamics"

Dennis L. Dresang, University of Wisconsin, Madison, "Ethnic Politics, Representative Bureaucracy, and Development Administration: The Zambian Case"

Claude S. Fischer, University of California, Berkeley, "The City and Political Psychology"

Richard Funston, San Diego State University, "The Supreme Court and Critical Elections"

G. David Garson, Tufts University, "On the Origins of Interest Group Theory: A Critique of a Process"

Mark Gavre, University of California, Los Angeles, "Hobbes and His Audience: The Dynamics of Theorizing"

Sheldon Goldman, University of Massachusetts, Amherst, "Voting Behavior on the U. S. Courts of Appeals Revisited"

Fred I. Greenstein, Princeton University, "The Benevolent Leader Revisited: Children's Images of Political Leaders in Three Democracies"

Fred W. Grupp, Jr., University of Connecticut and Allan R. Richards, Louisiana State University, "Variations in Elite Perceptions of American States as Referents for Public Policy Making"

Ted Robert Gurr, Northwestern University, "Persistence and Change in Political Systems, 1800–1971"

Richard Child Hill, Michigan State University, "Separate and Unequal: Governmental Inequality in the Metropolis"

K. J. Holsti, University of British Columbia, "Underdevelopment and the 'Gap' Theory of International Conflict"

Robert T. Holt and John E. Turner, University of Minnesota, "Crises and Sequences in Collective Theory Development"

M. Kent Jennings, University of Michigan and Richard G. Niemi, University of Rochester, "Continuity and Change in Political Orientations: A Longitudinal Study of Two Generations"

Jae-On Kim, University of Iowa, John R. Petrocik, University of Chicago and Stephen N. Enokson, University of Iowa, "Voter Turnout Among the American States: Systemic and Individualistic Components"

David Koehler, American University, "Vote Trading and the Voting Paradox: A Proof of Logical Equivalence"

Walter Korpi, University of Stockholm, "Conflict, Power and Relative Deprivation"

J. A. Laponce, University of British Columbia, "Prolegomenon to the Study of Spatial Archetypes and Political Perceptions"

Peter M. Leslie, Queen's University, "Interest Groups and Political Integration: The 1972 EEC Decisions in Norway and Denmark"

Alan Marsh, Social Science Research Council, "The 'Silent Revolution,' Value Priorities, and the Quality of Life in Britain"

Joseph A. Massey, Dartmouth College, "The Missing Leader: Japanese Youths' View of Political Authority"

Kenneth John Meier, Syracuse University, "Representative Bureaucracy: An Empirical Analysis"

Patrick J. McGowan, Syracuse University and Robert M. Rood, University of South Carolina, "Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to 19th Century Europe"

Stanton Peele, Harvard University and Stanley J. Morse, Pontifica Universidade Catolica de Sao Paulo, "Ethnic Voting and Political Change in South Africa"

N. Patrick Peritore, University of Missouri, Columbia, "Some Problems in Alfred Schutz's Phenomenological Methodology"

David E. Price, Duke University, "Community Control: Critical Democratic Theory in the Progressive Period"

Adam Przeworski, University of Chicago, "Institutionalization of Voting Patterns or Is Mobilization the Source of Decay?"

Douglas Rae, Yale University, "The Limits of Consensual Decision"

Joseph A. Schlesinger, Michigan State University, "The Primary Goals of Political Parties: A Clarification of Positive Theory"

Brian D. Silver, Florida State University, "Levels of Sociocultural Development Among Soviet Nationalities: A Partial Test of the Equalization Hypothesis"

J. S. Sorzaon, Georgetown University, "David Easton and the Invisible Hand"

Peter G. Stillman, Vassar College, "The Limits of Behaviorism: A Critique of B. F. Skinner's Social and Political Thought"

C. Neal Tate, North Texas State University, "Individual and Contextual Variables in British Voting Behavior: An Exploratory Note"

Kent L. Tedin, College of William and Mary, "The Influence of Parents on the Political Attitudes of New Voters"

Edward R. Tufte, Princeton University, "Determinants of the Outcome of Midterm Congressional Elections"

Eric M. Uslaner and J. Ronnie Davis, University of Florida, "The Paradox of Vote Trading: Effects of Decision Rules and Voting Strategies on Externalities"

Clement E. Vose, Wesleyan University, "Political

Dictionaries: A Bibliographical Essay"

Meredith W. Watts, University of Wisconsin, Milwaukee, "B. F. Skinner and the Language of Technological Control"

J. Weinberger, Michigan State University, "Hobbes's Doctrine of Method"

Herbert Weisberg, University of Michigan, "Models of Statistical Relationship"

Mary B. Welfling, Virginia Polytechnic Institute and State University, "Models, Measurement and Sources of Error: Civil Conflict in Black Africa"

Louis P. Westefield, Southern Illinois University, Edwardsville, "Majority Party Leadership and the Committee System in the House of Representatives"

Chemical and Biological Warfare*

GEORGE H. QUESTER
Cornell University

Why should the world be more averse to chemical and biological warfare (CBW) than to other forms of war, repeatedly attempting to ban such warfare by means of international law? Is there some objective difference between poison gas or plague bacilli on one hand, and steel bullets, or B-52 high explosive bombs, or nuclear weapons on the other? Or does CBW instead illustrate the faddishness and superficiality of disarmament campaigns in general?

The Allied Powers after 1919 may indeed have condemned chemical warfare simply to reinforce the impression that Germany was morally culpable in World War I. The Germans supposedly had initiated the use of gas warfare (although the "first use" here might really have to be pinned on France instead); if Germans had used such weapons first, such use must have been evil. Perhaps instead the aversion to gas reflected the horrors of such warfare for the soldier exposed to it, although casualty statistics suggest that fewer soldiers died or were permanently maimed after being incapacitated by gas, as compared with those stricken by other means.

Perhaps instead the techniques of chemistry and biology have seemed unclean and unmanly, the products of "odd-ball" scientists ensconced in laboratories, reflecting perverted scientific ingenuity rather than the bravery of cold steel. The sheer unpredictability of poisons and of epidemics could thus make all war a little less comparable with the past, and a little less manageable. Some or all of these explanations probably account for the remarkable aversion of much of the professional military to CBW, an aversion noted in several of the works under review.

Most recently, aversions to CBW have been powerfully reinforced in the United States, and in some other advanced industrialized societies, by the more general concern for ecology. We are becoming aware that it is difficult to predict the side effects of any major chemical or biological innovation, side effects which sometimes far outweigh—often negatively—the result originally sought. War of any kind is presumably bad for children and other living things. War fought with esoteric diseases or with herbicides may be particularly bad for the ecology, i.e., for living things far into the future.

Yet the very mention of ecology will suggest a certain faddishness to the political scientist, a certain "here today, gone tomorrow" about such causes which makes us reluctant to bet on them as bases for serious political movements. Much of this faddishness has all along been present in debates on the chemical warfare issue. In 1922, the U.S. Senate ratified a treaty which would have renounced all American options for chemical warfare; the treaty never became binding, but only because the French Parliament failed to ratify it for an entirely separate reason. By 1926, however, the U.S. Senate refused to ratify the Geneva Protocol, precisely because American public opinion had changed on the chemical warfare question. As Volume I of the SIPRI study (*The Rise of CB Weapons*) points out, what had seemed a loathsome German weapon in 1922 had been redefined by 1926 as a potentially humane technology which would allow enemies to be disarmed without first killing or maiming them. Part of this change of feeling, but only part, can be attributed to assiduous public relations work by the U.S. Army's Chemical Warfare Service. Perhaps the rest can be explained by the fact that the American public did not yet have to fear chemical attacks on its cities; in the view of the SIPRI volume, this would similarly account for Japanese resistance to bans on gas warfare.

Yet even this explanation may be too deterministic, since each country in the later 1920s had begun to show some apprehension about air raids in future wars—raids from hostile aircraft

* *Chemical Warfare: A Study in Restraints*, Frederic J. Brown (Princeton: Princeton University Press, 1968. Pp. 316. $9.00.)

The Control of Chemical and Biological Weapons (New York: Carnegie Endowment for International Peace, 1971. Pp. 130. $1.00.)

A Survey of Chemical and Biological Warfare, John Cookson and Judith Nottingham (New York: Monthly Review Press, 1969. Pp. 420. $3.95.)

Harvest of Death, J. B. Neilands, G. H. Orians, E. W. Pfeiffer, Alje Vennema, and Arthur H. Westing (New York: The Free Press, 1972. Pp. 304. $10.00.)

CBW, Steven Rose, ed. (Boston: Beacon Press, 1969. Pp. 209. $1.95.)

The Problem of Chemical and Biological Warfare, 6 volumes (Stockholm International Peace Research Institute, 1971, 1973)

I. *The Rise of CB Weapons* (1971, Pp. 395, Sw. Kr. 75)
II. *CB Weapons Today* (1973, Pp. 420, Sw. Kr. 75)
III. *CBW and the Law of War* (1973, Pp. 180, Sw. Kr. 40)
IV. *CB Disarmament Negotiations, 1920–1970* (1971, Pp. 412, Sw. Kr. 75)
V. *The Prevention of CBW* (1971, Pp. 287, Sw. Kr. 40)
VI. *Technical Aspects of Early Warning and Verification* (1973, Pp. 350, Sw. Kr. 75)

carriers or by intercontinental airplanes following the trails blazed by Lindbergh and others. In the speculative analyses of the time, war was expected to be brought home to the civilian population by air raids on cities, with the killer weapon often cast as poison gas. But the literature of the same period shows a persistent strain of analysis, in Britain and on the continent as well as in the United States, favoring the development of nonlethal incapacitating chemical and biological weapons. Volume V (*The Prevention of CB*) of the SIPRI books chronicles some of this literature; H. G. Wells's *Things to Come*, and the movie produced from it, are among the best examples, in which a sleep-inducing gas becomes the wonder-weapon for the great civilizing force which establishes a hegemony to restore peace to the world.

The world today is obviously moving back into an anti-CBW posture, and it is again necessary therefore to separate the serious motivation from the less serious. Ecology has been already mentioned. The sheer magnitude of what is possible in CBW has been enormously broadened since the 1930s, in part by precautionary research on both sides in World War II, although CBW was generally not used. Such research has clearly been continued in the United States and the Soviet Union since 1945. While some interesting nonlethal weapons have been developed, enormously lethal ones have also been developed in the same effort, making the very range of possibilities frightening.

Poison gas may have been used by the Egyptians in Yemen since 1963, but this probably does not account for so much of current anti-CBW sentiment. Irritant gases and chemical defoliants were extensively used by the United States in South Vietnam, and here we have a more important cause of the latest shift in public attitudes, indeed for the publication of a number of the books under review. Yet again some critical questions might be raised about the straightforwardness of these indictments. Is the American use of CS gas (a "tear gas") in Vietnam under attack because it was "gas," or because it was America in Vietnam? If the historical impression of the immorality of "gas warfare" has been merely another stick with which to beat U. S. Southeast Asian policy over the head, has it been such a sturdy stick? Some of the authors under review here are mainly opposed to chemical warfare per se (although one senses that not a single one of them supported non-CBW American military operations in Vietnam). Yet others were primarily opposed to American presence in Vietnam, and might not even have been so "anti-CBW" in other contexts.

Some New Literature

The six SIPRI volumes, collectively entitled *The Problem of Chemical and Biological Warfare*, are a very scholarly achievement. By drawing together the European and Asian chemical warfare experience of the entire twentieth century, the authors generally manage to avoid the parochialism of other works which focus narrowly on the experience and problems of the 1960s. The SIPRI series is a joint product of a number of competent authors, which causes some problems. There is a substantial amount of repetition, occasionally even within particular volumes. Important segments of the series amount to basically noncontroversial, and close-to/encyclopedic technical handbooks, most particularly Volume II on *CB Weapons Today*, which deals with biochemistry more than with politics. Elsewhere, judgments had to be reached about historical events which will never be certain, for example on the exact extent and nature of Japanese chemical warfare in China in World War II, or alleged U.S. use of biological warfare in Korea, or Egyptian use of poison gas in Yemen. In some sections of the work, one senses an excessively self-conscious policy of nonalignment, whereby, for example, the International Committee of the Red Cross, "ICRC," is made to sound no more reliable on investigations of CBW allegations than the so-called International Scientific Commission, whose charges of American germ warfare in Korea are repeatedly dignified in Volume V by the initials "ISC."

The SIPRI series not only collects a great deal of valuable data on the history of CBW, and of efforts to head it off; it also presents fascinating analyses on why CBW was indeed avoided at various crucial stages of World War II, and on what its military impact was at various stages of World War I. One notes that CBW might have changed World War II in making Panzer offensives less possible. Some fair service is given to the arguments that CBW can be more humane, although the SIPRI volumes seem reluctant to let this line of reasoning flow very far before it is rebutted and terminated. All in all, these volumes are a product of which the Stockholm Institute can be proud, coming up to the fine standard of the *Yearbooks on World Armaments and Disarmament*.

Frederic J. Brown's *Chemical Warfare: A Study in Restraints* is an equally scholarly work, based on a detailed examination of the available government memoranda on decisions about gas warfare. It shows how CBW decisions, especially in the United States, have been based on much of the same bureaucratic infighting and "organizational process" drift that characterizes other public policy. Brown argues that chemical warfare has above all been an excessively demanding form of combat, so technologically and organiza-

tionally complicated that the U.S. Army and other armies have always had difficulty in getting prepared to wage it; perhaps lack of preparedness thus has outweighed even the fear of retaliation in explaining why we have seen relatively little CBW since 1918. Brown's work is especially good at recapturing the atmosphere within which chemical warfare campaigns were launched during World War I, and then were narrowly averted in 1940 and 1945. He vividly portrays the biases of professional military officers against this weapon which "civilian chemists" were handing over to them.

The Carnegie Endowment book on *The Control of Chemical and Biological Weapons* is less comprehensive than its title suggests. Rather, it is a collection of four monographs on relatively contemporary issues on CBW. The first, "Legal Aspects of the Geneva Protocol of 1925," amounts to a persuasive legal brief against the Nixon position that the Protocol should allow the use of nonlethal gases. As a brief, it tends to overstate its plea at points, for example in analogizing the use of tear gas in POW riots to the gassing of inmates in German concentration camps, and it brushes past any of the stronger arguments for distinction between "good" and "bad" CBW. "The Use of Herbicides in War" makes a strong case that the U.S. may have been hurting its allies and defeating its own purposes by its defoliation policy in Vietnam. "The Military Value and Political Implications of the Use of Riot Control Agents in Warfare" similarly exposes substantial uncertainties and ambivalences about whether directly nonlethal gases in Vietnam have been used to decrease, or to increase, deaths among the Communist forces. "Limitations on Chemical and Biological Warfare Going Beyond Those of the Geneva Protocol" is the least position-oriented paper in the book, presenting a relatively clear and dispassionate analysis of further opportunities and obstacles for the control or prevention of CBW.

Harvest of Death, by J. B. Neilands et al., is an outgrowth of a study begun under the auspices of the American Association for the Advancement of Science, by an ad hoc group which evidently saw the issues as requiring more pressing advocacy than did the Association itself. The study could be better written, but it makes its points clearly enough, that the use of CS irritant gases and chemical defoliants in Vietnam have been evil in all respects, even as the authors feel that the Vietnam war itself is evil. While the book assembles a great deal of technical data on the direct effects and side effects of these forms of chemical warfare, the data might more often have been allowed to speak for themselves.

CBW, edited by Steven Rose, is a collection of very thorough and well-written papers prepared for a 1968 conference on the topic. As such it makes an excellent introductory volume on the subject, and something more. There are chapters on each of the major forms of CBW now possible, and summations of the events in Vietnam and Yemen. Some profound insights are offered on research policy in the U.S., U.K. and U.S.S.R., and on the inevitable loss of responsibility in the secrecy surrounding such work. Indeed, advocates of greater tolerance for CBW will find it difficult to assure us that any subtle distinctions they proposed would actually be adhered to, when scientists or military officers can always hide their actual practice behind the necessary cloaks of military secrecy. A chapter on "Preventing CBW," by J. H. Humphrey and Matthew Meselson (Meselson also is coauthor of a section of the Carnegie Endowment volume) presents a very strong and commonsensical argument that any relaxation of existing aversions to CBW may lead to an open-ended and uncontrollable escalation. While virtually every author in the Rose volume betrays a commitment to preventing all of CBW, the style of argument is the more convincing because it presumes no instant consensus with the reader on the other moral issues of international relations.

A Survey of Chemical and Biological Warfare, by John Cookson and Judith Nottingham, indeed lives up to its title, cataloging and chronicling virtually every known sort and instance of chemical or biological warfare. The authors, here as elsewhere, are plagued by having much more information about CBW research and procurement in the West than in the Communist world; by "telling all they know," they implicitly give the impression that the West is the primary offender. A perhaps unfortunate reliance on an East German indictment for facts about West German activity mars what is otherwise the calm and scholarly tone of the book. Its examination of the "International Scientific Commission" ("ISC") findings of U.S. biological warfare in Korea virtually erases any remaining plausibility for these charges.

CBW: Good or Bad?

Is there an argument for greater tolerance of CBW, an argument that should be given a greater hearing? Or, after a hearing, might we instead conclude that the world is far better off keeping most or all of chemical and biological warfare illegal and beyond the pale? From the viewpoint of arms control, should we even endorse a conspiracy to deny CBW a fair hearing, if these weapons options are better left undiscussed?

Of the books under review, only an appendix in SIPRI Volume V and some sections of Volume I really make much effort to consider the argu-

ments for legitimating chemical and biological weapons. Basically two strands of argument can be offered: (1) Chemical or biological weapons might make war less horrible; and (2) CBW might make war less likely.

As to making war less horrible, such weapons may enable an army to defeat and incapacitate its enemies without killing as many of them, or as many innocent bystanders. While one can put down this argument simply by suggesting that commanders are congenitally indifferent to the homicide of enemies, this rejoinder does not reflect reality. In the U.S. Civil War, was Lincoln really indifferent to how many Confederate soldiers were killed in the process of restoring the Union? Generals and Presidents typically become indifferent to killing only when there is no other way to win battles, or when this is necessary to keep their own soldiers from being killed. The argument of course becomes even more convincing when the local civilians are assumed to be loyal to "our side," and would suffer in any conventional artillery barrage directed at a hostile army in their midst.

As noted, data from World War I suggest that (as compared with other weapons) far more soldiers on both sides could be eliminated from the battlefield by gas attacks without being killed or permanently disabled. Long-term susceptibility to emphysema may suggest the horror of mustard gas attacks, and (as Brown notes) Adolf Hitler's aversion to chemical warfare is indeed said to have stemmed from his personal experience of being gassed in 1918. Yet one must compare this kind of wound with the waves of men who never returned from the trench war at all, after being subjected to machine gun or artillery fire.

Although far deadlier gases have of course been developed since 1918, less deadly CBW weapons have also been devised, weapons which potentially might keep an enemy soldier from killing without doing the slightest permanent damage to him in the process. If used properly, some CBW weapons might thus make war more humane. Whether they would be used properly is of course a more complicated question, a question which the Carnegie Endowment book addresses rather cavalierly, and SIPRI Volume V addresses rather well. It is a question to which this review will return.

But if CBW could make war less unpleasant, would it therefore ipso facto make it more likely? There is at least some risk that the prospect of "humane war" with "tomorrow's weapons" might make all sides more ready to take to the battlefield in resolving political disputes. Yet the impact of CBW on the likelihood of war may depend even more on a different aspect of these weapons: whether they assist the offense or the defense. Does CBW, like the tank, encourage armies to plunge into enemy territory, or does it like the prepared fortification, encourage each side to sit back and let the other attack first? At the margin, weapons which favor the offense increase the likelihood of war, since whoever strikes first in a politically doubtful situation may win. Weapons which favor the military defense conversely increase the chances of peace even during the gravest political crisis, since either military commander will advise his president that there is no point in striking first.

Which way do CBW weapons work? Since gas can filter down into the airshafts of prepared fortifications, or of natural caves, it may seem to aid the offensive; the very premium that is placed on technological innovation (whereby 1916 gases may make 1915 gas masks outmoded, etc.) may similarly suggest striking while the advantage of an innovation still persists. Gas might thus be undesirable even though it made war more humane, if in the process it had made war more likely.

Yet as the SIPRI Volume I points out, the equipment needed for chemical warfare has historically been heavy and burdensome for the individual soldier and for the army as a whole. Could Hitler's Panzer forces have slashed as quickly through Poland and France if each side had been committed to poison gas war? The answer might well be no. If Hitler had been on the brink of a successful amphibious invasion of England, wouldn't gas clouds on the British beaches have been a most effective way of stimying such an offensive? Would Britain indeed not have escalated to CBW at that point? Would we not all have applauded?

If CBW consistently buffered the defenses of armies confronting each other, peace would tend to be more likely when such weapons were kept legal and in stock. One must of course be very careful here in defining terms such as "offensive" and "defensive" in discussing CBW. As used in the SIPRI series and in the Rose book, all forms of CBW weapons are "offensive," while "defensive" refers exclusively to gas masks, inoculations, etc., which neutralize the impact of chemical and biological agents on the human body. While this distinction will seem relatively clear, it does not help us with extrapolations about the likelihood of war.

Pandora's Box?

CBW might thus discourage war in some cases, as well as reducing the costs of war. Yet one has to watch the strategic interactions very closely here to be sure that anything like this will really happen. Gas weapons can be humane, but they can also be very deadly, more so than any weapon

developed before the atomic bomb. If chemical warfare had been legitimate in 1939, Hitler's forces might have been slowed and immobilized by all the protective gear they had to carry with them, but air raids on German and Allied cities might have been carried through by nerve gas, with substantial increases in casualties inflicted on the home front. What in fact kept Hitler from using nerve gas "defensively" to blunt the Allied landings at Normandy? As the SIPRI Volume I describes, it was primarily fear of chemical warfare retaliation on the cities of the Reich.

The gas issue is the clearest reminder of how much World War II was a "limited war," with each side still deterred from climbing this final rung of the "escalation ladder," by fear that the other side would reciprocate. Even when all other restraints on the bombing of cities had been lifted, poison gas was not to be dropped into the streets of Hamburg and Dresden and London and Tokyo. If one today were to see aversions to chemical and biological warfare as simplistic, he still would have to confront the fact that Hitler and Roosevelt and Churchill successfully conspired to spare the world such warfare. Were these leaders simply taken in by the moralisms of 1922? Did they not realize that chemical or biological warfare in principle offered the prospect of less painful (if not painless) war, as suggested by H. G. Wells in 1933?

Perhaps the psychedelic gases were not yet ready (or are still not), to transport soldiers blissfully from the battlefield into the nearest prisoner-of-war compound. Yet more important, there hung over the war a sense that "no gas" was a salient distinction, a distinction enshrined in international law and national practice (despite Italian and Japanese violations), a distinction which should not be thrown away, since the next "firebreak" might be difficult to find. Even if a World War II army might have decided to use gas in a particular battle to reduce casualties on both sides (e.g., in forcing Japanese soldiers out of caves on Saipan), everyone involved sensed that it might not stop at this. Will men *be able* to stop with only the more humane uses of chemical or biological warfare? Perhaps, but only if a clear and intricate groundwork of legal definition and strategic dialogue were prepared well in advance.

Will men indeed *wish* to stop with only the more humane uses, once the option has been opened? Perhaps, but only if they are not tempted to win battles and spare themselves casualties by less humane uses. For example, CS gas has been used in Vietnam by American forces to drive Viet Cong out of the caves in which they have been holding innocent villagers hostage. This tactic presumably spared the lives of the villagers and perhaps even of the Viet Cong, wherever the Americans could have thrown grenades or fired rockets into the caves anyhow.

But CS has also been used to drive Viet Cong out of caves when they would have been secure against American grenade attack, driving them into the open where they can be killed by artillery or aerial bombardment. In the first case, CS added a humane alternative where a deadly conventional means of attack already existed. In the second case, CS instead added deadly means of attack which otherwise would not have existed at all.

There is not just a danger of further escalation once the barrier to chemical warfare is lifted, therefore, but a real temptation to escalate. If a new barrier could be erected to separate the desirable uses of CBW from the undesirable, the world might benefit. But if the barrier were not erected, or if it were erected too weakly, a process of escalation might begin for which no end was in sight.

This possibility at least in part accounts for the willingness of a number of authors to dismiss any and all limited uses of CBW, and to condemn the United States government's efforts to keep legitimate the options of tear gas and chemical defoliants in war. At one point the SIPRI volume comments that tear gas has never been used without leading to the use of poison gas (the U.S. in Vietnam by now has disproved this rule).

As noted, the Baxter and Buergenthal piece on "Legal Aspects of the Geneva Protocol of 1925" in the Carnegie Endowment study goes so far as to compare the use of tear gas in suppressing a POW camp riot to the use of poison gas in Auschwitz. If one allows that tear gas is a well-established riot-control weapon in domestic society (all but the authors of the Neilands book seem willing to accept this) the case of POW's is indeed hard to judge. Are they being tear-gassed as enemy soldiers, or as persons who have submitted themselves to treatment comparable to that of civilians? Would Professors Baxter and Buergenthal really have them shot instead?

The distinction between domestic police action and international war is everywhere fuzzy. Guerrilla wars such as the one in Vietnam confuse issues all the more. All such wars tend to start as domestic police actions, wherein all forms of CBW would presumably still be legal, and some are normal. At what point did the Vietnam conflict become a war? If the Viet Cong after a certain date come to be treated as prisoners of war rather than treasonous rebels, do they win an exemption from poison gas or tear gas as of that date? And what of the IRA in Northern Ireland? There is a seeming paradox in a rule that soldiers in war are rigorously to be spared exposure to CBW, while one's own civilians may be exposed; but this is the law of the Geneva Protocol, and in

effect is the customary practice around which an important arms control firebreak has been based. Perhaps it makes sense if we see CBW (as exemplified by tear gas) as "humane." Yet if it makes sense, it still raises difficult questions about when the Viet Cong or the IRA change status from domestic insurgents to international army.

There is of course a different kind of apprehension possible about humane chemical weaponry—i.e., not that it would lead to perennial escalation into the lethal range, but rather that it would make war, and military suppression in general, all too easy and too acceptable. Imagine how ruthless a dictator might be if he could continually immobilize his opposition without doing them any bodily harm. Imagine how much of politics would become a matter of "might makes right," as long as the might remained "humane" might. How indeed can American society continue to condemn LSD and the drug culture, if it allows the development and testing of weapons based on LSD and drugs? Is it forseeable that a new oppressed class will emerge which is continually being drugged into submission by the gas attacks of nonlethal CBW? The tactics of passive resistance and civil disobedience are premised on burdening the police and the state with a choice between unacceptable violence or capitulation to protester demands. As the police of Paris or Alabama or Prague develop a third choice, will protest of this sort be all the more ineffective?

This brings us back once again to the interface between "domestic law enforcement" and "international war." As SIPRI Volume V points out, extensive use of "tear gas" at home tends to legitimate its use on the battlefield. In developing esoteric nonlethal battlefield weapons, we conversely run the risk that these weapons will be brought home for domestic use against our own population. The evaluation of such a trend thus depends heavily on what side one is on in the persistent substantive conflicts at home and abroad.

Some Current Issues

For the moment, the most interesting CBW issue indeed pertains to whether the United States can maintain the legitimacy of tear gas as used against an enemy in wartime. The U.S. is not yet legally bound at all to desist from CBW, although several of the works reviewed cite the argument that the Geneva Protocol has become customary international law and thus binding even on states which failed to ratify it. In response to the recent wave of pressure for an explicit American ratification of the Protocol, President Nixon resubmitted it to the Senate, accompanied however with an explanatory statement that tear gas is not covered by the Protocol. While this may be weak as law (as shown by the Baxter and Buergenthal piece in the Carnegie Endowment book or more exhaustively in the SIPRI Volume III on *CBW and the Law of War*) the United States is strong as a nation, and might by its example make this new legal distinction stick. Opponents of CBW within the U.S. Senate have thus been divided on whether to welcome the U.S. renunciation of lethal CBW activities, or to resist this effort to slice the Protocol by such an exemption. Support for Nixon's policy was clearly increased by his accompanying decisions to declare that the U.S. will unilaterally dispose of all its biological warfare agents, never use lethal chemical weapons first in combat, and never use biological agents under any circumstances.

If CBW weapons were our only weapon for killing millions of civilians, the problems of political and moral analysis would be complicated enough. Yet the complication is compounded by the existence of H-bombs which can inflict the same mass homicides even more effectively. Are H-bombs bad because they make war so horrible; or are they a blessing in disguise because they deter war and make it unthinkable? Could CBW weapons thus provide the "poor man's deterrent," giving a capacity for deadly retaliation to states which might not have mastered nuclear physics? ("Poor men" in international relations may of course be less deterrable themselves, since they have less to lose.)

CBW weapons have not served this purpose as yet in history, because they always would have been difficult to deliver to their target in the last hours of a losing war. If Hitler had possessed the H-bomb in 1945, could he not have insisted that Allied forces stay out of Germany, for fear of his "last-gasp" retaliation when such forces approached Berlin? But the threat of nerve-gas attack on London and Paris was never to be as potent, since no single rocket or airplane could carry enough of the materials to deter an Allied pursuit of total victory.

Yet one consequence of continued research and development in CBW is surely to generate compactly deliverable substances (especially the biological) which will be deadly enough to serve as nuclear weapons equivalents. Research in "painless war" will thus produce additional weapons for infinitely painful war; the risks of proliferation of nuclear weapons will be paired with the risks of proliferation in deadly chemical or biological weapons.

Could such CBW proliferation in any way be desirable? Perhaps the threat of Egyptian nerve gas attack on Tel Aviv will be comparable to the threat of Israeli nuclear attack on Cairo in the future, or even now. Yet the world would be more at ease if neither side here acquired either nuclear

or CBW weapons. The world is approaching a consensus that nuclear proliferation is undesirable, because such proliferation more often than not increases the risks of war, because it almost always increases the costs of war, and because such proliferation will hasten the day when small dissident factions or criminal syndicates can threaten to blow up a city if their demands are not satisfied. All of such fears apply equally to a proliferation of deadly biological warfare capabilities. CBW indeed might constitute the greater proliferation risk, if underdeveloped and possibly unstable states, or irresponsible political movements, find biology easier to master than nuclear physics. Which weapon will leak out to Black September first, nuclear warheads, or some sort of deadly biological agent? Further CBW research in the U.S., Britain or U.S.S.R. furthers the proliferation risk, for technology once uncovered will leak out. We might well conclude that the H-bomb is all the deadly potential the world needs, or can handle.

As SIPRI Volume V notes, the U.S. argument against an uninspected ban on chemical warfare at times seems almost to assume that there would be no means of retaliating if the Russians suddenly attacked with deadly gases in violation of bans on CBW manufacture and use. Yet the likelihood is hardly great that such a gross assault on American positions would escape nuclear retaliation. More difficult problems in the control of CBW would of course emerge if nuclear disarmament were someday also gotten under way.

This leads us at the end to the question of "inspection." Could we trust the enemy not to stockpile chemical and biological weapons, while we fulfilled our agreements to dismantle all such weapons? Can we ourselves be trusted to fulfill our agreements? Would a reassuring process of monitoring compliance with such agreements have to be disruptive to normal political, scientific or industrial processes?

The SIPRI Volume VI on *Technical Aspects of Early Warning and Verification* is a thoughtful and eclectic collection of papers on these problems. As in other instances of disarmament verification, for example the Comprehensive Nuclear Test Ban or the Nuclear Non-Proliferation Treaty, the study here echoes a Swedish optimism on such questions: verification is necessary, but it can be accomplished at bearable costs. Yet as in these other instances, some other observers may be more pessimistic about whether verification can really be so effective without being excessively obtrusive, perhaps interfering with the disease-fighting pharmaceutical industries of the countries concerned.

Yet some of such concern about the reliability of verification may even be beside the point. Sometimes an arms-control agreement can be self-enforcing. If we really distrust the chemical or biological weapon enough, our statesmen and generals will simply not want to have it around, lest it come into use accidentally, lest by a shift of the wind or other miscalculation we kill more of our own soldiers than of the enemy's. President Nixon's decision to promise never to use biological weapons, even if the enemy used them first, in effect suggests the conclusion that disease will never be a practical weapon for any army that attempts to exploit it. We might as well ask why armies have never employed trained rattlesnakes against each other; on average we would wound two of our own men for every one of the enemy, and an "arms control" agreement not to use this mode of warfare will mostly be an effort to clear the air.

The residual backup deterrent of the atomic bomb, together with the general inapplicability of chemical and biological warfare, might thus truly make inspection and verification unnecessary for bans of CBW. If so, why has our government still resolutely insisted on verification as part of any general ban on the manufacture of such weapons? As SIPRI Volume IV on *CB Disarmament Negotiations 1920–1970* suggests, this insistence may simply reflect years of arms control negotiations in other areas of weaponry, in which the U.S. has instinctively had to demand inspection, and the Russians have instinctively dismissed the need for it.

Alternatively, it might indeed reflect the U.S. government's reluctance to see all forms of chemical warfare banned, perhaps because the prospect of "humane warfare" cannot yet be finally dismissed. More books on the subject may have to be written before this option is fully and finally foresworn.

Tradition and Nationality: A Classic Revisited*

SAMUEL BEER
Harvard University

Walter Bagehot was a frightened Liberal. The adjective is not too strong. Throughout much of his life he was haunted by the fear of social dissolution. A recent biographer finds him "obsessed" with "the problem of authority in the modern state."[1] This intense concern with disorder appears in his letters supporting the coup d'état of Louis Napoleon written in 1851 when he was only twenty-five. It informs his fear in *The English Constitution* that universal manhood suffrage would make parliamentary government "impossible" (pp. 161–163).[2] It is expressed in his panicky response to the passage of the Reform Act of 1867, which led him to put forward in the 1872 Introduction the highly counter-productive proposal that the business and aristocratic classes form a tacit coalition against the menace of a politicized working class (pp. 277–281).

These anxieties may have been based in family experience or personal psychology—there was a history of mental disorder among his near relatives. I want to argue, however, that they arose from a theory of politics. For Bagehot was not the insouciant empiricist that Mr. Crossman took him to be. His perceptions were controlled by an inner eye which at once clarified some brilliant insights and blinded him to elementary facts of political behavior and of British political experience. This inner eye, it is no surprise to say, was formed by classical Liberalism, and Bagehot's blindspots were blindspots of classical Liberalism. If I can put my thesis in one sentence, it is that while Bagehot rightly receded from the bland and optimistic rationalism of that school, he could not overcome its individualism and so was left in a quandary about how to explain the basis of political order.

The Doctrine of Double Government

The two main themes of *The English Constitution* are stated in its opening pages. One is Bagehot's famous discovery of the fusion of executive and legislative powers. His major and complementary theme is the separation of two parts of the constitution: "those which excite and preserve the reverence of the population—the *dignified* parts, if I may so call them; and, next, the *efficient* parts,—those by which, it, in fact, works and rules" (p. 61). While Bagehot admits that this doctrine of "double government" does not hold "with microscopic accuracy," he is quite serious about it and makes the separation of "efficient" from "dignified" apply to institutions, classes, and attitudes. On the efficient side are found the Cabinet and the House of Commons; on the dignified, the Monarchy and House of Lords. For the effective conduct of public affairs, Bagehot looks to businessmen. Yet these members of the middle classes cannot inspire deference. A "manufacturer's son has no chance" in electoral competition with "an honorable, or a baronet. . . ." While the House of Commons does have some "dignity," just as it retains some aristocratic personnel, it is vastly overshadowed by the Lords with their "theatrical show" and the Queen, toward whom "the multitude" feel "reverence." Apart from this function of inspiring deference, however, the traditional upper classes contribute very little. When they do appear in the Commons, for instance, they "generally cannot speak, and often cannot think."

This doctrine of "double government" is such a misperception of the facts of British political experience that it can be accounted for only on the grounds of overriding theory. For certainly a leading trait of the British polity with regard to institutions, classes, and political culture has been not the separation, but the fusion of the dignified and the efficient, the deferential and the pragmatic, the traditional and the modern. If we can believe Tocqueville, Taine, Mosca, Schumpeter, and Lowell, the role of the traditional upper classes in the 19th and 20th centuries was not only to attract deference, but also to supply leadership. And generally Britain survived with relative success the various crises of modernization in large part because of the fusion of elements whose separation would have promoted institutional deadlock, class conflict, and civil violence.

Bagehot's Concept of Tradition

The immediate source of Bagehot's misperception was his concept of tradition. We can get a clearer view of the ultimate theoretical basis of this idea if we look at *Physics and Politics* (1872), a work that elaborates the theory of political development presupposed by *The English Constitution* and briefly sketched in its concluding chapter. In the later book, Bagehot's central con-

* Walter Bagehot, *The English Constitution*. With an Introduction by R. H. S. Crossman, M.P. (New York: Fontana Paperbacks, 1963. Pp. 312. $1.50.)

[1] Alastair Buchan, *The Spare Chancellor: the Life of Walter Bagehot* (London: Chatto & Windus, 1959), p. 186.

[2] Page numbers given in parentheses refer to the Fontana edition.

cern was the founding and development of nations. (The title of the German translation was appropriately *Der Ursprung der Nationen.*) The term he used to identify this concern was "nation-making." And given his premises, the making and maintenance of a nation must indeed have appeared as something of a problem.

Early man, according to Bagehot—and this is a crucial assumption—was a creature of unpremeditated impulse, neither social nor rational. In the struggle for existence, however, that group tended to survive which acquired a cohesive polity based on rigid identities of behavior—a "cake of custom." The origin of any cake of custom was imitation, which Bagehot regarded as instinctive. By "chance predominance," to use his Darwinian language, a model of behavior appeared, was imitated and hardened into custom. Once order had been established by such a nation-making tradition, the way was open for the rise of rationality, first embodied in "government by discussion," then in science and business.

Tradition, so conceived, is rigid, repetitive, unreflective, an "hereditary drill"[3] handed down from past generations. Intrinsically, it is opposed to rationality, which operates by eliciting and propagating new ideas. Given this fundamental opposition, the institutions, classes and norms of political culture produced by tradition and reason respectively could never be fused. And the efficient and dignified parts of the constitution must remain separate.

This wrongheaded view of tradition accounts for the error in Bagehot's main thesis in *The English Constitution.* But it has a further theoretical background that is also illuminated by *Physics and Politics.* Bagehot revealed the premises of his analytic thought in the chronology of his developmental scheme. His portrait of early man showed where he agreed with and where he dedeparted from the Liberal creed. With the Liberals he shared a deep-seated individualism. By this I mean the view common at least since Hobbes and Locke that social groups consist of separate, isolated egos, related to one another only externally, not internally. According to this Liberal view, individuals modify their behavior by rational calculation, as when by contracts, individual or social, they create cooperative arrangements. Such relations with others, however, do not change the inner man, his purposes and motives: they change only the means by which he pursues these purposes or expresses these motives. Still, for conventional Liberals such systems of rational calculation and conscious self-control sufficed as the basis for order. Thus, in the polity they trusted to the outcomes of "government by discussion" to produce stability and progress, just as they trusted to analogous processes in the free market to produce economic equity and growing wealth.

In much of what he wrote Bagehot displayed the same confident rationalism. For him the great engine of progress was "government by discussion": it had enabled some men first to break out of the fixed polity and then to develop the remarkable powers of the modern state. But Bagehot was not just an unsolemn version of John Stuart Mill. Departing sharply from Liberal convention, he saw that its bland rationalism did not and could not account for political order and that in the making and maintenance of nations another powerful force was at work. He characterized this force in his vivid and irreverent way variously as "dignity," "deference," "stupidity," and so on, but it comes down to some aspect of the national tradition. Tradition gave "dignity" to institutions, established "deference" between classes and maintained "stupidity" as a body of unquestioned norms and beliefs. He saw the immense power of this nation-building force over emotion and motivation, while the conventional Liberal could only lament its influence and neglect its political function. When Bagehot tried to understand tradition, however, his individualist prepossessions permitted him to see it only as an external relation among isolated egos. The best he could do was to call it imitation and account for its power by postulating an independent instinct.

The Menace of Rationalization

This analysis shows the connection between two curious aspects of Bagehot's writing: his insistence on the separation of the dignified and efficient parts of the constitution and his fears of political disorder. The key is his conception of tradition as imitation. This conception fits with, although it is not a necessary consequence of, his atomistic view of society. To look at tradition in his way is to set it in fundamental opposition to rationality. As Bagehot wrote: " . . . discussion and custom cannot be . . . combined; their 'method' as modern philosophers would say is antagonistic."[4] Since imitation is based on instinct not reflection, once an item of imitated conduct is subjected to rational examination, it loses its imitative character.

This radical incompatibility means that, although a single polity may contain at one time elements of rationality and tradition, they can only be mixed or balanced in some mechanical

[3] *Physics and Politics* in *The Works and Life of Walter Bagehot,* ed. Mrs. Russell Barrington, 10 volumes (London: Longmans, Green, 1915), VIII, p. 18.

[4] *Physics and Politics,* p. 112.

way, but not combined, or fused in institutions, in the role of a governing class or in the norms of a political culture. In an appropriately mechanical image, Bagehot compares the effect of the dignified parts on the efficiency of the English polity with the "solely-ornamental wheels introduced into the clocks of the Middle Ages, which tell the then age of the moon or the supreme constellation; which make little men or birds come out or in theatrically." "All such ornamental work," he notes, "is a source of friction and error" (p. 207).

The process of rationalization is, moreover, irreversible. Rationality can take over subjects from tradition, but tradition cannot reclaim them. "Once effectually submit a subject to that ordeal [i.e. discussion]," he writes, "and you can never withdraw it again; you can never again clothe it with mystery; or fence it with consecration; it remains ever open to free choice, and exposed to profane deliberation."[5] In Bagehot's theory once a society is launched on modernity, rationality and its products will inevitably crowd out and ultimately extinguish tradition and traditionalism. This inexorable process of rationalization erodes the great foundation of order and threatens to release the old barbarous passions. Looking at his world from the vantage point of these prepossessions, Bagehot could not fail to fear Disraeli's bold extension of the suffrage. The same theory that distorted his perception of the constitution also exacerbated his fears for its future.

The errors resulting from this theory were numerous: the inability to perceive the fusion of reason and tradition in British political life; the conclusion that tradition must be continually wasting away under the assault of reason; the implication that the bonds of order represented by tradition cannot be replenished in the modern age. The essence was a quite extraordinary failure to understand the nature and strength of the modern nation-state as a political community.

Misperception of the Cabinet

Bagehot's individualist perspective also distorted his perception of the Cabinet, whose nature and functioning in certain vital respects he did so much to reveal. The lasting power of his analysis of the Cabinet is not the mere empirical discovery of the fusion of executive and legislative powers, but rather his understanding of why this fusion is a necessity of modern government. His premise is an appreciation of the distinctive character of government in modern times. This is an age when policy must be "continually changing" (p. 70). In attempting to meet this constant need for change, the men in control of government may not be able to agree. In particular, if those who make the laws, levy the taxes, and authorize the expenditures are different from those who execute the laws and collect and spend the revenues, there will be disagreement and conflict between them. "The tax imposers are sure to quarrel with the tax-requirers. The executive is crippled by not getting the laws it needs, and the legislature is spoiled by having to act without responsibility . . . " (p. 70).

A more confident rationalist would have trusted to the mechanisms of government by discussion to resolve such differences. Bagehot, on the other hand, saw the fusion of executive and legislative powers as a necessary condition, if British governments were to rule with some degree of coherence. In explaining this fusion, he stressed the norms of collective responsibility and the ties of party. What he refused to consider in his brilliant analysis was the role of tradition. Clinging obsessively to his doctrine of "double government," he argued at length that the Cabinet belonged to the efficient part of the constitution and was not substantially affected in its operation by the dignified parts, the Monarchy and House of Lords.

What this argument obscures is, in the first place, the effect of Britain's monarchic past upon the origin and rise of the Cabinet. As L. S. Amery observed,[6] the British constitution has always had two elements: an initiating, directing, energizing element—formerly the Monarch, and nowadays the Cabinet—and a checking, criticizing element —formerly Parliament as a whole, and nowadays especially the Opposition. This bipolar conception of authority, arising in the early days of the British polity, survived as a force shaping the development of the Cabinet. In sociological language, one might put the point by saying that the monarchic role was assumed by the Cabinet, endowing this plural executive with norms of collective responsibility. It made a vast difference that this plural executive was the offspring of a unitary executive. Serving a common master, ministers in the early Cabinet learned to act in unison before they had freed themselves from that mastery. Before their responsibility to Parliament was established, ministers were taught by the King the responsibility of taking the initiative in policy. Owing their office to a chief because he was in fact the King's first minister, they learned that their tenure depended upon his tenure well before he became the nominee of a party. In short, while the Cabinet was the core of the efficient part of modern British government, it could not have performed this task if it had not inherited a code of conduct from the dignified past. Here as

[5] *Ibid.*, p. 104.

[6] *Thoughts on the Constitution*, 2nd ed. (London: Oxford University Press, 1953).

elsewhere tradition was so far fused with rationality as to constitute a necessary condition for the effective operation of rationality.

The distortions of Bagehot's doctrine of double government are even more serious if one looks toward future rather than past development. For an essential element of his view is that the efficient parts of the constitution cannot acquire "dignity." This conclusion follows logically from the radical opposition he sets up between reason and tradition. In conformity with this theory, the Cabinet is portrayed as an agency of purely instrumental significance, lacking in "theatrical show," and incapable of arousing reverence as an institution.

This is a wrong reading of the facts then and now. Feelings about the Cabinet include more complex elements than merely a clear-eyed assessment of its instrumental operation. Certainly, that is true of the members of the Cabinet themselves. An ex-member recently described how ministers, while gathering at No. 10 Downing St., laugh and joke and call one another by their first names, as one would expect of party colleagues. But once they enter the Cabinet room, and the meeting starts, "the whole atmosphere changes." Members now address their remarks only to the Prime Minister, and even the oldest friends refer to one another in the third person and by their ministerial titles. For "here is no longer a set of individual men, but the collective sovereign power of the state."[7] The Cabinet is "not just a meeting of persons, but a continuing body, that has for centuries been the seat of ultimate authority."[8] Such sentiments, I submit, are what Bagehot meant by "reverence."

Fusion of Tradition and Reason

My argument has been largely negative. The main thrust has been to show that Bagehot's theoretical presuppositions—especially his Liberal individualism—distorted his analysis of British government and politics. His error is also of general interest to political science. The rigid separation of tradition and reason of which Bagehot was guilty still menaces thinking about modernization and political development. The typologies of even the great Max Weber tend to propagate this distortion. The consequent misunderstandings also often color judgments on present day society, leading critics who are fearful of modernity and its effects on social cohesion, to take a far more pessimistic view of our plight and prospects than the facts warrant, just as Bagehot did a hundred years ago.

[7] Patrick Gordon Walker, *Encounter*, 6 (April, 1956), p. 19.

[8] Walker, *The Cabinet* (London: Jonathan Cape, 1970), p. 106.

BOOK REVIEWS

Youth in Transition, Vol. I: Blueprint for a Longitudinal Study of Adolescent Boys. By Jerald G. Bachman, Robert L. Kahn, Martha T. Mednick, Terrence N. Davidson, and Lloyd D. Johnston. (Ann Arbor: Institute for Social Research of The University of Michigan, Third Printing 1972. Pp. xii, 269. $8.00, cloth; $5.00, paper.)

Youth in Transition, Vol. II: The Impact of Family Background and Intelligence on Tenth-Grade Boys. By Jerald G. Bachman. (Ann Arbor: Institute for Social Research of The University of Michigan, 1970. Pp. xvi, 289. $7.00, cloth; $5.00, paper.)

Youth Look at National Problems: A Special Report from the Youth in Transition Project. By Jerald G. Bachman and Elizabeth Van Duinen. (Ann Arbor: Institute for Social Research of The University of Michigan, 1971. Pp. ix, 98. $2.00, paper.)

Youth in Transition, Vol. V: Young Men and Military Service. By Jerome Johnston and Jerald G. Bachman. (Ann Arbor: Institute for Social Research of The University of Michigan, 1972. Pp. xiv, 254. $8.00, cloth; $5.00, paper.)

The Youth in Transition Project (YIT) was undertaken in 1965 to chart longitudinally the social and psychological development of a cohort of male high school students. Funded principally by the United States Office of Education, and additionally by the Departments of Labor and Defense, the project has sought to assess the causal linkages between the objective environments of an individual and his physical and mental states. The publications listed above constitute approximately half of the output of the project to date.

Volume I provides an overview of the longitudinal design employed by the researchers and a justification for their selection of particular instrumentation. Data were collected by interview, written questionnaire, and standardized test for a nationwide stratified cluster sample of approximately 2200 boys beginning in the fall of 1966 as all were entering tenth grade. Categories of inquiry included, among others, the aptitudes and abilities of each subject, his motivations, affective states, values and attitudes (including a limited number of expressly political items), family and peer relationships, and plans and expectations for the future. This information was supplemented by questioning of principals, teachers, and guidance counselors in the schools attended by the subjects, and by an evaluation of community characteristics. Test items and survey instruments are included in a lengthy set of appendices.

Volume II presents the first substantive results of the YIT inquiry. Since longitudinal analyses were not yet completed at the time of publication, the volume emphasizes the relationships among selected variables at Time 1. Socioeconomic level is found to be the strongest predictor to such dependent variables as intelligence and ability, school grades, college plans, occupational aspirations, feelings of efficacy, and political knowledge. In addition, family size and home life, religious preference, and race are also assigned some explanatory power. The findings on race are particularly interesting since the evidence presented indicates that most of the variance between racial subgroups may be attributed to regional and socioeconomic differences among the subjects. This point might have been made more conclusively, however, had the authors chosen to include a comparison of poor, rural, Southern blacks with poor, rural, Southern whites.

Volume V in the series, which was published some two years later, represents an attempt to apply the YIT informational base to a particular policy problem, the development of an all-volunteer military force. Drawing upon their data on personal and social attributes, the authors assess the motivations for and likelihood of enlistment in the armed services, and the temporal changes in preferences for the military and for other occupations. They conclude that there is no particular "military type" with a distinctive social or psychological profile, but that because there is, in effect, a "college type" (higher SES, more skills and accomplishments) who self-selects out of the pool of potential military manpower, an unrepresentative segment of the grade cohort is ultimately available for enlistment. This finding has implications for the timing of the recruiting effort, the type of incentives which may prove effective in recruiting a larger number of highly competent young men, and the nature of the relationship between recruiters and school personnel.

The final YIT offering to be reviewed here, the special report on youth and national problems, may be of most direct interest to political scientists. This monograph provides an overview of attitudes among the YIT cohort on such issues as Vietnam, national unity, racial tensions, crime, pollution, and population control. While taking advantage of some of the longitudinal data developed by the project, the report is based primarily upon the final data collection of the series, which occurred during the summer of 1970. Al-

though the analysis is at times rather trivial and fails to identify the sources of variation in young men's attitudes, one significant (though not surprising) conclusion does emerge: Treatment of the Vietnam and population issues in particular suggests that an individual's perceived social distance from an issue or problem influences not only the salience which he attaches to his related attitudes, but the direction of those attitudes as well. Thus as the YTT cohort approached high school graduation, not only did the Vietnam War become more important, but attitudes toward it became more unified and increasingly negative. In contrast, attitudes on the more remote (for high school boys) yet highly publicized issue of population control were generally more diverse and frequently inconsistent with one another.

Without question, the Youth in Transition Project has developed a large and diverse set of data which offer much promise for many lines of inquiry. Project variables encompass attitudinal, experiential, behavioral, and environmental phenomena within a highly functional developmental framework. But at the same time, both the project itself and its various publications suffer from several fundamental defects which may seriously compromise the realization of this potential.

To begin with, the authors are aware of, yet somehow insensitive to, differences in the scalar qualities of their data. In particular, correlation coefficients (r) are frequently reported for nominal and ordinal data, and when interval data are available, the correlation ratio (*eta*) is employed almost exclusively. While this latter procedure may be justified when the assumption of linearity appears inappropriate, it must be noted that *eta* is not sensitive to ranking, and therefore may not always prove an adequate substitute for Pearson's r. In addition, the format selected for the numerous graphs in Volume II tends visually to minimize the dispersion of the data, and may be somewhat deceptive. This "creative" use of statistics constitutes a disservice to the unsophisticated reader.

These statistical indelicacies are accentuated by the authors' penchant for attributing great significance to relatively inconsequential results. In point of fact, very few of the bivariate relationships reported in these volumes account for as much as three per cent of observed variance, and multiple classification analysis using *eight* principal independent variables rarely accounts for even twenty per cent.

Related to this statistical insensitivity but even more fundamental is the authors' rather unsophisticated approach to the construction and evaluation of indexes. Numerous attitudinal scales based upon ordinal responses have been assigned interval scores (means) and treated as interval variables. No efforts have been reported to assess either the validity or the reproducibility of these indexes. At the very least, the authors should have tested for and reported the degree of unidimensionality of their scales, or should have clearly indicated that they did not assume organization around a single factor. Implicit adoption of the latter position may help to account for the relatively low levels of explained variance.

The one variable cluster which does *appear* to have considerable explanatory power is the index of socioeconomic status. Close inspection reveals, however, that the components of this index have in fact been selected not because they accurately reflect the dimensions in question, but precisely because they are the best predictors. Thus the development of the index is circular, and, the authors' appended protestations to the contrary notwithstanding, the reported results are in fact misleading.

Finally, and more substantively, the authors fail in these works to develop adequately their theoretical perspective or to assess the relationships between their own findings and those of other researchers. They offer simplistic explanations for several of their observations (e.g., the increased dissatisfaction and distrust of government noted in the cohort over time is attributed to a parallel increase among the general population), and virtually overlook the significance of others. Indeed, one might characterize the authors as literally wallowing in data, engulfed in so much information that they are unable to organize it in any nontrivial fashion. Thus the weakness of their analysis may be a function of the richness of their data, and the true lesson of YTT may be that manageability must be a central consideration in the planning of any such large-scale research. Despite this organizational shortcoming, YTT data should prove a veritable cornucopia for secondary analysis.

JAROL B. MANHEIM
The City College, CUNY

The Politics of Authenticity: Radical Individualism and the Emergence of Modern Society. By Marshall Berman. (New York: Atheneum, 1970. Pp. 320. $8.95.)

This book is a model of a new kind of scholarship, passionate or committed scholarship. Its author attempts to look at the past by way of what he believes to be the most important issue of our times: the repression of the self by the system. The book, according to its author, comes at a favorable moment when one can again after the disappointments of Stalinism hope for a political solution to the problem of the self, a moment when political and self-consciousness can go hand in hand. The approach to the works he interprets

is determined by his view of the world: Marxist in its revolutionary appeal and in its economic understanding of the sources of alienation and false consciousness; Freudian in its apparent assertion that sex is the backbone of the self; existentialist in its treatment of the self and its concentration on authenticity; above all New Left in its commitment to unprogrammatic political change and in having as twin goals freedom, understood to mean being and doing whatever one wants to be or do, and community. In short this small book bears the weight of all the most dazzling intellectual positions of the day. Whether it can make them coherent is another question. And even if, improbably, this question were answered in the affirmative, it would further have to be asked whether this reading of Montesquieu and Rousseau does justice to the integrity of their teachings.

This issue of the adequacy of the interpretations becomes pressing from the beginning of the book with the presentation of Montesquieu, which is a sort of preface to the much fuller treatment of Rousseau. Berman speaks only of the *Persian Letters*, which he asserts, without serious argument, to be in contradiction with the *Spirit of the Laws*. He concentrates largely on the erotic themes connected with Usbek and his harem and draws therefrom that Montesquieu was criticizing the *ancien régime*, which is true, and that he was a radical individualist, which is not true, at least in the sense intended by Berman. Berman is at his strongest in characterizing the ways in which certain kinds of society enslave and manipulate men; at his weakest when he tries to elaborate what either his philosophers or he himself thinks is the good or nonalienated society. This weakness is not fortuitous; it stems from his religious devotion to the self, undefined, undefinable, unlimited and unlimitable, the source of all value and all right. Berman knows what is bad—alienation—but he does not and cannot tell us what is good. With Montesquieu, all to the contrary, a clear view of what is good, what man's nature and natural needs are, is elaborated, and his satire is based on the contrast between established practice and natural right. Berman simply adopts the critical part of Montesquieu's teaching, because it can be misread to imply that anything goes, that any constraint of inclination is tyranny, and rejects as a conservative loss of nerve the positive teaching about what is legitimate behavior and what is not. Montesquieu's teaching concerns civil freedom, not authenticity, for a being that has a nature, not a self.

Berman's mode of interpretation is most pernicious in his treatment of Rousseau. He divides that treatment into three parts: Rousseau on alienation; his politics of authenticity; his politics of inauthenticity. Alienation is in turn divided into two parts, that in traditional society and that in modern society. This division is Marxist in inspiration, based on the different human effects of property relations in feudal and bourgeois society. It is not totally foreign to Rousseau, for he was the first to use the word *bourgeois* in its contemporary meaning, and he described with unrivaled eloquence the differences between aristocratic and bourgeois societies and men. But Berman is totally misleading in giving the impression that property relations are responsible for alienation in Rousseau's teaching. No new society, no upheaval in the modes of the distribution of property, can overcome the tension in man between living for oneself and living for and in terms of others. In an example of his always loose reading of books, Berman gives the impression that in the *Discourse on the Origins of Inequality* Rousseau makes property the source of "inauthenticity," of the distortions resulting from *amour-propre*. This is not so. Men's hostility to one another and their dishonesty to themselves and others began prior to the establishment of anything that could be called property. It is in the nature of man, according to Rousseau, to care only for himself; and when he gets into contact with other men, when he needs them, he must act as if he cares for them and can be useful to them; and thus he begins to define himself in relation to them. This is a contradiction in the *nature* of man's individuality in relation to his sociality. There is a profound difference between Rousseau and Marx on this point, and it explains much of the difference in their solutions to the political problem. For Rousseau, the problem is ultimately in man, not in society. Thus Berman's entire discussion of *amour de soi* and *amour propre*, the key terms in Rousseau's psychology, is, to say the least, defective.

Rousseau does, so to speak, nothing but explain what the self is and is not; but so soon as one does that, certain kinds of belief and conduct must be praised and others condemned. In other words, limits must be set. Rousseau's goals are the natural man and the moral man, not the authentic man. The authentic man, for Berman, is one who does whatever he wants, the inauthentic man the one who does what others or the system wants. For Rousseau, to be sure, a man can be free and whole only if he does what he wants; but he must also want what is in accordance with his nature; freedom without limits imposed by nature or moral law leads only to the worst sort of alienation. Berman manages to hide and distort Rousseau's position by saying he was seeking for authenticity; then, like a magician in a circus, he cuts Rousseau in half; what was once an organic whole becomes two distinct personages, the authentic Rousseau and the inauthentic Rousseau, the former freeing from all constraints, the other

imposing the worst of all tyrannies. To perform this work of magic, Berman must take books that are thought-out unities and treat them as though they were written by Dr. Jekyll and Mr. Hyde taking turns sentence by sentence.

Berman's discussion of Rousseau's authentic side is platitudinous, admiringly trotting out all the things from which Rousseau wanted to liberate man. Then, with the ferocity of a hippy Savonarola, he turns on Rousseau for betraying the erotic faith. Rousseau is accused of the most terrible crimes—believing in original sin, suffering from sexual neurosis, being a dropout. In a breathtaking peripety, Rousseau becomes the most totalitarian of totaliarians, not so much the source of the totalitarianisms of the Terror or the Soviet Union, but of that most terrible of totalitarianisms, modern liberal democracy, e.g., the United States.

I shall leave to the reader the excitement of seeing how this comes to pass, but something must be said about the source of Berman's fury. It begins in the limitations Rousseau puts on sex. Berman takes it for granted no harm can come of any form of indulgence whatsoever, so long as it comes from an authentic self. He pays no attention to Rousseau's subtle arguments about the relation between sex on the one hand, and love, art and morality on the other. Rousseau attempts to establish, on the basis of man's fixed nature, what things can go together and what not. He sees in sex one of the prime sources of selfishness, dependence and exploitation of others, as well as one of the possible bases of love, society and happiness. This ambiguity, this notion that man might have something in himself to control and overcome, is intolerable to Berman. Similarly he refuses to see anything but hardness of heart in Rousseau's unwillingness to give way to Emile's tears, although Rousseau insists that tears are the first source of alienation, the root of the desire to master and of the sentiment of enslavement. Finally, the moral demands of a free society, the alienation of the particular will to the general will to avoid its alienation to the particular wills of other men, does not impress Berman. What Kant saw as the most sublime of all moral teachings, the only basis of freedom and the dignity of man, and which by way of him became the core of Hegel and Marx, is for Berman deception and tyranny. He has thought none of this through. Similarly, he takes Rousseau's preference for solitude as a "dropping out," a weakness of old age. But that preference was there from the beginning and is not the result of Rousseau's peculiar psychology. It has to do with the necessary, the intransigent, tension between human nature and society. The real satisfaction of man is in the sentiment of existence which can best be experienced alone, at the end as well as in the savage beginning.

The source of Berman's blindness and his sloppy reading is in his dogmatic presupposition that there is a harmony between the growth of the self and the demands of civil society. It may be so, but surely Rousseau is the most powerful critic of that view. He is, to be sure, the source of radical libertarianism, radical egalitarianism and the demand for a real community as over against the institutions of liberal society. But it is also the case that for him individual desire poses a problem for community. His reflection on the ancient city, which Berman regards as so dangerous, was Rousseau's attempt to see what a participatory community really is. What common good can there be for men of unique, individual selves? Letting go, which is really what Berman is selling, would, according to Rousseau, release not the expanding self but the passions of nature bloated by bourgeois society, engendering monsters of vanity lacking even the bad conscience which acts as a corrective within that society. Rousseau contains both the inspiration for and critique of the New Left. That is why he is so paradoxical to Berman. His commitment makes it impossible for him to study Montesquieu or Rousseau seriously and turns his book into propaganda rather than scholarship.

ALLAN BLOOM
University of Toronto

Tocqueville. By Hugh Brogan. (London: Fontana Paperbacks, Collins, 1973. Pp. 95. $1.50, paper.)

It is difficult to decide for which public this short book has been written. For those who have studied Tocqueville's work, it does not contain one sentence which could be regarded as original, and for those who wish to be initiated to Tocqueville's thought, one must express a warning. Mr. Brogan measures Tocqueville by his own prejudices, which are at times either arrogant or journalistic.

He gives first some hints of what he terms, "the problem of Tocqueville." Here the worldwide Tocqueville renaissance is reduced to George Pierson's book, *Tocqueville and Beaumont in America*, but in fact the Tocqueville revival begins with Dilthey, the great German philosopher-historian, although this is not the place to review the history of the Tocqueville revival.

I cannot resist the temptation, however, to quote a few lines from Mr. Brogan (p. 10), where he deals with the Tocqueville renaissance:

> Tocqueville was also assisted by the boom in sociology. He was unquestionably a sociologist, and if he had a fault, it was a tendency to take himself too seriously. American sociologists, whom their worst friends have never found it necessary to defend against charges of

frivolity, took him to their hearts; and as American political science followed along the path of solemn unintelligibility Tocqueville's writings were doubly combed for "insights," which, rendered down into jargon . . . could enhance the reputation and salaries of gentlemen with academic careers to make and academic families to feed . . .

This quotation gives the reader of this review perhaps an idea of Mr. Brogan's attitude towards his American colleagues. One wonders whether he has ever read Riesman or Nisbet, whose works are so deeply imbued with Tocquevillean ideas.

In two further chapters, Mr. Brogan appoints himself a supreme judge of Tocqueville's *Democracy in America*. Here he is not less condescending or insolent. He fails to understand the meaning of the "tyranny of the majority," which so deeply impressed minds like Burckhardt, John Stuart Mill, or later, Dilthey, Acton, and Ortega y Gasset. Mr. Brogan knows so little of sociology that he does not see that the "tyranny of the majority" is only another term for the phenomenon of the mass society, which was Tocqueville's primary concern.

Mr. Brogan writes at another page (p. 45):

Unhappily his book [*Democracy In America*] was not rewritten, and from that day to this everyone with a grievance, real or fancied, against the American people, has found in Tocqueville a plausible, high-toned justification for complaining of tyranny. And the vanity of authors, who of course always deserve enormous sales, which only a conspiracy can cheat them out of, has been much soothed by his words. He himself was probably suffering from a professional hangover. *An Enlightenment writer, born out of his due time, he was hankering for the irresponsibility of the eighteenth century.*

I have allowed myself to italicize the last sentence in order to illustrate the miscomprehension of Tocqueville's mind. Few political scientists have so profoundly anticipated the future. Mr. Brogan regards Tocqueville as a belated eighteenth-century writer, completely ignorant that his basic approach was that of a jurist; hence his affinity and his rank, equal to Bodin or Montesquieu.

Of Mr. Brogan's appreciation of the second part of *Democracy in America*, two passages should perhaps be quoted:

Tocqueville, reasoning from the speculative to the unknown, is boringly fanciful, and it becomes hard to forgive him for his solemnly pedagogical air. At other times some flash of insight, information or common sense will redeem him. The trouble is that it can never be predicted which way the worm will turn (p. 50).

How witty and elegant!

Another choice example:

What is one to say of this mixture of shrewdness and the higher silliness, except that it is typical of *Democratie II?* (p. 51).

I do not know what "higher silliness" means, but I do know that *Democratie II* is regarded, without exception, as a classic of sociology.

After these examples I do not think it is necessary to discuss Mr. Brogan's treatment of the *Souvenirs* or the *Ancien Régime*. Yet even the *Ancien Régime* returns to the theme of the danger of the mass democracy. I quote only one passage, taken from the Preface:

Since men are no longer bound to each other by links of caste, class, association or family, they tend only too easily to concern themselves with their private interests. They are always only too inclined to consider themselves alone and to withdraw into a narrow individualism where every civic venture is stifled. Far from fighting against this tendency, despotism makes it irresistible, for it takes away from the citizens every common passion, every common need, any necessity to understand each other, any opportunity to work together. It walls them up, as it were, in their private lives. They were already disposed to keep themselves apart; despotism isolates them; they become cold towards each other; despotism freezes them.

If one attempts to understand Tocqueville, his main works must be linked together. Both *De la Démocratie en Amérique* and his *Ancien Régime* are comparative studies in State administration. Tocqueville asks himself what are the consequences of centralized administration for the existence of the human being, and what can we or should we do to safeguard our threatened freedom. He may have been more explicit as an analyst than as a reformer, but his questions concern us today even more than ever, and Mr. Brogan's book contains little trace of them.

J. P. MAYER

University of Reading, England

Christian Political Theory and Church Politics in the Mid-Twelfth Century: The Ecclesiology of Gratian's Decretum. By Stanley Chodorow. (Berkeley: University of California Press, 1972. Pp. 300. $15.00.)

The present monograph deals with a subject of considerable interest to historians of medieval political and legal thought as well as to students of ecclesiastical institutions. The importance of Gratian's *Decretum* is granted on all hands, but, surprisingly enough, hardly any attempt has ever been made to understand it on its own terms or determine its immediate political significance. Chodorow's essay fills this gap in a commendable if not altogether satisfactory manner and argues persuasively for the dogmatic and methodological novelty of Gratian's work. Its central thesis is that the *Decretum* is not a mere compilation of canons or a piece of timeless legislative wisdom

but rather a theological treatise of sorts, whose guiding principles echo the platform of the reform party which emerged during the early decades of the twelfth century, in the wake of the Investiture Contest, under the leadership of the Chancellor Haimeric, St. Bernard, Peter the Venerable, Gerhoh of Reichersberg, and a host of lesser figures. The ecclesiology propounded by the new curial powers emphasized the spiritual nature of the Church over against the worldliness of the Gregorian reformation while at the same time advocating a more conciliatory attitude toward the Empire in accordance with the compromise worked out in the Concordat of Worms (p. 17 ff.). Among the leaders of his generation, Gratian appears to have been the only one to think through the political implications of the "reform of the reformation" (cf. p. 27). Viewed in the perspective of the 1120s and 1130s, his *Decretum* may be described as an attempt to endow the Church, now free from the control of the secular authority, with a juridical constitution properly its own. Its basic thrust is revealed as much by its omissions as by its express statements. Contrary to earlier practice, secular codes of law (specifically those of Theodosius and Justinian) are never mentioned by name in the *Decretum* (cf. p. 15, 150, 209, 220); nor does Gratian appeal to the authority of Gregory VII whenever such an appeal could be construed as lending support to Gregory's followers (p. 202 ff.).

To the tensions implied in the dual nature of the Church as a mystical body and a juridical entity may be traced most of the ambiguities and inconsistencies that Chodorow claims to have uncovered in the *Decretum*. One wonders at times whether the alleged confusions are those of Gratian or of Chodorow himself. The discussion of the relation between divine and human law would have gained in clarity and sharpness if a more concerted effort had been made to link Gratian's views to the broader tradition of medieval political theory stemming above all from Augustine. The translation of *ius naturale* by "natural law" rather than "natural right" (p. 105) merely adds to the confusion and makes it that much harder for Chodorow to explain in what sense the legislator is, or is not, bound by his own laws (p. 135 ff.).

Students of political thought should be warned that the *Decretum* contains little or nothing by way of a general theory of Church and State. The canons devoted to this subject (cf. p. 211 ff.) deal on a more practical level with the manner in which the two powers were expected to coexist and cooperate within the framework of the Christian society of the Middle Ages. Faint indications here and there (e.g., pp. 85, 186) suggest that Chodorow shares the increasingly popular but highly debatable view that the roots of modern democracy are to be sought in medieval constitutionalism rather than in the egalitarian natural law doctrines of the seventeenth century. However that may be, Gratian himself was no democrat, as is evident from his stand on the place of the Pope not only at the head of the hierarchy but apart from it (p. 179) or on the use of the material sword to coerce heretics (p. 223 ff.). The reader is left with the impression that by providing the Church with a legal code adapted to its needs as a separate and independent juridical community, Gratian helped set the stage for the great confrontations of the following century. In light of this fact, one is inclined to agree that the *Decretum* is "one of the most significant works of political theory written in the mid-twelfth century" (p. 7). The issue on which further debate among scholars may be anticipated is whether or not the cleavage between the Gregorianism of the eleventh century and the reform theology of the early twelfth century is as deep as Chodorow's intriguing and highly plausible hypothesis makes it out to be.

ERNEST L. FORTIN

Boston College

Size and Democracy. By Robert A. Dahl and Edward R. Tufte. (Stanford, Calif.: Stanford University Press, 1973. Pp. x, 148. $6.95.)

The dilemma of democracy, as Dahl and Tufte see it, is rooted in the ineluctable circumstance that every unit of government is constrained by its size (especially of population, but also of land area, natural resources, gross national product, and so on). If, as they posit, citizen effectiveness and system capacity are equally important criteria for judging the viability of a democratic polity, the intrusion of the size factor makes for democracy's dilemma: all other things being equal, the smaller the polity, the more will citizens act responsibly and competently in controlling public decisions, but the less will be the system's capacity to respond to citizen preferences. Citizen participation in collective decisions and the collectivity's ability to respond to citizen preferences are inversely related. As a result, the determination of a governmental unit's optimal size necessarily involves a tradeoff between the two criteria of democracy.

This theme Dahl and Tufte pursue with relentless logic and a good deal of empirical evidence. They pursue it at varying levels of governmental complexity—in terms of countries, of equivalent units within countries, and of local versus national units. They pursue it in terms of classical propositions about the optimal size of the polity, from Aristotle to John Stuart Mill, and they pursue it in terms of aggregate statistics and survey data. They introduce, where necessary or desirable, considerations of social and political pro-

cesses, like representation, coalition politics, group conflicts, or party competition, that often confound the assumption of *ceteris paribus* so essential to theoretical clairvoyance. Where, then, do they come out?

When setting out on their investigation, Dahl and Tufte assumed, as had their predecessors, that it is possible to stipulate some "optimal democratic unit." The assumption proved illusory, and they conclude that "no single type or size of unit is optimal for achieving the twin goals of citizen effectiveness and system capacity" (p. 138). This is so because "democratic goals conflict, and no single unit or kind of unit can best serve these goals" (p. 138). If one were to follow the logic of each criterion to its extreme, citizen effectiveness would be maximal in a system of minimal capacity or minimal in a system of maximal capacity. Clearly, to avoid absurdities of logical inference, something has to give.

What has to give, according to Dahl and Tufte, is the theory of democracy, or at least those "sturdy simplicities of popular democratic thought" (p. 138) which lead to the conclusion that practice is wrong—undemocratic because it is not in line with theory. Rather, "democratic theory will have to leap ahead and not jump directly into the past, as 'new' or 'radical' democratic thought invariably seems to do" (p. 139). In a highly complex and interdependent world, "it seems to us doubtful that solutions will be found in theories formulated in and for profoundly different times . . ." (p. 139). So far, so good. But where do Dahl and Tufte take us from here?

Frankly, I do not quite follow their argument. An adequate theory of democracy, they say, must tackle "the neglected problem of political units and their inter-relations" (p. 139). Units of different size are needed—small ones "to provide a place where ordinary people can acquire the sense and the reality of moral responsibility and political effectiveness . . . "; and very large, possibly transnational units whose existence "will increase the capacity of the system to handle critical problems, and thus the collective effectiveness of a body of citizens . . ." (p. 140). But this surely does not solve the initial problem, as Dahl and Tufte admit: very large units "will also increase the ineffectuality and powerlessness of the individual citizen," and "finding space for truly significant activity in the very small unit is obviously going to be difficult" (p. 140).

What Dahl and Tufte seem to have in mind is some kind of transnational, democratic and federal system. But to say that federalism is needed, they argue, "is not so much to provide an answer as to restate the question, or, at best, to suggest a direction in which a part of the answer might be found" (p. 140). Federalism, they continue, "is not so much a theory of inter-unit relations as a set of institutions, different from country to country, conveniently anchored in the nation-state . . ." (p. 140). I am puzzled, for I have always thought of some form of federalism as at least one theoretical answer, *ceteris paribus*, to the problem of solving the relationships among interdependent units of different size.

Whither, then, is democratic theory to leap? One task, Dahl and Tufte suggest, "may be to specify not an optimal unit but an optimal number of units with comparatively fixed boundaries" (p. 141). And, leaping ahead fearlessley, the authors yet warn that "boundaries of each unit would be too small or too large for all functions assigned to it; but the costs of a small number of units with relatively fixed boundaries would be less than the costs of any larger number of units . . ." (p. 141). All of which leaves me quite perplexed, for as of last count I am living in some dozen units of varying size, and though reasonably competent in these matters, I find my effectiveness as a citizen is only matched by the incapacity of these units to respond to my preferences, not the least important of which is some reduction in the costs that stem from their multiplicity. Unfortunately, Dahl and Tufte do not provide any clue about how to fix an optimal number of units that would get me out of my predicament. How few are few, and how many are many units? How does specifying an optimal number of units differ from specifying the optimal size of a single unit? And would not the optimal number of units depend on the optimal size of the subordinate or superordinate units in which a given function is performed with least cost?

Another task of theory, Dahl and Tufte recommend, is "to clarify the powers, obligations, and characteristics of citizens and leaders, including officials, in democratic units of different types" (p. 142). As specialization is related to size, leaders in large units even now specialize; and just as leaders specialize, "citizens must, and to some extent do, also specialize" (p. 142). But no sooner has this gutsy leap forward been made when we are informed: "Yet indefinite specialization, like indefinite proliferation of units, would lead sooner or later—and may already have—to so much fragmentation as to create insuperable problems of coordination" (p. 142).

I am sorry to be quoting so much, but I want to let Dahl and Tufte speak for themselves, and if I have quoted incorrectly or out of context or distorted their meaning, I will stand corrected. But, it seems to me, Dahl and Tufte's conclusions do an injustice to the main part of their book that is infinitely greater than any injustice that I might do to their epilogue. For *Size and Democracy* is an eminently interesting and elegant analysis of some critical issues of modern democratic theory. The

book is studded with axioms, theorems, hypotheses, inferences and generalizations which can keep a bevy of political scientists happy in their empirical hunting grounds, and I cannot even begin to list some of the many fruitful leads to research that two fertile minds are able to extract from what is a theoretical analysis in the best sense of the term. I must leave it to the reader to find out for himself or herself.

HEINZ EULAU
Stanford University

Two Concepts of the Rule of Law. By Gottfried Dietze. (Indianapolis, Ind.: Liberty Fund, 1973. Pp. 108. $5.00.)

In this concise, compact little book, consisting of two essays, Professor Dietze has examined the relationship between constitutionalism and the law of the state—between the Law State and State Law. Much of the book is devoted to aspects of German constitutionalism and the development of ideas in Germany, making it an important study of social movements and the "history of ideas." Moreover, although Dietze does not make it so explicitly, the book is extremely relevant for a United States trying to contend with an administration which, while it came to office emphasizing "law and order," has meant any order and any law, without regard to substantive content. Dietze tells us, for example, that dangers to freedom are greater when gradual, because there is less resistance, with the Law State (or Just State) being restricted by State Law.

One of the author's most important themes is that justice and injustice, State Law and the Law State, are matters of emphasis, not opposition. Just as State Law restricts the Law State, State Law is necessary to protect the Law State. Also, "without injustice, there is no justice" (p. 13), like Edmond Cahn's observation that we will never know justice except through a "sense of injustice."

The theme of the first essay—"The Just State and State Justice"—is that a Law State may be only formal in nature, making it susceptible to takeover by virtually any political ideology, including Hitler's. We must, Dietze suggests, keep in mind the liberal commitment which underlies the formal rules, so that the emphasis in our "rule of law" is not on "rule" or "order" rather than "law." Dietze says that while the German Basic Law provides much protection for freedom, a strong possibility exists that the "social" nature of the German Law State may dominate, because "the Just State has not regained its primacy over the state's own concept of justice" (p. 45), with constitutional government "as much determined today by the law of the Federal Republic as it was previously by the law of the Weimar Republic and Empire, i.e., by temporary beliefs" (pp. 45–46).

In the first essay, Dietze says strengthening of governmental power is to be feared in democracies as in any other form of government. But one also has to fear permissiveness, the theme he sounds in the second essay, "State Justice and the Just State," which is less balanced in tone. Here, except in talking about Germany, Dietze is at his most normative, showing his unhappiness with both democracy and American "permissiveness": "As a result of the New Deal and the New Frontier, there spread, under the Warren Court, a permissiveness . . ." making the 1960s "the most terrible period since the Civil War" (p. 62). He also argues that "there is inherent in each liberal the danger of undue permissiveness" (p. 67) because for the liberal the measure of all things is man.

The valuable point that we may "sentimentalize" away the Law State is damaged by overblown language about democracy as involving "the improper permissiveness that drifts toward anarchy under the pluralistic 'government' of an atomized society in which everybody can behave and misbehave according to the principle, *tel est mon plaisir*" (p. 54). Because of his fear of permissiveness or anarchy, Dietze is willing to tolerate much to preserve constitutionalism. Thus, while "To be defensible, actions in the name of the law must not endanger, but serve, constitutionalism," Dietze immediately defends the "dictatorial power of the Reich President" (p. 77), and justifies some authoritarian acts as necessary to protect constitutionalism.

Dietze is unabashedly a traditional political scientist, who defines our discipline as one which "emphasizes the restriction of power for the sake of justice," and thus is "sympathetic to the Law State" (p. 48), as well as "interested . . . mainly . . . in the freedom of the individual from the power of the state" (p. 56). However, neither this wishful and hopelessly outdated view of the discipline nor the further erroneous assertion that political science, "having become value-free" and having succumbed to formalism "will probably be as unable as politics to withstand the democratic tide" (p. 49) interfere with Dietze's excellent analysis of political ideas and the development of constitutionalism which is well worth reading because of its superior quality.

STEPHEN L. WASBY
Southern Illinois University at Carbondale

Administration: The Word and the Science. By Andrew Dunsire. (New York: Halsted Press, 1973. Pp. x, 262. $8.75. First published in 1973 by Martin Robertson and Co. Ltd., London.)

Dunsire—who is Senior Lecturer at the University of York in England—is faithful to his book's title. That title promises to dig deep into "administration" as word; and it implies a broad

survey of administration as science. Nearly one-fourth of the text is devoted to "administration" as word; and Dunsire also seeks to capture the essence of administrative science and practice in Europe and America, roughly from the 16th century onward.

Given the range of professed coverage and goals, diverse and perhaps kaleidoscopic reactions to the book may be expected. For example, some readers may come away from the book feeling that they have more knowledge about the "word" than they had any prior need for, or can ever hope to put to any future use. Some readers also may conclude that the volume's sweep is too broad and brief to be satisfying to those knowledgeable about the "science" as it developed in the several countries and eras surveyed by Dunsire.

My overall reaction is that the promise in the title is not only basically kept, but is kept well. Despite the awesome dimensions of the literature and experience he addresses, Dunsire deals with them effectively and persuasively, on balance.

To provide detail about the volume, this review will distinguish three clusters of chapters. Dunsire nowhere indicates he would be comfortable with such an arrangement, but the convention here is convenient.

In a first cluster of three chapters based largely on English experience, fourteen meanings of "administration" are distinguished and elaborated through p. 52, and a fifteenth is added on p. 199. This could prove heavy going, but Dunsire eases the burden by his comprehensive grasp of authors and topics. Thus I was variously rewarded for trying to keep the meanings straight. Illustratively, Dunsire highlights Jeremy Bentham's 1816 warning about falling into a politics/administration dichotomy, which nourishes those who know we could learn far more from the past if we paid appropriate attention. And Dunsire provides (esp. pp. 26–33) a review of important historical background for the generalist/specialist debate that is always a central issue, and now perhaps more than ever. Only occasionally does the associational richness seem to detract from or exist independently of the argument, as it did for this reader in Dunsire's care to note (pp. 77) that Lorenz von Stein—"the greatest name in nineteenth-century German Administrative studies"—had "no connection with the Freiher, Carl Vom Stein."

With the "word" in hand, Dunsire devotes a second cluster of five chapters to tracing and analyzing the development of the "administrative sciences" in various countries and eras. The range of coverage of these chapters can be suggested economically:

- the developing administrative sciences in England are contrasted with those in France or Germany, and their substantial differences are traced (p. 53) to the differential impact of "the centralising 'absolute monarchy' phase of European political development";
- the emphasis on bureaucracy, especially in France and Germany, is analyzed and related to the less absolutist nature of those states in the 19th century;
- the "American science of administration" is sketched, from Woodrow Wilson through the Simon and Dahl of the late 1940s;
- the "new administrative science" which developed in the third quarter of the 20th century, especially in America and England, is reviewed;
- the "rise of comparative administrative" is highlighted as a development of the 1950s which Dunsire traces to "the outgoing generosity of American aid and technical assistance programmes" (p. 134).

This second cluster of chapters represents summaries-cum-syntheses and, although one can quibble about what they do not intend to do, they are typically very good at what they intend. But let me risk a quibble or two. I was disappointed by several such self-imposed tethers: "It is not within the practicable scope of the present book to present more than an outline treatment of . . ." (p. 102). The patent competence and comprehensiveness of that "outline treatment," in short, whetted my appetite for more. Similarly, I found that the chapter on the "new administrative sciences" omitted much recent behavioral and rational/technical activity that properly could have been mentioned, given the author's own definition of scope. For example, Dunsire refers to early humanistic approaches to organization analysis, like Maslow's. But he does not deal with the decade or so of subsequent research and experience with the "action research" or "organization development" to which those early approaches led, research and experience that have raised the expectations and also the blood pressures of many students and administrators.

Four essays about central differences in degree that are often seen as polarities constitute the final cluster of chapters. Dunsire directs attention, in turn, to administrative science and popular understanding, to public and "private" administration, to centralization and decentralization, and to theory and practice.

These chapters did not well sustain this reader's interest, for at least two reasons. First, much of the material is tied into the problems and prospects of public administration as a field of inquiry in England, with which I could not sustain a continuing identification, perhaps because its nuances escaped me. Second, the issues raised in those chapters are more intractable and elusive than

the narrative sometimes allows. Consider (p. 201): "There is undoubtedly a gap between practitioner and theorist; but it is not expressible by a 'theory/practice dichotomy,' it is a mere communication gap."

ROBERT T. GOLEMBIEWSKI
University of Georgia

Antonio Gramsci: Life of a Revolutionary. By Giuseppe Fiori. (New York: Schocken Books, 1970, 1973. Pp. 304. $3.45, paper.)

"The only ambition of this book," its author comments in the Foreword, "is to complete the portrait of Gramsci" by drawing the "body and legs" in addition to the well-known "head" of a great intellectual and political leader. The result is an account of Gramsci which gives only minimal attention to the content of his political ideas. One can no more separate the head from the heart than the reverse, however, and this reviewer is left wondering how complete a biography this is of the remarkable man who was one of the founders of the Italian Communist Party and who in his philosophically erudite essays, written in prison under the most trying circumstances, produced the most telling criticism of vulgar and mechanistic Marxism yet to appear within the ranks of Marxism itself.

Antonio Gramsci was born in Sardinia in 1891 and died in 1937 shortly after having been released by the Fascists from a ten-year imprisonment. Because he was always in frail health, it is extraordinary that Gramsci could have survived ten years of incarceration as a political prisoner; this very confinement, however, served as the occasion for his most penetrating writing on such subjects as "theory and practice" in Marxist epistemology, the political and social condition of the Italian South, the role of Benedetto Croce in the Italian intellectual tradition, and a variety of other themes from literary criticism to the role of Henry Ford in American economic history.

Despite its limitations, Fiori's biography has many virtues, and one is grateful that an English translation is now available in a paperback edition. The author, a journalist, historian, and novelist, like Gramsci, is of Sardinian upbringing. Antonio Gramsci was highly conscious of having come from the "underdeveloped" Italian south. Fiori is peculiarly able to empathize with his subject; although he obviously admired Gramsci, he does not romanticize him. This is biography, not hagiography, and the book is written with restraint and taste and a scrupulous regard for the sources, and is never wordy or pretentious.

Still, at the end one is left with many questions unanswered (or perhaps even unasked). For example, why did Gramsci, who delighted so much in humanistic studies and who in his writings displayed a truly exceptional capacity for scholarly detachment, forsake the world of the academy for the political struggle and, having done so, willingly assume the role of a Communist party leader? Fiori provides us with some clues in references to Gramsci's early pamphlets (p. 101 ff.), but one wishes that the author had delved more deeply into the influence of the Italian revolutionary's university years upon his subsequent career. How could Gramsci, who, from the beginning of his association with Marxism and the Marxists, had always been the protagonist of "openness" and "dialogue" and had constantly opposed a coercive social order, bear to remain in the closed world of international communism particularly after the ascendancy of Stalin? In many respects he seems as out of place among Communist writers as do the intellectual dissenters in the Soviet Union today.

Fiori's biography, then, is more comparable to an artist's sketch than to a finished portrait. If, however, one reads it in conjunction with Gramsci's own *Prison Notebooks* and with J. M. Cammett's study of Gramsci's role in the formation of the Italian Communist party, one can readily see why Antonio Gramsci deserves to be regarded as one of the exceptional political minds of our century.

In 1947, Benedetto Croce, the Italian philosopher whom Gramsci regarded as at once an inspiration and his worthy opponent, had occasion to review the first two volumes of Gramsci's posthumously published prison writings. Croce praised his fellow southerner for his "openness toward truth regardless of its source," "scrupulous precision," "equanimity," "kindness," warmth of feeling and lucidity of style (Croce, in *Quaderni della Critica* [July 8, 1947], p. 86–88). The arduous life of Antonio Gramsci demonstrates once more that the human spirit lives on long after dictatorships seeking to imprison and destroy it have vanished.

DANTE GERMINO
University of Virginia

Political Obligation. By Richard E. Flathman. (New York: Atheneum, 1972. Pp. 334. $12.95.)

Professor Flathman's book breaks new and fruitful ground in the study of political obligation, a concept which not so long ago was on one side of the Atlantic virtually discarded as a linguistic anachronism, and on the other treated simply as a behavioralist phenomenon. Professor Flathman himself draws on both of these recent traditions in political thinking. They provide him with tools of analysis and methods of approach which, after re-examination and reshaping, can be used to gain a more positive and penetrating insight into the concept of political obligation than these two traditions offered.

The central theme of Professor Flathman's

book is that "the practice of political obligation operates only if men *choose* or *decide* to do or refrain from doing certain *actions* because they believe there are *good reasons* for accepting and obeying or rejecting and disobeying rules that require or forbid these actions" (p. xv, author's italics). Those who disobey the law through habit, training, fear of punishment or expectation of reward are not engaged in the practice of political obligation, since it is the nature of obligation that it has to be assumed by free choice after rational reflection.

Flathman argues that the notion of obligation also requires disinclination on the part of those who assume it to act in the way required. In support of this claim he asserts that there would be no need for a society to have any rule unless there were evidence of "substantial and continuing disinclination to act in the manner prescribed by the rule" (p. 163). Two objections may be raised to this. First, there almost certainly are (and logically and conceptually there certainly could be) rules which no members of a community feel any disinclination to disobey, but which are stated and understood as regulations that the authorities would seek to enforce in the event of noncompliance. Such rules may indeed be laid down not because they are needed to secure compliance on the part of persons disinclined to conform to a required form of behavior, but because that form of behavior itself is held to be so vital to the well-being of the community that it needs to be given the status of a law. Second, Flathman's assertion that no rule would be made by a society in the absence of "substantial and continuing disinclination to act in the manner prescribed by the rule," cannot substantiate the thesis that no *individual* member of that society can be held by another member to have an obligation to act in accordance with the rule in the absence of evidence of disinclination so to act (p. 156). The weakness of Flathman's position can be seen in the way in which he handles the position of adolescents and persistent criminals. He recognizes both the *need* not to leave such persons without any obligation to obey societies' rules, and the *fact* that they cannot be said to be disinclined to obey rules they have never accepted; but instead of attempting to reconcile these two factors through a re-examination of the disinclination generalization, he simply asserts that adolescents and persistent criminals are "exceptions" whom it is "appropriate" to treat as having obligations to obey.

One of the points which Professor Flathman is concerned to stress is that political obligation as a practice cannot be established without reference to the content of particular regulations. He demonstrates that it is not possible to infer from the arguments in support of an obligation to accept the general rule "obey the law," an obligation to comply with any specific law. He further argues that reliance on the general rule to secure compliance with trivial legal rules is liable to weaken the force of the general rule as a conduct-guiding concept. While he accepts the force of Hanna Pitkin's thesis that it is part of the meaning of law that those to whom it is applicable are obligated to obey it, he denies that it is necessarily meaningless to discuss the question "Why do I (ever) have an obligation to obey the law?" This question is not a semantic puzzle which only permits of linguistic unscrambling. As Flathman shows it is logically and conceptually possible to question the fundamental rules and conventions of any or every known society. Further, he points out that a purely semantic interpretative answer to the question "Why do I (ever) have an obligation to obey the law?", can never in itself provide a *reason* for obedience. That is not to say that reasons cannot be adduced in support of the general rule, "obey the law"; reasons which Flathman considers at length in a sympathetic and fruitful analysis of Socrates' *Crito*.

Professor Flathman is much more critical of consent theories of political obligation. He insists that it is not possible, without undermining the very notion of consent, to hold a person to have given his consent, whether express or tacit, to any action or arrangement unless he knows, intends and communicates what he does. These necessary conditions of consent lead to the embarrassing result that few persons in any society can be shown to have ever given their consent, and that in consequence consent theories of obligation render virtually all governments illegitimate. This conclusion seems unwarranted, at least with respect to those societies which permit their adult members to choose their governments from among a range of contenders, including those who stand for change in societies' basic arrangements and principles. It is far from clear that fewer persons could be shown to have a political obligation by consent to the laws of the United States, for example, than the number having an obligation under the requirements laid down by Professor Flathman.

LESLIE J. MACFARLANE
St. John's College, Oxford University

Jean Bodin and the Rise of Absolutist Theory. By Julian H. Franklin. (London and New York: Cambridge University Press, 1973. Pp. 113. $8.95.)

Julian H. Franklin has established himself as one of the foremost authorities on the complex and notoriously obscure thought of Jean Bodin. In this slim but weighty study, Professor Franklin's great intellectual powers and prodigious scholarship are brought to bear in a careful and detailed analysis of the development and sub-

stance of Bodin's doctrine of "*pulssance souveraine.*"

Most textbook discussions of Bodin's absolutism tend to assume that his notion of sovereignty was continuous between the *Methodus*, published in 1566, and the *République* of 1583. This view obscures the fact that in the latter work Bodin had abandoned the theory of sovereignty presented in the *Methodus*. The notion of limited sovereignty developed in the *Methodus* largely drew from and was supported by certain persistent medieval ideas which, in the climate of the Renaissance, were generally favorable to the development of constitutionalism. Franklin contends that a coherent absolutist doctrine did not predominate in the sixteenth century, but appeared rather late with Bodin's formulation.

This is not to suggest, however, that the idea of absolute authority did not exist prior to the publication of the *République*. It was in fact a universally accepted idea in French law. Yet it was situated in the context of the notion of the commonwealth as a *corpus mysticum*. Franklin argues that the often immoderate rhetoric of some of the commentators was in fact a means of expressing the cohesion of the commonwealth through a glorification of its head. Nevertheless, the theme of their discussions of, for example, the sovereign courts and the Parliament of Paris, was the limitation of the king's power; it was a theme embedded in the French political tradition. The commentators wrote against the background of a centralized administration and the medieval principle by which the king must govern with consent. Thus, despite absolutist imagery and language, the commentators of the early sixteenth century were delicately poised between monarchist and constitutionalist ideas. Though such opinions came to be more prudently and more timidly expressed at the height of royal power, around the middle of the sixteenth century, they did persist and were reasserted in the 1570s. Only against this background, Franklin suggests, can one understand Bodin's early theory of sovereignty and appreciate the significance of his shift to absolutism.

The theory of sovereignty developed in chapter VI of the *Methodus* represents a "mature and reasoned conception of the state" in which "absolutist elements are not only absent but are deliberately repudiated" (p. 23). Here Bodin does not depart significantly from the French political tradition according to which the monarch was undoubtedly conceived as supreme, but within the law. He felt that his later discussion did not differ from the constitutionalist perspective maintained in the *Methodus*, but was merely an elaboration of that position. Franklin shows that the turn to absolutism "was not a direct natural outgrowth" of the theory contained in the earlier work; it was a "sudden and drastic shift." And in a period in which political and religious positions were inextricably entangled, Bodin's concern and focus were distinctively political.

The point on which his later doctrine turned was the question of resistance. It was a direct response to the tumult surrounding the Huguenot party and its allies and the subsequent revolutionary movement occasioned by the massacre of 1572. To the emergence of a doctrine of resistance developed by Huguenot theorists such as Beza, Bodin responded by claiming that a "monarchy . . . is either absolute and immune from resistance pursuant to law, or it is not a monarchy at all" (p. 93). This was, Franklin argues, the core of his notion of absolute authority. It was "not that the ruler must always be obeyed, no matter what he ordered, but only that he could not be legitimately resisted" (p. 96). This meant that those "limitations" upon absolute authority which Bodin acknowledged in the *République*, the law of nature and *leges imperii*, were not enforceable by the community. Even if the king habitually and blatantly transgressed the law of nature, "his subjects were legally and morally enjoined from forceable resistance" (p. 93). In short, all institutional restraints which had been included in the first view and supported by the larger tradition were now without binding status. If the older tradition of discourse had favored a constitutional monarchy, Bodin's formulation clearly opened a "legal path . . . to autocracy."

In a time of intense political debate, Bodin's doctrine was only one of the competing theories of sovereignty. Those offered by the Huguenots and the later Catholic League were more empirically satisfactory; but it was Bodin's view which prevailed. The "sheer intensity of the ideological conflict in this period doomed the older tradition as a means of settling constitutional disputes" (p. 104). Bodin's doctrine was a response to this crisis and became widely influential (e.g., in Germany and England) as the general trend toward absolutism became dominant.

Franklin's latest contribution to the growing body of literature on Bodin is a work of impressive scholarship and reflects a sense of intellectual craftsmanship which seems to be rare these days. His sensitivity to the subtleties of Bodin's thought and the skillful use of materials provide us with a fresh understanding of one of the major figures in the current of culture and politics of the early modern period in France.

RANDAL H. IHARA
Transylvania University

Dialectic of Enlightenment. By Max Horkheimer & Theodor W. Adorno. Translated by John Cumming. (New York: Herder & Herder, 1972. Pp. xvii, 256. $9.50.)

This work, first published in 1947, appears in

translation at a moment when the "critical theory" of the Frankfurt Institute for Social Research is beginning to interest some American social scientists. Some of the fruits of the Frankfurt school are already well-known here, especially *The Authoritarian Personality* and the writings of Herbert Marcuse. But Horkheimer and Adorno, the founders and main expositors of critical theory, have written principally in German, and it is only in the past few years that their chief writings have been translated. Political scientists who desire an introduction to their thought will probably find *The Positivism Dispute in German Sociology* (Adorno et al., Heineman, 1973) best suited to their purposes.

The present work is ill-suited as an introduction because it is not a book but "fragments" (p. xi), and opaque ones at that. The authors' theme is the disintegration of reason or the dissolution of enlightenment thought and practice in the present century; its task is to trace the causes and to find a way out of the impasse. But since the authors write as heirs of enlightenment rationalism (pp. xiii–xiv, 208), they are both patients and physicians. The pathology of their predicament is this. They accept the Hegelian premise that theory or knowledge is but consciousness of the praxis of the society in which it occurs; all thought is thus essentially historical and milieu-dependent. On the other hand it need not be acquiescent, for by discovering the contradictions within society, it achieves a critical distance by negating, in thought, the status quo. Such is critical theory, or as Adorno also calls it, "negative dialectic."

For the authors, enlightenment is not the propaganda of an educated elite but the movement of consciousness in history. Enlightenment begins as the effort to overcome myth, animism, and resignation to divine fatality. It is the discovery of human freedom, including the wrestle with nature for mastery. From this germ modern enlightenment matured into the scientific and industrial domination of nature for the relief of man's estate; and yet both practically and theoretically enlightened men are enslaved by the instruments of liberation. Practically they are in servitude to bourgeois economy, as the authors attempt to show in their chapter on "The Culture Industry: Enlightenment as Mass Deception." They are dominated in thought by the hegemony of the scientific instrumentalist conceptuality. Since the latter is by design objective, unreflective, and domineering, enlightenment thought at high tide "has extinguished any trace of its own self-consciousness" (p. 4). Enlightenment, specifically positivism, thus falls prey to myth—the "myth of facts": "Under the title of brute facts, the social injustice from which they proceed is now as assuredly sacred a preserve as the medicine man was sacrosanct by reason of the protection of his gods . . . animism spiritualized the object, whereas industrialism objectifies the spirit of man" (p. 28).

The book's thesis that enlightenment at the stage of advanced capitalism returns to myth seems to have been suggested by experience of the Third Reich. The authors' attempt to reveal the return to mythic-magical modes in modern society, and especially to show the tendency of bourgeois society toward fascism, is not so much argued as elicited by an organized flight of ideas and psychoanalytic associations. Their tendentious interpretation of Homer's Odysseus as the prototype of bourgeois man, and of de Sade as a spokesman for developed bourgeois consciousness, may be cited as yet more proofs of the capriciousness of depth psychology. If hunting for hidden motives were legitimate, it could be said that the authors use depth psychology as a pseudorational ritual to exorcize unpleasant realities with which they cannot otherwise deal. But this sort of thing leads to mutual abuse and to the self-destruction of reason of which the authors complain. In this respect they may be right when they imply that their own medication is part of the disease (p. xiv).

If liberal democracy deserves the harsh judgments levied here, it is important to know the authors' assessment of the results of science and domination in socialist nations. This question is not addressed at all. From other writings we learn that they do not endorse Soviet socialism. Their revolutionary language therefore refers to no political reality. The final irony for these men, who more than any other academics inspired the revival of the German Left, was that in the closing days of their career they became the objects of leftist fury.

These remarks do not imply that the Frankfurt school does not deserve careful study. American social and political thought has been so heavily colonized by German thought—by logical positivism, existentialism, Marxism, depth psychology, and sociology—that it is perhaps necessary now to examine the sources.

The translator has not always succeeded in putting the complex sentence structure into good English, and he has suppressed some unusual expressions and Heideggerian mannerisms of the authors. Otherwise the translation appears to be reliable.

HIRAM CATON
The Australian National University, Canberra

The Development of the Marxian Dialectic. By Dick Howard. (Carbondale and Edwardsville: Southern Illinois University Press, 1972. Pp. xiii, 205. $7.95.)

Most doctoral dissertations have certain unavoidable characteristics, determined by their

purpose, that make them instructive to their respective authors rather than to their readers. *The Development of the Marxian Dialectic*, originally written as a doctoral dissertation, is an instance in point.

The main objective of the book appears to be an analysis of Marx's earliest writings from *The Difference between the Democritean and Epicurean Philosophy of Nature* to some sections of the *Economic and Philosophical Manuscripts of 1844* in order to establish the "groundwork of Marx's future theory" and "the unity . . . [of] Marx's life work" (p. 134). Although this groundwork is not specified, the title of the book suggests that dialectical philosophy is supposed to provide the foundation of Marx's thought. This is little help, however, since the author does not explicate the concept, nor does he define the term "dialectic" anywhere in the book.

The Development of the Marxian Dialectic falls into one of the familiar patterns of Marxian scholarship. It first summarizes the content of Marx's earliest writings, and then tries to show how Marx's own views evolved from the philosophy of Hegel and his followers. Scholars, regardless of whether they do or do not adopt a Hegelizing interpretation, tend to agree that Marx made his first discoveries by the process of encounter with Hegelian philosophy, by criticizing its internal consistency and by contrasting its claims with the state of affairs in Germany and France. Thus, Marx's philosophical, social, and political thought is shown to emerge from an attempt to reformulate or resolve some of the problems which had been stated and discussed in the 1840s. Apart from some happy formulations of uncontroversial matters, *The Development of the Marxian Dialectic* does not seem to contribute much that is particularly original to the existing literature written along these lines. The author, however, is sometimes carried away by Marxian rhetoric; one example is the significance he attaches over and over again to Marx's pronouncement about making philosophy worldly and the world philosophical, which allegedly cancels the opposition between idealism and materialism. The author's language occasionally lacks clarity; he fudges important issues and indulges in the oracular, when he could and should be lucid and explicit. Consequently, some texts of Hegel and Marx are more difficult to understand in his formulation than in the original. For instance, what does it mean to say that according to Marx "when philosophy enters the world it loses its inner unity and must recover it dialectically in interaction with the world" (p. 52)? Feuerbach criticized Hegel for inverting subject and predicate and wished to assert by this metaphor that Hegel confused what is ontologically primary (e.g., a man or a citizen) with what is derived (e.g., consciousness) and secondary (e.g., the state). The author often uses Feuerbach's metaphor literally, writing that Hegel makes the constitution into the subject and treats the legislature as its predicate (p. 68), and so forth. His final analysis fails to bring out Marx's explicit rejection of the Hegelian opposition between a natural reality and a supernatural world of abstract ideas, between the real and the ideal, where "ideal" means an existence independent of the world of our senses. His comment on Hegel's *Science of Logic* (p. 188, note 16) seems to reproduce Lenin's error and to confuse the opposition of idealism and realism with that of idealism and materialism. Hegel's dictum, "Every philosophy is an idealism," applies to the former and not to the latter.

The preceding observations should make it clear that the title of the book does not reflect its content. The term "dialectic," as a noun or an adjective, does not serve any useful purpose unless its various senses are carefully distinguished and its proposed uses clearly defined. "Dialectic" can be and has been differently understood, both in Hegel and Marx. There are those who say that there is a dialectic method or logic (without explaining what they precisely mean by it) and there are those who maintain that there is none that one could adopt even if one wanted to. For others, the dialectic is at most a method of exposition (with its notorious triad of thesis, antithesis, and synthesis), a transformation of concepts from each other, a way of looking at the world that emphasizes change and development through conflict, a tension between opposite tendencies resulting in their supersession, the appearance of unintended consequences, or a regular succession of action and its reversal. There is no way of discovering which of these or other senses the author has in mind when the term "dialectic" occurs in the title or in numerous places in his book.

The author has stated in the Introduction that he relied on primary sources for the most part and referred to secondary literature only in exceptional cases (p. viii). This is not entirely true. His observations on Moses Hess and Arnold Ruge are based on quotations borrowed from Georg Lukács's article on Hess (the writings of Hess are easily available in the edition of A. Cornu and W. Moenke of 1961), and his knowledge of Ruge appears to be derived from the writings of K. Hartmann and A. Cornu. His presentation of Feuerbach's philosophy is embarrassingly inadequate (pp. 49–51), and there is little evidence that he knows Bruno Bauer or Max Stirner first hand. It is false to call Adam Ferguson the teacher of Adam Smith or to say that Hegel took the term "civil society" from the latter (p. 179). *Buergerliche Gesellschaft* ("civil" or "bourgeois society") is derived from *Buergerstand*, the dominant estate of 19th century Germany. Ferguson was

unquestionally first to use the term, and *The Wealth of Nations* had a lasting impact on Hegel, but Hegel's use of the concept of *buergerliche Gesellschaft* is his own and irreducible to the ideas of his British predecessors.

Z. A. JORDAN

Carleton University, Canada

Technology Assessment: Understanding the Social Consequences of Technological Applications. Edited by Raphael G. Kasper. (New York: Praeger Publishers, 1972. Pp. 306. $16.50.)

This volume consists of papers given at a series of seminars conducted by the Program of Policy Studies in Science and Technology at The George Washington University from January 1969 to May 1970, along with a summary of the discussions which followed. While interest in assessment of technology has grown since then, this collection remains a valuable introduction to the literature and problems in this area.

As many of the seminar participants recognize, technology assessment is nothing new. From ancient times human beings and their political agents have been concerned with the perceived or anticipated consequences of technological innovation, and both legal institutions and administrative agencies have passed judgment on technologies and their consequences. What is new about our current concern with technology assessment—as exemplified in the creation of bodies such as the Environmental Protection Agency and the Office of Technology Assessment—is the drive to make such assessment a central function of government, to be carried out in accordance with consciously determined basic social norms. Technology assessment inevitably involves both the philosophical judgments embodied in such norms and the creation of institutional frameworks to operationalize and provide sanctions for agreed-upon standards. Problems are involved both in reaching consensus about the philosophical standards to be embodied in technology assessment, and in deciding what kind of political and administrative arrangements can best serve to effectuate these standards. But above all, as this collection illustrates, it is difficult to synthesize the discussion of broad objectives with the discussion of particular means to achieve them. The literature in this area thus tends to deal either with one aspect or with the other, but rarely relates the two.

This tendency is clearly illustrated in this volume. The contributions of Clarence H. Danhof on "Assessment Information Systems," Franklin P. Huddle on "The Social Function of Technology Assessment," and Leon Green on "Technology Assessment or Technology Harassment" focus essentially on the broad problems of goals and values. Other articles covering "Technology Assessment and the Congress" (Richard A. Carpenter), "The Adversary Process in Technology Assessment" (Harold P. Green), "The Management of Technology Assessment" (Louis H. Mayo), "Technology Assessment and the Food and Drug Administration" (Alan H. Kaplan and Robert H. Becker), "Technology Assessment in the Executive Office of the President" (Charles W. Kidd), "The National Transportation Safety Board" (Ernest Weiss), and "Technology Assessment and Citizen Action" (Ellis H. Mottur), deal almost exclusively with detailed means toward particular limited ends, some describing existing systems of assessment, others speculating about what might be done. What is missing from most of these discussions is any clear focus on the politics of technology assessment in the broadest sense, the question of how a putatively pluralist society, with a political system based on conflict among competing interest groups, can or should arrive at the kinds of value consensus which would necessarily have to underlie any coherent national program of technology assessment. Some participants assume there is such a thing as the "public interest" which can provide a standard for technology assessment; others deny this either implicity or explicitly. If serving the public interest is an intellectually legitimate goal, then the various technical questions about analytical and institutional means for precisely defining and achieving it can be given meaningful answers; if not, discussing them is essentially playing games or describing the game-playing of others.

Symposia such as this can hardly be faulted for failing to solve a problem which seems to continue, necessarily or otherwise, to baffle most political philosophers. Judged on the basis of what this work sets out to do and can reasonably be expected to do—to provide an introduction to the increasingly extensive and diffuse literature on technology assessment—this is an extremely interesting, useful, and thoroughly commendable volume. Ideally, it will spur others to readdress the philosophical problems upon the resolution of which the ultimate relevance of works such as the present volume depends.

VICTOR FERKISS

Georgetown University

Sayyid Jamal ad-Din "al-Afghani": A Political Bibliography. By Nikki R. Keddie. (Berkeley: University of California Press, 1972. Pp. xvii, 479. $20.00.)

The seemingly indefatigable Nikki Keddie has done it again. "It," in this case is the production of a definitive study of the immensely influential Sayyid Jamal ad-Din "Al-Afghani" (1838–1897). The author has laboriously examined all the available material on the Sayyid—only the opening of the Russian archives or the discovery of new ma-

terials in the Ottoman archives, where only one document on Jamal ad-Din has been found to date, may provide us with a different view of the man or his work.

Here, Professor Keddie gives us rather rudimentary details of the man. She establishes conclusively that he was born not in Afghanistan, hence the misnomer "al-Afghani," but in Asadabad, a village in western Iran. He received a traditional Iranian Islamic education in Qazvin and Tehran and in the Shi'a shrine cities in Iraq, primarily Najaf. At the age of 18 or 19 he began a life of travel which saw him remain in any one place for only a few years—to India, Mecca, Iraq, Tehran, Afghanistan, India, Egypt, Istanbul, Egypt, India, London, Paris, Iran, Russia, Germany, Iran, London, Istanbul.

In all these places, Professor Keddie tells us, he attracted disciples through the power of his written and spoken words as well as the force of his personality. He lived modestly from the contributions of rulers, nobles, and commoners, devoting himself to developing his ideas on the problems of the Islamic world. He curried and received the favor of numerous rulers, although falling from grace with regularity. (He was expelled from Afghanistan, Istanbul, Egypt, and twice from Iran.) As he aged and found himself unable to play a direct role in political life, his writings became more shrill. He began to portray himself as "a messianic savior of the Muslim world" (p. 211). His life ended in Istanbul where the Sultan, having learned the lesson, did not expel the Sayyid but kept him under virtual house arrest. He died painfully from cancer of the jaw, which court physicians attributed to excessive cigarette smoking, his last years having been characterized by "increasing obscurity, restlessness, and some bitterness" (p. 421).

His life seems unfulfilled; even his words raise problems. As Professor Keddie suggests, he was a master of "precautionary dissimulation" (p. 79), making the content and style of his message appropriate to his audience and his then immediate goals. Moreover, for all his travels in Europe, Afghani consistently deprecated the economic, political, and technological bases of the power of the West. He attributed the weakness of the Islamic peoples to the selfishness of individual Muslim rulers and the corruption of Islam by the *ulama*. Finally, only in the Khalifat movement in India, years after his death, did his ideas constitute the basis of a serious political program.

What then justifies the very widespread interest in Jamal ad-Din's thought and, thus, this book? Basically that his thought has been the inspiration for many significant political and religious movements in this century. He has become a virtual "mythic hero" in much of the Middle East (p. 422), where his influence has proved to be significant and lasting. His writings anticipate many of the major themes of contemporary Middle Eastern political concerns.

Professor Keddie shows how Afghani gave priority to the liberation of Islamic lands from European encroachments. He called for an intellectual revival and political and social activism. He supported local and Islamic nationalisms. He sought religious reform as the cultural basis of a unified and strengthened nation. He appreciated the role of public opinion and was instrumental in the establishment of many important political journals and newspapers, especially in Egypt. Afghani laid the blame for the ills of the Middle East to the decadence of rulers and clergymen whose corruption then spread to the people. He was a crucial figure in the arousal of the Muslim peoples, a process which continues at least partly through the power of his words. "The only thing that will help us emerge from our present difficulties," he told his followers, "is zeal. Zeal is possessed by only some who know that their honor is only in their race, their power is only in their community, and their glory is only in their fatherland" (p. 109).

Nikki Keddie discusses the sources and development of Afghani's thought; illuminating both its nuances and major alterations over time and place. She writes of his major disciples—Muhammad Abduh, Rashid Rida, Sa'd Zaghlul, and Mirza Riza Kirmani (the assassin of Nasir ad Din Shah) among them. And throughout, the author gracefully acknowledges her debt to a number of other studies of al-Afghani, especially those by Homa Pakdaman, Elie Kedourie, A. Albert Kudsi-Zadeh, and Niyazi Berkes.

Not that the book is without flaws. The level of detail is, perhaps, too great. The explication of his thought in each of the cities in which he resided seems occasionally to detract from our understanding of al-Afghani. To have organized the book by conceptual areas rather than chronologically may have made the Sayyid's thought easier to grasp. (But it may also have imparted a consistency and unity to that thought which was absent.) But as it is, Keddie's work will long remain the basic reference on that now considerably less enigmatic figure, Sayyid Jamal ad-Din.

MARVIN ZONIS
University of Chicago

Participatory Democracy: Developing Ideals of the Political Left. By Daniel C. Kramer. (Cambridge, Mass.: Schenkman Publishing Co., 1972. Pp. 249. $8.95.)

The idea of participatory democracy is gradually being brought in from the academic wilderness and given serious attention. Kramer's contribution to the reappraisal is an attempt to decide

whether participatory democracy—or decentralized socialism or anti-bureaucratic socialism, which he treats as synonymous—would be worth trying to implement in practice. His conclusions are pessimistic—that it is unlikely that a majority of citizens would be self-governing in a participatory democracy, and that community is unlikely to be realized. Therefore, he argues, participatory democracy does not offer a satisfactory alternative to welfare state capitalism.

The general approach of *Participatory Democracy*, and Kramer's willingness to draw substantive conclusions are to be welcomed. Political theorists have tried to force apart ideals and reality for far too long. But the discussion suffers from Kramer's peculiar conception of citizen self-government, and his conclusions are thus far less definitive than he supposes.

The basic question asked in *Participatory Democracy* is how far a system of participatory democracy would in fact realize its major ideals and goals. Attention is concentrated on one ideal, that of the politically self-governing citizen, and the chapter devoted to exploring the meaning of this ideal is central to the whole argument. It takes as its starting point the assertion that direct democracy is an unrealistic notion, so that discussion of self-governing citizenship must be framed in terms of representative democracy alone. According to Kramer, a citizen is self-governing when "his or her views on one or more issues that can reasonably be called 'political' are among the reasons why at least one member or employee of the governments with jurisdiction over him votes or decides in the way specified by those views" (p. 39).

The concentration on "political" self-government is, Kramer says, for "reasons of style alone" (p. 41); it does not imply that other potential areas of self-government are unimportant. But even if these other areas are included—and some political theorists would wish to question whether they are non-political—this definition would remain a most inadequate one. Most obviously, it makes no reference to that central feature of "democracy" for most theorists, the free election of her representatives by the citizen, and makes the notion of citizen political self-government so weak as to be virtually meaningless. What it amounts to, as Kramer's examples illustrate, is a version of what Verba and Nie in *Participation in America* call "particularized contacting," i.e., individual approaches to public officials on matters of individual interest.

Although Kramer has set out to see if the ideals of participatory democracy can be realized, he has misunderstood the nature of the central ideal. This is largely because he has tried to assess the theory as if it were an extension of the individualistic, orthodox liberal democratic theory. He has not considered participatory democracy in terms of its own theoretical perspective. Thus, when possible objections to his definition of citizen self-government are discussed, Kramer counters the objection that the definition makes it difficult to decide how long an individual is self-governing by replying that it can only be "for a reasonable time" (p. 57). But advocates of participatory democracy do not see citizen self-government as something like the Cheshire cat. The political theory of participatory democracy does not start from individual, private interests, but is dialectical and communitarian, emphasizing the interrelations between everyday life and the political sphere—as Kramer's use of "decentralized socialism" interchangeably with "participatory democracy" might have suggested. The ideal is not that some *individuals* are sometimes, temporarily, self-governing, but that all *citizens* are collectively self-governing.

To be sure, this ideal requires the kind of evaluation of *Participatory Democracy*, but differences in theoretical perspective are crucial for the interpretation of relevant empirical evidence. Again, Kramer has missed the point that a participatory definition of citizen self-government is backed by the (empirical) claim that although representatives—democratically elected and strictly accountable representatives—are necessary in complex, large-scale societies, this does not preclude direct democracy also in many areas. Thus in his discussion of Yugoslavia, which in any case is far too cursory, Kramer makes no mention of the "economic units" in the enterprise which open up the opportunity for the ordinary worker directly to participate in everyday decisions at shop-floor level. He refers only to the central workers' council of the enterprise, i.e., to the body that is, in a different sphere, analogous to our existing, familiar elected bodies. Nor is the evidence considered which indicates that widespread lack of interest in matters outside the personal sphere might not be as "natural" as Kramer assumes, but related to the nonparticipatory nature of existing institutional arrangements. And to his charge that neighborhood and other councils would become preoccupied with "little concerns," the advocate of participation might well reply, "Little to whom"?

This is not to imply that all Kramer's criticisms fail to reach their targets. Theorists of participatory democracy have tended to neglect important questions such as the relation of participatory bodies to each other, the possible fostering of parochialism, and the precise relationship between different levels of participation. But in this last area, convincing solutions have hardly been provided by those, who like Kramer in his concluding paragraphs, argue that a better alternative to the present system is liberal democratic but "moder-

ately centralized" socialism. A deeper and theoretically more sophisticated consideration is needed of the empirical and theoretical critique of centralized, representative institutions by the participatory democrats before that solution can be accepted.

As in so many books today, there are some irritating misprints.

CAROLE PATEMAN
University of Sydney, Australia

Toothing Stones: Rethinking the Political. Edited by Robert E. Meagher. (Chicago: The Swallow Press, 1972. Pp. xvii, 289. $7.95.)

This book contains fourteen responses to a six-paragraph invitation sent out by the editor. This invitation, reproduced on pp. xi–xv, asks the contributors to address themselves to the "decay of the public realm and the consequent withdrawal from political life into private life." The editor asks the contributors to raise the level of their speculations above the particularities of the 1972 presidential election, although he allows that it may be argued that "the only thing wrong with the political whole precluding hopeful participation is its present leadership" (p. xiv).

The contributions, while arranged in three unnamed and apparently undifferentiated sections, are all thoroughly individual and some of them highly personal. They do not fall into any clear categories, although some of them are much less concerned than others simply to lament present calamities. There is thus a lack of unity in the book, each contributor spinning his own thread, and no attempt being made to draw them together. The variety of the contributions, and especially the distinctive focus on some particular aspect of decay that each of them has, makes it difficult to say what the book as a whole is about. The editor suggests that "the ranging scope of this volume as a whole represents a questioning if not a denial of any restriction of the political to 'politics' and so of political or public life to 'elected public office'" (p. xvi). But even if the import of this were clear, it would also be clear that nothing like the whole of the work is about it.

One wonders whether the broad theme given to the contributors could possibly have been dealt with adequately within the compass of a short essay. The contributions are certainly insufficiently comprehensive, and too concerned with the American polity, for the sweeping subtitle "Rethinking the Political" to satisfy the expectations that it raises. Furthermore, some of the contributions address themselves not to political decay but to the ills of society generally: and inasmuch as they leave unnoticed the links with politics, it is hard to know what to make of them.

There are two questions to be put to the book: What is it about politics that needs rethinking, that is problematic, or that is wrong? And what fresh thoughts are in order?

Paraphrasing one or other of the contributors—and the vagueness of some of these paraphrasings is meant to reflect the vagueness of their originals—what is wrong is: the institutionalization of inhumanity in American policy; the hypocrisy of modern America (John G. Hessler); various kinds of excess (even in individualism) in politics and society (L. John Roos); the dirtiness of politics and political actors; and the prevalence of *hubris* (William Arrowsmith). What is problematic is whether power such as America possesses can be humane (Joseph M. Duffy); whether a large modern government is controllable, especially when the people find it unintelligible and despair over its validity (William Pfaff); whether the political order can be concerned with morality, or only with the adjustment of conflict (Aldo Tassi); how to institutionalize pluralism; what to do about fatigue in American life (Robert C. Neville); and what to do about the dangerously limited powers that exist for transcending culture (D. S. Carne-Ross). What needs rethinking is attitudes to obscenity (George Anastaplo); what to do about a situation in which nothing is either sacred or profane, and in which there is no operative language of repentance and contrition for public men (E. A. Goerner); and the notion that everything is black or white (Elinor B. Bachrach).

None of the contributors will have it that a retreat from politics is legitimate, although some of them assume (uncritically, since they advert only to youthful enthusiasm in the Kennedy heyday) that a retreat is occurring. And the only systematic sets of ideals that are advocated are very familiar—the old vision of America, and Lockean liberalism, not much reinterpreted: see especially David Little's scholarly contribution, pp. 116–143.

As might be expected, many more or less specific proposals are presented to deal with the problems that are mentioned—getting the facts to the people (Allard K. Lowenstein); withdrawing support from society when government involves conscience in morally unacceptable activities; creating structures that are accessible and that make creativity possible; committing oneself to politics as a provisional and incomplete enterprise, and ceasing to look to the state for happiness; dedicating oneself, along with a few others, to making room for a truly common and sharable public life; remedying injustices rather than pursuing national interests; being very careful in translating aims into programs; seeking a *public consciousness* that is world conscious, has a sense of corporate responsibility, accepts dissent as politically functional, and is open-ended and flexible (Douglas E. Sturm, p. 165); doing something about obscenity to improve things; forming

groups to exploit the transcending power of literature; establishing universities of the public interest to be a "third force" (p. 255), which would perform some of the functions formerly performed by families and churches; and one of the contributors supplies a maxim to "operate in the exercise of political power: *Do not do that which may result in situations for which one does not possess the present means of remedy*" (p. 95).

Some of these proposals will no doubt seem naive and hopelessly idealistic to hard-headed critics—indeed some of the authors have reservations about their practicality. But some of the proposals are perhaps worthy of further development. They certainly need developing, for in several cases, what they amount to is unclear, their impact on and consequences for a political system are quite uncertain, and the reasons for supposing them to be desirable are not developed in any depth. As they stand—some of them quite sweeping, all of them emerging out of necessarily swift diagnoses of the situation and presented to the accompaniment of necessarily brief elaborations and justifications—it is hardly possible to deal with them seriously.

D. M. WHITE

Monash University, Australia

The Social Contexts of Research. Edited by Saad Z. Nagi and Ronald G. Corwin. (New York: Wiley-Interscience, 1972. Pp. 409, xiv. $13.50.)

Looking backward from America's tricentennial in 2076, the historian of science may well wish for a source-book giving an overview of the growth and change in science as an activity, in the twenty-five years following World War II. Ideally such a source-book will offer many statistical series, a broad perspective, and an insight into the values, assumptions, problematics and cultural orientations of the period in question. Fortunate indeed will be the historian who stumbles across *The Social Context of Research* in the library stacks (or who tunes it in on the console of a research machine?). In this work Professors Nagi and Corwin have assembled a timely, thoughtful and well-written set of essays which illuminate the immediate past, and the present problems, of research as a social activity at the end of a momentous quarter century for the scientific enterprise.

Adopting a somewhat less Olympian stance, and merely looking forward from the present, one may venture that these essays will provide a starting point for many important investigations. By codifying the available literature on a fundamental subject, they have both highlighted its importance and provided a reference point for subsequent enquiries. The editors' initiative is thus doubly welcome.

Nagi and Corwin provide a concluding chapter on "The Case of Educational Research" (which rightly causes them much unease and seems in part to have motivated their decision to create this volume, in search of better things). They also offer "The Research Enterprise: An Overview," by way of general introduction. The other contributions fall into three sets—(1) those which codify available insights and information (Howard M. Vollmer on "Basic and Applied Research"; Richard A. Tybout on "Economics of Research"; Simon Marcson on "Research Settings"; Robert L. Hall on "Agencies of Research Support"; Norman W. Storer on "Relations among Scientific Disciplines" and James P. McNaul on "Relations between Researchers and Practitioners"); (2) those that speculate on the validity of our present knowledge and perspectives (Roger G. Krohn on "Patterns of the Institutionalization of Research"; Don E. Kash on "Politics and Research" and Irwin Deutscher on "Public and Private Opinions"); and (3) an essay that reacts sharply to the tensions of the late 1960s (William Petersen on "Forbidden Knowledge"). This last entry makes fascinating reading. The first set, which through the very act of codification yields new insights, also provides much food for thought. But it is the second set which seems to offer some of the most intriguing possibilities, and calls for further brief comment.

Irwin Deutscher explores the embarrassing consequences of the fact that "one of the few instances in which an attitude is unlikely to be translated into an opinion or an act in any social context is when it is elicited in a rigorously controlled interview situation by highly trained interviewers employing a technically high quality instrument." In other words, what one says depends as much on one's audience as one's intentions. This disturbing reality carries weighty implications for much social science, but Deutscher is better at documenting the notion than developing its implications. In "Politics and Research" Don E. Kash explores the growing awareness of both scientists and politicians that the support of science necessarily involves political as well as intellectual choices. On the basis of a careful analysis he is bold enough to offer a six-point prognosis for the 'seventies. His whole analysis might usefully be contrasted with that of Yaron Ezrahi ("The Political Resources of American Science," *Science Studies*, 1971).

The most provocative paper is that of Roger G. Krohn. He boldly divides the history of science into three phases. In the first, science was nonprofessional and idiosyncratic. In the second, from roughly the mid-nineteenth century, science was not only academic and professional, but a "denomination struggling for adherents, having devices for exclusion of unbelievers, and granting

rights to those qualified to interpret the truth." The third phase, that of "big science," dates from World War II. In it science is again being transformed by a new system of support, and again finding a new place in society. Because of our failure adequately to analyze these three phases "we do not understand science." Furthermore "the traditional belief that there is one known and most favorable situation for science is ill-founded." Krohn argues his case well, and interested readers will find in Stephen Toulmin's recent book *On Human Understanding* (New York: O.U.P., 1972) much further material that analytically develops notions only implicit in Krohn's presentation.

What is apparent in all these essays is how the science-technology-medicine-complex has come to impinge on our social and political worlds through the last twenty-five years, and how much there is now to learn from systematic attention to the social contexts of research.

ARNOLD THACKRAY
University of Pennsylvania

The Political Theory of a Compound Republic: A Reconstruction of the Logical Foundations of American Democracy as Presented in The Federalist. By Vincent Ostrom. (Blacksburg, Va.: Center for Study of Public Choice, 1971. Pp. 132. $6.95, cloth; $1.25, paper.)

One of the consequences of the Watergate incident frequently cited by commentators is that Congress has been or will be able to reassert its proper role vis-à-vis the Executive: the monarchical presidency might be dismantled. Madison and Hamilton, Vincent Ostrom would probably suggest, had predicted this would happen; in a compound republic no single part can gain excessive power. For those of us raised on the emergence of presidential government such a view would previously have been dismissed as conservative resistance to an active, aggressive set of government policies. But times change, after all there are now as many liberal filibusters in the U.S. Senate as there are conservative ones. Faith in presidential government as a solution to our problem has evaporated in the face of Vietnam, political trials, and Watergate.

In another sense, prevailing wisdom is challenged by Professor Ostrom in his perceptive little monograph. Most accounts of *The Federalist* emphasize their polemical nature or perform an exegesis of a particular Number or theme—"faction" for example. Ostrom makes a significant departure from these in arguing that *The Federalist* should be viewed as a coherent, consistent body of political theory: theory in a true sense, basing its argument on logically consistent basic assumptions. These are not explicit assumptions according to Ostrom, but, nonetheless, easily discernible. Relying mainly on recent writings in political economy as his paradigmatic basis, Ostrom suggests the following basic principles underlying *The Federalist:*

(1) *The Individualistic Assumption.* "Men as individual persons should be the fundamental unit to be considered in organizing any political association" (p. 18).
(2) *Relative Advantage.* Individuals are guided by self interest and will seek to maximize their interests (pp. 21–23).
(3) *Human Fallibility.* "The condition of human fallibility requires analysis . . . reconsideration and an opportunity to alter, amend or change as new information and new understanding give rise to new possibilities" (p. 25).
(4) *Political Constraint.* "When men pursue their individual advantage to the detriment, disadvantage or injury of others, constraint is necessary if the greater good is to be realized" (pp. 25–26).

Building on these basic assumptions, Ostrom proceeds with a tightly argued interpretation of how *The Federalist* provides principles for construction of a government which incorporates these assumptions, as well as the importance of concurrent regimes and the distribution of authority among national decision makers in assuring their preservation. Space limitations preclude examination of each point; suffice it to say that the reader is provided with discussion of a host of topics—security, due deliberation, expression of self-interest, and taxation among others—as they relate to the organization of government in a compound republic. The author's cross-referencing his citations to three different editions of *The Federalist*—Modern Library, Cooke, and Rossiter—is very useful.

There are two problems with Ostrom's analysis—one trivial, the other major. Of minor importance is the author's general style and tone. Although the book is quite short, it is often tediously repetitious. Points just made are summarized and then repeated at the start of a new section or chapter. Of consequence, however, are the prescriptions of the book. Recent developments such as the antiwar sentiment and Watergate notwithstanding, Congress is hardly in a position to challenge even the diminished presidency of Richard Nixon. The notion that the system of countervailing powers at the national level persists in modern times is questionable at best. In addition, revenue sharing or what-have-you cannot eclipse the inherent inferiority of states in the vertical relationship with the federal government. One has only to thumb through recent works such as M. Reagan's *The New Federalism*, to see the essense of these new rela-

tionships between levels of government. To suggest, then, as Ostrom does, that the theory of *The Federalist* is appropriate to meet modern world realities is simply not supported by the author.

These latter points, however, should not obscure the merits of this study. They relate to conclusions, not to the quality of analysis preceding them. Ostrom has provided a novel and pertinent re-examination of the thinking of Madison and Hamilton. The use of concepts from the literature of public choice provide a new departure point for study of some old problems. Ostrom's work deserves the careful attention of students of American government and American political thought. If the prospects of serious intellectual challenge are not enough, the price of a copy alone makes this one of the best bargains on the market. One hopes that future monographs in this series will provide the same combination of scholarship and economy.

ROBERT S. ROSS
California State University, Chico

Obligation: A Social Theory. By Ralph Ross. (Ann Arbor: The University of Michigan Press, 1970. Pp. 200. $7.50.)

The great accomplishment of John Dewey's *Human Nature and Conduct* was to locate morals in the context of everyday *individual* life. By showing how our actions involve decisions about "right" and "wrong," Dewey made it obligatory for students of human behavior to take account of such decisions in the same naturalistic terms with which they dealt with attitudes, feelings, thoughts, and perceptions. What Dewey left to be achieved was to locate morals in *group* life, that is, in social and political conduct. That task requires that one show how those spheres are also permeated with moral qualities. Professor Ralph Ross's study of *Obligation* has now achieved that crucial task.

Like so many other important achievements, this one seems very simple once stated. We start with the idea that the patterns we see in conduct depend upon the analytical categories we bring to bear. Thus, the sociological aspects of conduct are those analyzed under such categories as statuses, roles, groups, institutions, and social systems, while the political aspects are those analyzed in terms of categories such as power, authority, influence, decisions, rule, and government. In the same vein, then, the moral aspects refer to conduct considered in such terms as choice, responsibility, evaluations, and obligations.

Each of these sets of categories is *limited* in that it deliberately selects out from the totality of human conduct only certain aspects for analysis. No set of limited categories can embrace the totality of conduct; none is "more basic"; all are equally "natural" features of behavior. Hence, when morality is viewed this way it becomes possible to analyze the conditions, correlatives, and consequences of moral conduct, and various systems of morals can be compared as objectively and profitably as systems of political, economic, or social behavior.

The locus of morality, for Ross, is in the obligations contained in the roles that are attached to statuses such as parent-child, husband-wife, ruler-citizen, teacher-pupil. We all occupy many of these statuses and they constitute the building blocks of all social systems. Effective freedom of choice among alternative sets of such role-obligations is both the defining criterion of moral behavior and the major source of variation in moral systems. Without such freedom, it is meaningless to impose any judgment of right or wrong. If a person is required by physical force or the absence of alternatives to follow a single line of prescribed action, then, in Ross's terms, the behavior is neither moral nor immoral; it is nonmoral.

The variability arising from differences in freedom of choice is conceptualized by Ross in terms of *existent* versus *critical* morality. The former refers to ongoing, unexamined patterns of conduct, formed of deeply habituated patterns of obligations, values, preferences and criteria of judging between right and wrong. All societies, of course, are "equal" in this type of morality.

But critical morality is a different matter, being present only when there are alternative courses of conduct to choose among, and when such decisions are made in terms of how well the chosen paths will serve desired ends and how much they will constrict or expand the human potential. Societies obviously differ greatly in the extent of their critical morality. Moreover, with this specification, the moral domain takes on certain qualities not found in other domains. For in none of those is freedom of choice a defining constituent. No society, however underdeveloped or unfree, is any less political, economic, or social than others.

The great value of this distinction between existent and critical morality lies in the break it makes with "cultural relativism," the doctrine which social scientists have traditionally invoked in insisting that one cannot usefully or meaningfully compare the moral systems of different cultures. Ross declares that not only *can* such comparisons be made but that indeed they *must* be made, if the most important human decisions are to be brought under rational scrutiny. The moral cynic's question, "Why is critical scrutiny of moral standards any better than simple uncritical acceptance of them?" is then seen as the *beginning* rather than the terminal point in moral discourse.

A few steps into that discourse reveals that

"cultural relativism" rests on an unexamined belief in the "equal humanity" of all people, regardless of the conditions of their lives. But Ross argues persuasively that "humanity" is a variable and not a constant; that the faculties which define humanity can be either stunted or developed; that the conditions of development of those faculties, including the moral, can be specified; and that while, finally, individuals and societies may "prefer" to remain undeveloped in their aesthetic, intellectual and moral faculties, this cannot be said to be a "choice," unless there has been a prior awareness of alternatives and freedom to choose among them. Only with such awareness and freedom is it possible to realize the full humanity of the species.

It is the task of critical morality, Ross asserts, to inform us about the profits and costs of alternatives, drawing upon the social sciences and the humanities for the information needed. Disdain for this enterprise has made the social sciences seem lifeless, uninspired and unconnected to crucial concerns of the people they study. For Ross, then, the establishment of critical morality as a legitimate discipline is essential to reconnect the study of human conduct to the fullness of that conduct itself.

> Man can be of only dubious consequence to himself or society if he is identified totally with sociological or behavioristic man, for man as stage actor or responder is not man as moral agent. The latter, but not the former, involves reason fully, and must be thought of under the categories of choice, decision, and responsibility (p. 53).

The program Ross advocates does not involve the surrender by the sciences of their insistence on objective inquiry into human conduct. On the contrary, such concern for the facts, gathered objectively under the canons of scientific procedure, must be preserved if we are to get the reliable information we need to choose among competing courses of conduct in far more enlightened ways than now seems possible. Nor may we refrain from such decisions on the grounds of their arbitrariness. For, minus God or other absolute standards, "arbitrariness" can refer only to variations in degree of reasonableness, information, rationality, and constraint. Since moral decisions vary greatly in these regards, they cannot sensibly be said to be all equally arbitrary.

Nor can decisions on moral questions be excluded from serious study on the grounds that one cannot logically go from a statement of the "is" to a statement of the "ought." That rule describes limits on grammar, but it has nothing to say about actual conduct itself, and the ways in which the "is" and the "ought" are closely connected at all times in such actual conduct. The pursuit of this line of argument reveals that the "relativist" finally has to retreat to a solipsistic cynicism, from which posture no clarification of human conduct is possible. The final position of the solipsist is the denial of life. That always stands as a human option, of course, and its implications are clear.

Ross's formulation has powerful liberating implications as well for the study of moral development. With his conceptions of the moral faculty, and the *enabling* role of freedom of choice, it becomes possible to study the processes and stages of moral development without defining those stages in terms of culturally-specific values, such as the democratic ethos. Effective freedom, and, by implication, the democratic society that maximizes such freedom, are now seen as the *conditions* required for, but not the content of, a fully developed moral faculty. People who reach that stage of development are always free to choose anti-democracy and anti-freedom courses of conduct, knowing full well that if that choice is implemented, there will be a decline in future critical morality and in individual moral development. Socrates may have thought that to know the good was to be unavoidably committed to doing the good. But the well-tutored and powerful fascist wise-guy knows better.

To liberate two major lines of consequential intellectual venture, as Ross has done, is itself an extraordinary achievement. But there are other matters of high value in this slim and elegant volume. Limitations of space make it possible only to state them briefly.

Chapters 8 and 9 offer an intriguing distinction between political science and sociology, identifying the former as concerned with overt, conscious and intentional decisions and consequences, while the latter is concerned with the "covert" patterns, those that are less conscious and intentional. In the same chapters, the discussion of "functionalism" is advanced beyond anything now in the literature, while in the chapter on the "Social Functions of the Intellectual," the author expands the horizons of intellectual activity so as to revivify the life of the mind and reinvest it with a much-needed espirt. In his discussion of "The Limits of Consensus," Ross provides a model of how to weigh and choose among moral alternatives, here between stability and flexibility as qualities of social organization. In analyzing the diverse forms and consequences of these two qualities of social life, Ross shows how well a brilliant polymath, such as he is, can enlighten human choices when he brings evidence from science, philosophy and art to bear upon the issues. In short, he provides us with a model of critical morality working at its best. Finally, in his concluding chapter on "Obligation in Our Time," Ross attacks some of the knottiest problems regarding "alienation" in a way that cuts decisively through the morass of sentimental con-

fusion so characteristic of most writings on that subject today.

To sum up: Ross shows that democracy, freedom, and critical reflection, using the insights of philosophy, science, and art, can make for richer and fuller lives. *Should* we prefer richer lives over other forms of existence? *Should* we be "more human" rather than "less human," where being more human means the fuller exercise of the moral and other faculties? Suppose one does not want to? Must one? No, one must not; one need not; but the consequences of doing so or not doing so are eminently clear. That is what critical morality can and does tell us. And that is why education in critical morality is an indispensable feature of any sensible curriculum.

This may all add up to whistling in the wind, as far as any influences on the conduct of education are concerned. After all, Dewey's equally seminal and penetrating discoveries in *Human Nature and Conduct* have gone largely unattended. But one can do no less than to call attention to what is in store for us if we persist in the desiccation of our study of human conduct, and, alternatively, what promises are implied about what we might become if we attend to the wisdom of this fine book.

MELVIN M. TUMIN

Princeton University

The Idea of Culture in the Social Sciences. Edited by Louis Schneider and Charles Bonjean. (Cambridge and New York: Cambridge University Press, 1973. Pp. 149. $9.95, cloth; $2.95, paper.)

The concept of culture is widely used by social scientists, although they often do not agree on its meaning. This book includes essays by scholars representing the disciplines of anthropology, sociology, economics, political science, history, and geography, who have attempted to identify the role of the concept in their discipline. As the editors have stated: "It was hoped that from scrutiny of their various contributions, significant convergences and divergences in usage of the idea might appear and that some important hints might emerge with regard to the unity—or lack of unity—of the social sciences" (p. vi).

Anthropologist James Boon presents, with great erudition, a synthesis of the use of the culture concept in his discipline. One gets the impression that he is passionately groping for a higher-level understanding of the concept. Sociologist Talcott Parsons attempts to summarize his work on the distinction between the cultural and social systems. In his explication of the four parts of the cultural system—employing the categories of cognitive symbolization, moral-evaluative symbolization, expressive symbolization and constitutive symbolization—he provides an elaboration of the culture concept.

Economist Kenneth Boulding advocates the development of a cultural economics which would provide a new dimension to economic analysis. He is distressed by the lack of interest among economists, with the exception of labor economists, in the role of culture. He points out that anthropologists have also neglected the study of economic institutions, as exemplified by Margaret Mead's refusal to consider writing, at Boulding's suggestion, "Coming of Age in the Federal Reserve." He presents an exceptionally lucid analysis of the relationships between phenomena at the macro- and microeconomic levels. His discussion of macroeconomic paradoxes (p. 62–64) is most interesting; he shows, for example, that decisions to save on the part of individuals may not result in the net accumulation of capital but may lead to increased unemployment and a decline in output. Or a rise in money wages may result in smaller real income during periods of inflation.

Political scientist Lucian Pye also examines problems at the macro (political culture) and micro (personality) levels. He argues that the basic problem of behavioral political science is to understand how individual psychology can account for the performance of political entities. He notes the tremendous diversity of subjective experience, only a small fraction of which is registered in behavior, and alleges his preference for going from the performance of the political system to the analysis of its probable psychological antecedents. He supplements the analysis by checking the hypotheses developed from it through survey research.

Historian Robert Berkhofer, Jr. associates the concept of culture with broad theories of history and society which leads him to criticize certain historians who have overstressed the importance of ideas derived from particular views of culture, as determinants of historical events. Geographer David Sopher examines the spatial patterning of culture, which shows that cultural notions have provided geography with important descriptive material and research tasks.

Finally, sociologist Louis Schneider reviews the contributions of the other participants and provides suggestions for developing and elaborating the concept of culture. He examines notions like automatization (cultures evolve in an autonomous fashion), hierarchy (elements of culture can be found at different levels), and alternation (members of different cultures do things that superficially look different when in fact they are equivalent), and suggests that they should be developed even further in order to increase the utility of the concept of culture. He finally expresses a cautious hope for the future of the idea of culture in the social sciences.

The volume is useful for those who wish to

rethink their use of the concept of culture and also those who wish to become acquainted with the viewpoints of other disciplines. Yet I myself did not get any significant new insights from this collection nor find any clarification of why some of the simpler definitions of culture might not be preferable.

My own favored definition is that which anthropologist M. Herskovits offers in *Cultural Anthropology* (New York: Knopf, 1955): "Culture is the man-made part of the human environment." This definition is striking in its simplicity and clearly includes Tylor's much quoted definition. Schneider objects to "terminological inflation," which occurs when a concept includes too much. This problem can be dealt with, however, by proper qualification of a term, such as material culture, subjective culture (e.g., Triandis, *The Analysis of Subjective Culture*, New York: Wiley, 1972), political culture, scientific culture, etc. It seems to me that the strategy of a *clear*, although admittedly too inclusive, definition is likely to increase effective communication. After all, the purpose of a scientific construct is to aid in thinking and in communication among scientists. Boon's analysis in this volume does stress the communication aspect of the construct, Parsons emphasizes the symbolic aspect, and the other essays clearly show that the function of a scientific idea is communication.

Herskovits's definition is certainly one of the ideas that is most easy to communicate. With proper qualifications it can be sharpened to be at least as useful as the definitions that are proposed by the essayists of this volume. For example, subjective culture can be defined as the subjective reaction to the man-made part of the environment. This definition immediately suggests that attitudes, beliefs, norms, roles and values are elements of subjective culture, and they can in turn be defined to fit into the broader category. For example, attitude can be defined as an idea, charged with affect, predisposing action toward some entity of the man-made environment. This line of argument was not considered in this volume, although it seems one that could have been fruitfully discussed. Perhaps it is a line of argument that only a psychologist, like myself, would find useful, and the failure of the volume to include a contribution by a psychologist resulted in this omission.

HARRY C. TRIANDIS
University of Illinois, Urbana-Champaign

The Structures of the Life-World. By Alfred Schutz and Thomas Luckmann. Translated from the German by Richard M. Zaner and H. Tristram Engelhardt, Jr. (Evanston, Illinois: Northwestern University Press, 1973. Pp. xxxvi, 335. $10.50.)

This volume is the first installment of the originally unfinished manuscripts of Alfred Schutz completed by his student, Thomas Luckmann—a *magnum opus* which took many years of labor from both of them. The publication of the second installment is expected in another year. Before his death in 1959 at the age of sixty while teaching in the graduate faculty of the New School for Social Research in New York, Schutz had finished only one book—*Der sinnhafte Aufbau der sozialen Welt* (1932), translated in 1967 as *The Phenomenology of the Social World;* in it he confronted Max Weber's sociology with Edmund Husserl's phenomenology in such a way as to lay the foundation of his later works. Though unfinished before his death, *The Structures of the Life-World* (*Die Strukturen der Lebenswelt*) represents the lifelong project of a great thinker whose themes have been scattered throughout his papers and manuscripts which were collected and edited in three volumes of *Collected Papers* (1962–66) and *Reflections on the Problem of Relevance* (1970).

Schutz devoted his whole life to "the problem of social reality" as aptly suggested in the subtitle of the first volume of his *Collected Papers.* As *The Structures of the Life-World* testifies, Schutz's phenomenology is an archaeology of social reality or the everyday life-world as the intersubjective world. The importance of this work of Schutz's (and of his other works as well) for the human or social sciences is indicated by his conceptualization of social reality as "the sum total of objects and occurrences within the social cultural world as experienced by the commonsense thinking of men living their daily lives among their fellow-men, connected with them in manifold relations of interaction" (*Collected Papers*, I, 53). Political reality may be viewed as a specific region or subworld of social reality thus defined. By reality or world is meant "certain unities of meaning" organized by human consciousness in certain fixed, specific ways correlative to external things both human and natural. In reading *The Structures of the Life-World*, those who know Schutz's previous works will recognize the familiar terrains and contours of his recurring themes. In it, however, there are surprises and additions and detailed analyses of ideas that previously were only sketched (see, for example, the fourth and last part on "Knowledge and Society").

Political scientists need to understand the very basic phenomenological framework underlying *The Structures of the Life-World.* First, phenomenology is a philosophy of meaning. Unlike natural objects, men, whose actions are the subject matter of the social sciences, are conscious and embodied beings who are capable of endowing meaning to their actions, that is, of performing their acts with goals in view. The task of phenomenology, therefore, is similar to an ar-

chaeological expedition which attempts to excavate the sedimented layers of the common-sense understanding of actors in the social scene: it is to explore the meaning of meaning. Second, following Husserl who discovered the importance of the life-world for theoretical enterprises, it is the task of Schutz and Luckmann in *The Structures of the Life-World* to *show* the relevance for the social sciences of the life-world *as* social reality where men as bodies endowed with active consciousness pursue their practical interests in interaction with other men. Above all, the decisive importance of the life-world (and thus phenomenology) is twofold: the life-world is the practical province of action whose analysis has the central place in phenomenology as in the social sciences, and it is also an intersubjective world. As the life-world is intersubjective from the very beginning, intersubjectivity is the modality of human existence "pregiven" to everyone as long as man is born of woman. The life-worldly experience characteristic of the normal, wide-awake adult in his natural attitude is the "primal type" of reality we experience; and all the other provinces of meaning are construed as modifications of it. By the same token, the stock of knowledge necessary to carry out everyday life-activities is also pre-eminently socially derived. In contrast to theoretical thinking which brackets everything in radical doubt, the life-worldly stock of knowledge is "taken for granted" or accepted as "self-evident" without question ". . . until further notice," although it is in principle questionable in a problematic situation. By means of his stock of knowledge at hand, the actor is interested in orienting himself in routine ways toward practical achievements. In the everyday life-world intersubjectivity or sociality itself is taken for granted in the following way:

> (a) the corporeal existence of other men; (b) that these bodies are endowed with consciousness essentially similar to my own; (c) that the things in the outer world included in my environs and that of my fellow-men are the same for us and have fundamentally the same meaning; (d) that I can enter into interrelations and reciprocal actions with my fellow-men; (e) that I can make myself understood to them (which follows from the preceding assumptions); (f) that a stratified social and cultural world is historically pregiven as a frame of reference for me and my fellow-men, indeed in a manner as taken for granted as the "natural world"; (g) that therefore the situation in which I find myself at any moment is only to a small extent purely created by men (p. 5).

Third, as phenomenology is concerned with the experiential meaning the actor confers to his action, the analysis of action must take into account the structure of internal time-consciousness or temporality as experienced directly by the actor himself. According to the temporal dimensions as experienced by the actor, Schutz formulates his typology of the social world: the worlds of (1) "consociates" (*Umwelt*), (2) "contemporaries" (*Mitwelt*), (3) "predecessors" (*Vorwelt*) and (4) "successors" (*Folgewelt*). Among these four worlds (or more precisely subworlds), the world of contemporaries is most directly relevant to the social-science analysis where the role of man is understood in terms of anonymity and typicality.

The Structures of the Life-World is divided into four parts: (1) "The Everyday Life-World and the Natural Attitude," (2) "The Stratifications of the Life-World," (3) "Knowledge of the Life-World" and (4) "Knowledge and Society." The first part is a short outline of the everyday life-world as man's fundamental and paramount reality or the paramount province of meaning which is taken for granted by the normal, wide-awake adult in interaction with other men through his common-sense knowledge. The everyday life-world is not only the world which modifies our actions but also the world which we in turn modify through our actions. The second part deals with the temporal, spatial, and social stratifications of the everyday life-world as we experience or live it. Schutz and Luckmann start their analysis with the *immediate* experience of the Other in the "thou-orientation" and the "we-relation" where we share simultaneously the flow of each other's lived experience and extend it to the *mediate* experience of the Other in the "they-orientation" of contemporaries (as well as of predecessors and successors) in relation to typification and anonymity whose "functionary type" (e.g., *the* government) is of great importance for political science. The third part is concerned with various phases of the actor's stock of common-sense knowledge. It ranges from the analysis of the situation, types of common-sense knowledge essential to the performance of action and the acquisition of knowledge to the problem of relevance. As life taken as a series of situations is conditioned by relevance, that is, by our "interest" in a given problem or something that "really matters to us," Schutz and Luckmann consider relevance as the most important though most difficult problem in describing the everyday life-world. With great care and detail they analyze relevance in its thematic, interpretational, and motivational structures and their intertwinement. The fourth and last part deals with the social derivation of all knowledge as "intersubjective mirroring," the presuppositions of knowledge as subjectively experienced and how it is objectified and how knowledge is transferred and distributed socially. Thus it is both a claim for a *social* theory of knowledge and a significant contribution to the sociology of knowledge. In short, it is a social archaeology of all knowledge.

In summary, *The Structures of the Life-World* is a work of great magnitude in the epistemology of the social sciences in general and phenomenological social science in particular. By revealing the *social* foundation of social-scientific epistemology, it has shown the relevance of the phenomenology of the life-world to the human or social sciences. It occupies an important niche in the philosophy of the social sciences. Its cardinal merit lies in showing how to *practice* rather than how to *talk about* phenomenology. With Schutz, phenomenology has finally come of age in the social sciences. The inclusion of Schutz's selected writings expertly edited by Helmut R. Wagner in "The Heritage of Sociology" series under the title of *On Phenomenology and Social Relations* in 1970 is a testimonial to this fact. Schutz's influence on the methodology of the social sciences has been growing steadily since his death—notably in the ethnomethodology of Harold Garfinkel, Aaron Cicourel, and others in American sociology today. Schutz's phenomenology makes a revolutionary challenge for the discovery of a paradigm in formulating the general principles of epistemology for both the social and natural sciences. It adds an important impetus to the development of the philosophy of the sciences. As for our profession, Schutz's phenomenological insights offer a genuine alternative to the philosophy of political science couched in the dated epistemology of logical empiricism that has dominated the American scene so long—perhaps too long.

HWA YOL JUNG

Moravian College

Mind and Politics. By Ellen Meiksins Wood. (Berkeley: University of California Press, 1972. Pp. 192. $7.50.)

Professor Wood, who teaches political theory at Glendon College, York University, has written a provocative, lucid study of the relation between epistemology and the differing concepts of individualism, which mark liberal and socialist political philosophy. To the liberalism of Locke and J. S. Mill she attributes the proverbial *tabula rasa* image of the mind passively receiving experience. To Rousseau and Marx she attributes a Kantian notion that the mind is a positive participant in the construction of experience through "dialectical" interaction between *a priori* categories of thought and the external world. Although she recognizes that both views may arguably fall within the same broad tradition of empiricism, her main thesis purports to be that these theories of knowledge imply quite disparate political results. Despite its apotheosis of individuality, liberalism creates an atomistic society in which individuals become objects or occupants of externally imposed roles, functions, and interests. All genuine sense of community is missing. The alienated individual confronts the state, like Mount Everest, as something that is simply "there." In contrast, Professor Wood holds, the dialectical theory of knowledge implies an inseparable interrelation between true individuality and community—within a socialist future. It promises the "convergence in all men of public and private" (p. 109) and the "transformation of human relations" (p. 169).

All of this involves some problems of interpretation and omission. The author herself concedes in the end that Rousseau must be understood "in moral rather than epistemological" terms (p. 172). As for Marx, she remarks early on that it "can be argued" (p. 7) that he falls within Kantian epistemology. But from then on, the argument becomes largely assumption. Relying a bit too much on the admittedly epigrammatic *Theses on Feuerbach*, she misses other important points of reference, including Marx's own explicit statement of a materialist theory of reflection in his preface to the second edition of *Capital*. Rather than letting Marx speak for himself, she too often reads into him the views of Fromm, Avineri, Sartre, and Marcuse, who happily agree with her but hardly represent the full range or sharpness of debate over Marx's epistemology. The names of Kolakowski and Schaff, for example, are curiously missing. Nor does she satisfy this reviewer on Marx and history. Dismissing the usual deterministic interpretations as "misconceptions" (p. 178), she nevertheless comments that for Marx the French Revolution could have been only bourgeois, since "historical circumstances and the development of productive relations as yet allowed nothing more" (p. 164). If that implies a dialectic in history, it leaves us uncertain about the exact point in history at which the mind of individual man will triumph in dialectical mastery of the external world. The possibility that that promise—whether from Rousseau or Marx—may somehow be betrayed is hardly touched by Professor Wood.

Yet, in dealing with liberalism, she draws in detail what she considers its betrayal—not only in liberal society but in the pessimism which she finds below the surface of liberal theory. Although she notes Locke's moral norms of natural right and voluntary contract, she considers far more significant his doubt that individuals will in fact behave rationally and his recognition of class inequality in postulating the existence of money prior to the social contract. As for J. S. Mill, she even suggests that his concept of free individuality may have been an "aberration" (p. 41) in his overall philosophy.

But given a twist in the meaning of "dialecti-

cal," the case of Mill may instead suggest that it is liberalism itself which in fact meets the demanding standard initially posited by Professor Wood. As she remarks more than once, actually in complaint, both liberal theory and society contain "an eternal tension . . . between individuality and sociality" (pp. 123, 159). In turn, her own futuristic vision of a conflict-free society in which individuality and community converge seems to imply on occasion the end of the dialectic (pp. 120, 170). And so ironically, it may be the latter-day liberals, the heirs of J. S. Mill, who alone live with an unending dialectic as they forever restate their creed in response to the refractory world around them. Or, why not just admit, as have so many commentators on Locke and Mill, that there has all along been a "discrepancy" between epistemology and political prescription in liberalism, rather than arguing either that one leads necessarily to the other or that one may be an "aberration" from the other? It may simply be that many political philosophers in the age of modern science and material progress have found any idealist theory of knowledge impossible to accept, but at the same time have found the dream of ultimate individual freedom irresistible. That may fit Marx as well as the liberals. And of course it badly compromises Professor Wood's basic thesis about the importance of epistemology.

But in the end does she still hold to that? There are hints along the way that it could be an open question whether a political thinker's epistemology or prescription in fact comes first. She concedes as well that our disputes over epistemology *per se* may forever elude resolution. And so in her final lines, Professor Wood suggests as an alternative that we see "the controversy as . . . a choice between different designs for man's life in society" (p. 185). By that she apparently means again liberal society *vs.* a Rousseauistic-Marxist vision. Whichever we choose, we can then presumably work our way back to an appropriate theory of knowledge and human nature. One may understandably be tempted to wonder whether this closing turnabout is quite fair play. But despite these difficulties, Professor Wood has managed to hold the problem of epistemology and political prescription in a sustained focus which is unusual in contemporary commentary. Anyone seriously interested in that general problem, or concerned more particularly with liberal and Marxist thought, indeed stands in her debt.

VINCENT E. STARZINGER
Dartmouth College

Television and the Public. By Robert T. Bower. (New York: Holt, Rinehart and Winston, Inc., 1973. Pp. 205. $7.95.)

Many scholars who are increasingly concerned with the pervasive influence of television on political behavior will welcome the publication of this book. Bower, head of the Bureau of Social Science Research in Washington, D.C., deserves credit for a solid examination of mass perceptions of television: who watches what, when, and why (according to a national survey conducted by the Roper Organization in 1970). His research replicated a study by Gary Steiner ten years earlier (*The People Look at Television: A Study of Audience Attitudes*). In addition, Bower analyzed the diaries of a viewing sample of Minneapolis-St. Paul households. It must be noted that the Columbia Broadcasting System sponsored both studies; however, there is no taint of bias on Bower's part.

Tables, charts, and graphs proliferate throughout the book. Unfortunately, readers are forced to apply their own tests of significant differences on some sets of statistics which would benefit by their inclusion. Despite a methodical redundancy in much of the text and Bower's occasional tendency to dwell on rather tedious findings, the data are clearly organized.

Although much salient data on public opinions of television have already been incorporated into conventional wisdom, this work is an impressive documentation of the paramountcy of television as America's most important source of significant political information. It should be noted that Bower's focus on perceptions of presidential election coverage and network news (albeit very brief inquiries related to educational and cable television are included) reinforces an emphasis on national politics that television itself has encouraged. Not even implicitly does the study suggest how television has helped to shape our concepts of community and thereby modify the potential of politics in society.

Little is said about political socialization *per se*, but there is much information that scholars in this field may find useful. Bower found, for example, that race was the best predictor of people's general attitude toward television in 1970. Blacks, regardless of education, took a considerably more sanguine view of both programming and commercials than whites. Inexplicably, whites viewed an average of three more hours per week than blacks.

In dealing with the age, Bower utilized multivariate and cohort analyses to rebut gently the "generational" theories of opinion differences. Strangely, questions about age-race relationships were not even raised.

As of 1970, Americans watched more television and with greater satisfaction than ever before. College-educated people were most likely to be critical of television. They spent slightly more time in front of their sets than less educated viewers and there was no discernible difference in their viewing habits.

Much of the criticism of television alluded to

excessive violence; other criticisms mentioned moral problems in general and children viewing adult programs in particular. The most objectionable violence was real violence: crimes, demonstrations, and wars covered by television news cameras. Other such data revealed by Bower (and elsewhere) will undoubtedly be utilized in pressuring news editors to project a more roseate view of current events.

The title may mislead impatient readers concerned with more profound inquiries than what people say they think about television. Americans may complain mildly about commercials, but they are unlikely to divulge much about the subtle effects of electronic hucksterism on themselves or on their political culture, generally. Most commercials are, after all, designed to exert a cumulative and subliminal influence on behavior. Similarly, students of the cumulative impact of explicit and excessive violence as entertainment are not likely to be enlightened by asking people what they think about television and how it affects them. Readers should know they can expect no more—nor less—than Bower promises in his Introduction. The publication of his basically one-dimensional analysis amounts to a small but worthy contribution to the literature on public viewing habits and attitudes about television.

PAUL CONWAY
State University College of New York at Oneonta

Congressional Voting in a Partisan Era: A Study of the McKinley Houses and a Comparison to the Modern House of Representatives. By David W. Brady. (Lawrence: The University Press of Kansas, 1973. Pp. 273. $10.00.)

Students of Congress, including those who concentrate upon contemporary behavior, should find Professor Brady's book a valuable addition to the relatively slim literature on the institution in the 19th century. The study consists of a complementary combination of history and quantitative analysis of roll-call voting in the 55th and 56th Houses (1897–1900). Brady makes generally good use of such tools and techniques as the Riker significance coefficient, indices of party cohesion and party likeness, factor analysis, and multiple linear regression in order to document and explain the extraordinary degree of party voting in these Congresses.

Briefly, the author's major thesis is that the high levels of party conflict and intraparty loyalty were produced by an interactive combination of external forces (chiefly constituency influences based upon the well-known socioeconomic and sectional cleavages of the period) and internal forces (principally the hierarchical and centralized party leadership structures in the House). Not unexpectedly, Brady draws upon the notion of the election of 1896 as a "critical" one and upon Polsby's account of the institutionalization of the House during the 1890–1910 "take-off" period. He outlines the issue agenda of the time (including the bitter clash between agrarian and industrial interests) and shows that these issues were the source of consistent, unrelenting interparty conflict, a conflict which was heightened by the structure of power in the House. The increasing volume and complexity of the congressional workload attendant on the emergence of the new agenda in the industrializing nation created a strong impetus for the centralization of power in the hands of an experienced and knowledgeable leadership.

Specific findings are too numerous to summarize here, but it should be mentioned that Brady finds the McKinley Houses to have the highest levels of party voting in the history of the body. Factor analysis shows that party is the sole factor which can be reasonably inferred to underlie roll-call outcomes. Because Brady is well aware that party sometimes has a certain "emptiness" as an explanatory variable, he goes on to explore the interrelationships between it and several constituency factors. Democrats are most likely to come from predominantly agrarian southern and western districts, while Republicans are more likely to represent industrialized northeastern or midwestern districts. Against individual party support scores the author regresses party affiliation, interparty competition scores, and an "Industrial-Agricultural Scale" score for the district. To control for regional effects he runs separate regressions for each of the four major regions. Unfortunately, data limitations force Brady to rely heavily upon the industrial-agricultural score as the sole measure of the nonpolitical aspect of constituency. Despite this limitation, constituency performs rather well in the regressions, generally following hard on the heels of party competition in terms of independent explanatory power, particularly in the 55th Congress. Methodological pragmatists will be gratified to know that the full and regional regression equations "predict" with considerable success (as such things go) in both Congresses.

Brady deserves commendation for his efforts to relate his findings to evidence about other sessions of the House, especially several 20th-century ones. He examines the works of Lowell, Turner, MacRae, Truman, Froman, and Shannon, finding both points of convergence and divergence with several of these authors.

The author's discussion of institutional factors is informative and represents a thoughtful application of the institutionalization interpretation. He contends that the leadership of the House was well-equipped to exercise control, in view of its clear experience advantage over the rank and file and also its simultaneous control over the

leadership of the standing committees. The high rate of membership turnover and large numbers of freshmen and second-termers created an environment in which the taking of cues from the party leadership became natural and expected. Moreover, as Brady observes, the leaders of the respective parties tended to come from districts which were prototypically Democratic or Republican in their constituency characteristics. Thus, the policy preferences of the leaders were readily translated into the official stands of the legislative parties, and were promoted effectively because of the positional advantages enjoyed by the party leaders. Brady finds that the most extreme supporters of the party positions were the leaders themselves. In sum, the influence of party as a voting cue was, as Brady hypothesizes, attributable to a unique combination of clearly defined constituency pressures and skilled, authoritative, and cohesive leadership. The extent to which this contrasts with the modern House is the subject of Brady's brief concluding discussion. Two obvious major differences are the greater heterogeneity of today's constituency interests and the absence of any substantial overlap between party and committee leadership in the modern House.

Because the book is rich in data and because the analysis is both meticulous and refreshingly modest in tone, there is some temptation to minimize flaws in the study. Most noteworthy is Brady's tendency to treat the House as an institution functioning in relative isolation from both the White House and the Senate. Despite the considerable technical skill which he exhibits in this book, Brady does little to answer the nagging question of whether it is possible to strive for generalization based upon statistical analysis of yeas and nays without also doing serious injustice to the rich complexity of the "life-worlds" of the legislators under scrutiny.

CHARLES M. TIDMARCH
Union College

American Criminal Justice: The Defendant's Perspective. By Jonathan D. Casper. (Englèwood Cliffs, N.J.: Prentice-Hall, Inc., 1972. Pp. 175. $5.95.)

Crime and Justice in American Society. Edited by Jack D. Douglas. (Indianapolis, Ind.: The Bobbs-Merrill Company, Inc., 1971. Pp. 297. $7.50.)

Crime, Dissent, and the Attorney General: The Justice Department in the 1960's. By John T. Elliff. (Beverly Hills, Calif.: Sage Publications, 1971. Pp. 276. $10.00.)

Research in the field of the administration of criminal justice has hardly struck out in original directions; indeed, the old staples of plea bargaining, civil rights, deterrence, and drugs have continued to form the bulk of the writing. The three books reviewed here, however, attempt in different ways to carve out newer approaches.

Perhaps the most original approach is that of Professor Jonathan D. Casper, who, in *American Criminal Justice: The Defendant's Perspective*, examines criminal justice from the perspective of the defendant, rather than that of the judge, prosecutor, defense lawyer, or neutral observer. His point of view—that of the "consumer" of criminal justice—has largely been ignored.

Professor Casper is appropriately modest about the significance of his research, which is limited to taped interviews with 71 defendants charged with committing felonies in the State of Connecticut. Of these, 49 were inmates of Connecticut correctional institutions, 16 were on probation, and 6 had received dismissals or acquittals. The men in prison were selected on a random basis, whereas those on the outside were self-selected—they responded to a solicitation letter. Although the sample is quite skewed—it is limited to one jurisdiction and by and large to those who were ultimately convicted—the defendants' attitudes and perceptions, according to Professor Casper, suggest lines for further research and dispel some of the common myths about the typical defendant as "crazed killer or mugger or the glassy-eyed junkie" (p. xiii).

If these defendants are typical, what they learned from their jail experience is what they knew before. They see the system "as an extension of their life on the street."

> . . . The criminal justice system like the streets is a game of resource exploitation. The defendant typically has little in the way of resources and doesn't win. He can, though, with luck and skill, lose less than he might. In this way, I want to argue, the system has no moral component in the eyes of the defendant. . . . If the policeman's job is to catch criminals and the prosecutor's job is to convict them, this is about the same as saying that the defendant's job is to break and enter. All are, in the defendant's eyes, about equally immoral (p. 18).

Crime and Justice in American Society, edited by Professor Jack D. Douglas, is an acerbic response "to the alarming and fundamental inadequacies" of the report of the President's Commission on Law Enforcement and the Administration of Justice.

The introduction and the eight essays in the book center on three controversial themes. The first is that "the public outcry over 'crime in the streets' is a form of hysteria which distorts realities and, which, consequently must not become the basis for any national policies pertaining to the fundamental issues of crime and justice in our society."

The second theme is that "laws, law enforcement, crime and justice are largely determined by

political decisions" (p. xviii). Mr. Douglas argues that criminal laws are enacted by the middle and upper classes to place the poorer classes under the more direct control of the police, while the middle and upper classes pass only civil laws to control violations within their own ranks.

The final theme is that the assumption on which law enforcement procedures and judicial practices proceed is no longer valid—if it ever was. The basic assumption of these essays is that our laws and legal practices, not the social imbalances of our society, are causing our most severe problems of crime and justice. This assumption forms the basis of their attack on the view of the President's Commission that the challenge of crime in a free society can be met only by a socioeconomic reordering of our society.

The now-controversial office of the attorney general, which until recently has been largely ignored as an area for research, is the subject of Professor John T. Elliff's *Crime, Dissent, and the Attorney General*, the first book in a series under the general editorship of Professor Joel B. Grossman of the University of Wisconsin. This series is designed to explore the policy-making role and functions of courts and other agencies concerned with law in the political system.

Based on extensive interviews with past and present officials in the Department of Justice and access to some records, this book describes the forces that led the Department of Justice to assume a larger role in the criminal justice process than ever before. Professor Elliff compares and contrasts the roles of the attorneys general Robert Kennedy, Nicholas de B. Katzenbach, Ramsey Clark, and John Mitchell, focusing on Clark and Mitchell, much to the disadvantage of the latter.

One chapter devoted to Domestic Intelligence Surveillance anticipates the revelations of Watergate. During the 1960s the Justice Department supplemented the FBI's activities by creating its own computerized Interdivisional Intelligence and Information Unit, originally designed to anticipate civil disorder. The growth of the activities of the domestic intelligence-gathering efforts was largely unknown by political scientists and, as reported in this book, had somehow escaped the attention of most of the members of Congress.

This intelligence gathering occurred without the benefit of clear administrative, legislative, or judicial guidelines. Professor Elliff explores the uses of electronic surveillance and covert infiltration, and their possibly chilling effects on freedom of speech. He concludes that "if political liberty is to be firmly established in the long run, the Justice Department must give up its functions under statutes that proscribe essentially political offenses" (p. 255).

Well written and convincingly documented, this book lives up to its goal of setting the Department of Justice at the center of the legal policy-making process.

RUTH G. WEINTRAUB
Academy for Educational Development, N.Y.

The War Industries Board: Business-Government Relations During World War I. By Robert D. Cuff. (Baltimore: The Johns Hopkins University Press, 1973. Pp. 304. $13.50.)

Robert Cuff's detailed and imaginative history of the War Industries Board uncovers the political anatomy of an important experiment in government economic controls. While the book suffers somewhat from lack of breadth and comparative perspective, it provides many valuable insights for scholars with a general interest in administration, large-scale regulation and planning, and business-government relations.

The early chapters document the dominant role of business interests in seeking government regulation when the marketplace is disrupted. Wartime mobilization had created production bottlenecks, shortages of military supplies and basic commodities, inflation, and an uncoordinated scramble among military purchasing bureaus and private firms. The federal government's initial stabilization efforts were dominated by industry-staffed advisory committees, which pushed a reluctant administration and Congress for legal powers to enforce their plans. As reorganized in 1918 under Bernard Baruch, the War Industries Board (WIB) came to be seen as one of the greatest concentrations of government power in American history: fixing prices, dictating priorities in production and shipment, and generally establishing the prototype for a governmentally planned and integrated economy.

The heart of Cuff's book re-examines the extent and application of WIB's power. The supposed paragon of centralized control, he concludes, was in fact disorganized, weak, and often ineffective. Its authority was frequently circumvented by recalcitrant military bureaus and businesses. The agency was deeply dependent on the voluntary cooperation of industry (and deferential to its interests) in setting prices and administering priorities. This was partly due, Cuff holds, to WIB's structural weaknesses. It lacked legal authority over the military and business; inadequate administrative resources induced dependence on industry for staff, knowledge and implementation; consequently it was fragmented into decentralized committees, oriented to particularistic needs of specific industries. Yet WIB's lack of centralized coercive power, Cuff argues, was entirely in accord with the motives of WIB's business man-administrators, who hoped to demonstrate that the nation's needs could be met by voluntary cooperation among public-spirited industrialists, and who believed that the mobilization effort should pre-

serve the fundamental structure of American business. Therefore WIB leaders were ambivalent in pressing for greater legal authority (although it is not clear they could have gained it), and were content to rely on moral suasion and "regulations" that merely placed a governmental imprimatur on agreements engineered by dominant firms in each industry.

This is revealing scholarship, more balanced than most "revisionist" histories of the Progressive era. But one is not entirely persuaded how far WIB's organizational weaknesses actually rendered it ineffective. Cuff's emphasis on the *process* of regulation rather than its *consequences* leaves the reader with no external indicia of WIB's impact and no systematic discussion of wartime production, changes in business profits, or inflation. (In fact, inflation abated during 1918, especially for basic metals subject to WIB agreements.) Ironically, Cuff's diligence in probing beyond official reports to in-house memoranda and letters of top WIB officials may account for an apparent overemphasis on agency failings and disregard of successes: such high-level communications are likely to stress problems and crises, rather than ongoing regulatory activities handled routinely and effectively by lower-level officials. Moreover, WIB's performance is often implicitly measured against an impossible standard—the highly integrated, powerful, and uncompromising agency pictured in its own press releases. The resulting implication, surely unwarranted, is that when a WIB regulation was bargained-out with industrialists the agency had failed, that what was good for General Motors was *necessarily* bad for the country.

An inherent weakness of the single case study is underlined by Cuff's attribution of WIB's problems to its organizational structure and voluntarist ideology. What has been the fate of comparable regulatory efforts during the Depression, World War II, in post-war Europe, and in the Nixon administration, where the government agency *had* what WIB lacked—clearcut legal powers, greater administrative resources, more balanced leadership? A comparative analysis, one suspects, would reveal that WIB-like decentralization and particularism, bargained-out regulations, concessions to the economic stability of regulated industries, all develop in other settings, and stem from the functional dynamics of modern industrial organization rather than the legal, organizational, or ideological characteristics of the regulatory agencies. Indeed, the most valuable aspect of Cuff's study is its illumination of *generalized* regulatory processes: control by normative appeal and threat of adverse publicity; reliance on businessmen for advice, information and implementation; pursuit of agreement rather than legal coercion. In this regard the story of WIB enhances the literature of institutional studies on which a theory of governmental economic planning and regulation might be based.

ROBERT A. KAGAN
Yale Law School

The Desirability of a Correctional Ombudsman. By Timothy L. Fitzharris. (Berkeley: Institute of Governmental Studies, University of California, 1973. Pp. 114. $3.00.)

The author advocates the establishment of an ombudsman specializing in complaints arising from the state corrections system. The California Legislature had passed a bill to set up such an institution in 1971, but it was vetoed by the governor. While a second attempt is presently being made in California, Minnesota established a correctional ombudsman in July 1972. The Minnesota correctional ombudsman differs from the proposed California version in one important respect: he is responsible to the governor, not the legislature.

Fitzharris examines the adequacy of existing grievance procedures available to inmates of the California state prison system. In his view the courts are over-burdened, slow, and reluctant to intervene in what they consider a responsibility of the executive branch. Most complaints are handled at the administrative level, but the author finds serious weaknesses here: the willingness and capability to resolve problems varies greatly from place to place, and a large proportion of prisoner complaints are made against the institution itself or its staff, and—not surprisingly—prisoners mistrust staff investigating themselves.

California law provides a third type of complaint channel: the right of prisoners to appeal to top executive-branch administrators and to members of the legislature. The author shows convincingly, however, that the governor and his staff, as well as legislators, do not have the time, expertise, and inclination to carry out their own investigation; invariably the agency about which the prisoner complained is asked to suggest an appropriate reply. These political channels of complaining therefore do not add significantly to the existing (and deficient) administrative complaint channels.

The author discusses the most important structural characteristics required for maximizing the benefits to be derived from a correctional ombudsman—such as his independence, his accessibility, the investigatory powers he needs, and the expertise required of an incumbent and his staff, as well as his jurisdiction and his powers to recommend and to make his findings public. The discussion emphasizes all essential characteristics of the institution of ombudsman, and his pro-

posals are sound. The appended version of the California bill can be considered a good model for this type of institution.

The author has high hopes about the benefits that could be derived from the proposed office. He expects the ombudsman to stimulate communication and flow of information in prisons. In a more general way Fitzharris expects the ombudsman will improve the quality of prison life and prison administration. To an extensive list of "advantages of a Correctional Ombudsman" the author adds a small cautionary note: the institution of ombudsman should not be considered a panacea or a kind of structural penal reform (p. 70); yet he immediately adds that he considers his proposal a "giant step along the path of cooperation between the citizenry, penology professionals, and prisoners towards the desirable goals: prisoner rehabilitation, social reintegration and achievement of positive citizenship." Other detailed studies, however, should warn us against exaggerating the probable impact of ombudsmen on bureaucracies (e.g., L. B. Hill on New Zealand).

As an advocate of the correctional ombudsman, the author fails to examine thoroughly the question why one should prefer a correctional ombudsman over a general (state) ombudsman for all citizen grievances. He suggests that his proposal is a reform project in its own right and will more than pay for itself (for example, by eliminating troubles that might lead to riots). He also implies that since the state of California is too large for one ombudsman to handle all areas of complaint, it will require specialization. S. V. Anderson suggests in the Preface that contributions of general ombudsmen to the field of corrections have been worthwhile but modest. Even if all these arguments could be substantiated (and no serious attempt is made in the book) one cannot escape the conclusion that one ought to try a general ombudsman first before specializing. This proposal for specialists is partly the result of the increasing frustration which advocates of this humanizing institution are experiencing in the face of a remarkable skill of American politicians at preventing innovation in this field.

KARL A. FRIEDMANN
The University of Calgary, Canada

The Fight for Competitive Advantage: A Study of the United States Domestic Air Trunk Carriers. By William E. Fruhan. (Boston: Division of Research, Graduate School of Business Administration, Harvard University, 1972. Pp. 183. $9.00.)

Compared with the airlines of other countries or with other regulated industries within the United States, the U.S. airlines have met the needs of consumers fairly well. Yet the industry and its regulator, the Civil Aeronautics Board, have had more than their share of critical analysis. While Professor Fruhan is not violently critical, he still ascribes much of the industry's chronic problem of overcapacity to policies and decisions of the Civil Aeronautics Board.

Using regression techniques, Mr. Fruhan demonstrates that nearly all the factors which determine the relative profitability of an airline are beyond the control of the airline's management. Route structure, competition on specific city pair routes, and fare level and structure are all controlled principally by the Civil Aeronautics Board. Of the two key factors within management's control, Mr. Fruhan through his regression analyses minimizes the importance of aircraft type selection, leaving schedule frequency as the only quantifiable and important factor available to management to gain competitive advantage.

According to Mr. Fruhan, the airline which can gain a strong share of the market will gain a more than proportional share of the profits available in that market. Mr. Fruhan then asserts that the key to an increased share of the market is to increase scheduled flight frequency. Obviously, this results in competitive airlines steadily increasing flight frequency, in reduced number of passengers per flight and in financial crisis for the airlines, including risk of bankruptcy for the weaker carriers.

The Civil Aeronautics Board abets the process by bailing out the industry through fare increases when a financial crisis occurs. This prevents the process from becoming self-limiting as a result of financing problems.

Most observers will agree that overcapacity is the airlines' biggest problem. Many, however, will be less certain of the causation chain which Mr. Fruhan outlines. Mr. Fruhan does not admit to the re-equipment cycle as a cause of overcapacity at all, and includes the business cycle as an indirect cause only through its impact on financial availability. Yet the change of gauge involved in shifting from 30 passenger DC 3's to 100 passenger jets on the same route and then later shifting to 300 passenger wide body aircraft without reducing passenger schedule availability would seem to be an important cause of overcapacity. Unfortunately, Mr. Fruhan does not discuss alternate possibilities. Instead he asserts that his own causation chain is correct without proof other than that timing of the periods of overcapacity fits his theory.

There are some more specific problems with the book as well. Mr. Fruhan's statistical treatments are incompletely documented in the text, but some of his results present uncomfortably high R^2 results for a complex problem in the social sciences. They may be legitimite, but they lead to

suspicions of problems with the technique. Further, there is an important inconsistency in that Mr. Fruhan asserts early in the text the "CAB's greater concern with the level of industry profits rather than individual carrier profits", yet makes individual carrier profit problems (the weakest carrier) the principal reason for the CAB's willingness to legitimize overbuys of aircraft through fare increases. This is his major thesis.

Recurrent overcapacity problems are of serious concern in the airline industry, and could easily become the reason for future drastic policy shifts by the government's regulators. Mr. Fruhan suggests an explanation for the problem, and discusses possible attacks on it. Although his explanation of the causation chain for the problem is different from other explanations, his recommendations are not at variance with other proposals.

ROBERT L. THORNTON
Florida State University

U.S. Congressional Directories, 1798–1840. Edited by Perry M. Goldman and James S. Young. (New York: Columbia University Press, 1973. Pp. 417. $20.00.)

Every scholarly library will want to hold this collection of basic documents on the organization of the U.S. Congress for its first half-century. For some early Congresses, members are reported according to their place of Washington residence, in most instances a boarding house. Co-editor Young has already demonstrated (in his earlier book *The Washington Community 1800–1828*) how significant these groups were for Congressional outcomes.

Much information on early committee structure is now also made available in this compendium. This sort of data is incredibly hard to come by, and for students of early congressional history, and U.S. political history more generally, this volume comes as a godsend.

There is a short, informative introductory essay by the editors on the possible uses of data from the directories, and an account of their provenance.

NELSON W. POLSBY
University of California, Berkeley

Education, Training, and the Urban Ghetto. By Bennett Harrison. (Baltimore and London: The Johns Hopkins University Press, 1972. Pp. 267. $10.00.)

Many political scientists—especially urbanologists and education, welfare, and manpower policy analysts—will want to read this book based on the author's prize-winning economics dissertation (The William Polk Carey Prize in Economics at the University of Pennsylvania). After examining the race, sex, education, training, individual earnings, employment stability, and occupational status of more than 50,000 persons in 16 metropolitan areas, Professor Harrison comes to the conclusion that the only way to improve life in American cities is to increase the income of urban minorities by remaking our economy. He argues that the quality of urban life cannot be enhanced without a reduction in crime, that crime cannot be reduced without drug control, and drug control cannot succeed without "massive job development."

Harrison's major thesis is that the economic conditions of urban minorities in general and the unemployment of nonwhites in particular cannot be improved by such things as more years of schooling, training programs, reverse commuting, or ghetto dispersal. He says, "Attempts to change the worker himself—whether to remedy his personal 'defects,' or to move him to a 'better' environment—have not worked in the past, and there is no evidence that they will work in the future" (p. 171). Instead, he offers two reasons why income levels of blacks and whites may differ: (1) market imperfections, e.g., monopoly or discrimination, and (2) a fundamental segmentation of the labor force according to class, race, and sex.

From his analysis of data collected in 1966 and 1967 by the Department of Labor and the Bureau of the Census, Harrison rejects the proposition that income differences between races can be attributed to real differences in productivity related to their education. He devotes considerable attention to showing that, in the present economic situation, there is little pecuniary incentive for nonwhite ghetto dwellers to increase their human capital by investing in education:

> The present value of the lifetime return to completion of four full years of high school is nearly $19,000 for whites, as compared with about $6,000 for nonwhites. . . . For whites, the risk of unemployment falls with years of school completed. . . . For nonwhites. . . the average effect of education on employment . . . is virtually zero. . . . Education helps members of both races to move into what are considered to be more prestigious occupations. But, once there, the nonwhites are systematically underemployed, receiving earnings which are hardly above the levels enjoyed in the previous position, and facing almost exactly the same expectations of unemployment as before. For ghetto whites . . . the occupational mobility . . . is translated into substantially higher earnings and significantly lower risks of joblessness (p. 68).

Moreover, he points out that the unfounded conventional wisdom which says that a person with more education will earn more money and attain higher status, may raise expectations that cannot be realized and, therefore, could result in serious frustrations.

Using Bluestone's concept of the segmented economy ("The Tripartite Economy: Labor Markets and the Working Poor," *Poverty and Human Resources*, July/August 1970), Harrison concludes that education does not provide a vehicle for mobility between the periphery of the economy (the welfare sector, the "hustle," the training sector, the secondary labor market) and its core. Furthermore, he claims training programs merely "recirculate the working poor among the segments of the periphery economy" (p. 148). His solution to improving inner-city life is not to affect the supply of ghetto labor, but to increase the demand for it.

Harrison refers to *Griggs* v. *Duke Power Co.*, 91 U.S. 849 (1971), as what may well be a landmark decision in reducing the market imperfection of discrimination disguised as over-credentialing, or requiring educational standards unrelated to successful job performance. Harrison endorses the Philadelphia Plan and Affirmative Action, but he makes it clear that even good-faith efforts to hire the hard-core unemployed in private industry will not "make a serious dent in the problem" (p. 178).

Thus he calls for the federal government, by supporting new towns in-town and community development corporations, to expand its delivery of desperately needed urban public services and to create millions of new jobs, pulling workers out of the periphery into the core of the economy. He regrets that only 0.3 per cent of the federal budget was spent on job development in 1969 and sees his proposal increasing wages in the ghetto by a factor of one to three.

More income may be a highly desirable, and even necessary, condition for better life in the inner city, but is it sufficient? Harrison's regression analyses do not show how economic variables affect political, sociological, and psychological ones; on the contrary, his causal path runs in the opposite direction. His assumption that more jobs mean better lives for urban residents may be similar to the assumption held by many, until challenged by the Coleman Report (*Equality of Educational Opportunity*, 1966), that more money spent on schools results in higher pupil achievement. Andrew F. Brimmer and Kenneth B. Clark remind us of the critical importance of other dimensions. Brimmer says, "Although a computer programmer may earn as much as (or more than) a high school principal, he clearly has less weight in the community's affairs" (in Andrew F. Billingsley, *Black Families in White America*, 1968, p. 174). And Clark emphasizes ownership, control, management, and independence as necessary ingredients of self-respect and self-worth (*ibid.*, pp. 168–169). Will public service jobs, even if developed to the extent recommended by Harrison, provide sufficient political clout and the human dignity to improve the lives of those who reside in the urban ghetto? I doubt it. Harrison's critical analysis of the modest economic impact of minority training programs is superior to his prescription for increasing the political power of ghetto residents.

JeDon A. Emenhiser
Utah State University

Marihuana Users and Drug Subcultures. By Bruce D. Johnson. (New York: John Wiley & Sons, 1973. Pp. 290. $12.95.)

Drug Use in America: Problem in Perspective. Second Report of the National Commission on Marihuana and Drug Abuse (Washington: U.S. Government Printing Office, 1973. Pp. 481. $2.25.).

In America the drugs which are thought to be the most devastating in their effects are the opiates. Opiate dependency is a threat both to the user and to the fabric of society. A rational strategy to deal with this and other drug problems, including marihuana use, should begin with a systematic effort to acquire information. In recognition of the fact that drug use in all its varieties, including the legal, semilegal, and illegal, needs far more investigation than it has received, Congress in 1970 provided for the creation of a Commission on Marihuana and Drug Abuse.

In its recently published report, *Drug Use in America: Problem in Perspective*, the Commission indicates that public discussion of the drug phenomenon should include consideration of the moral, philosophical, historical, and political implications of drug use. Public policies and decisions should derive from these considerations. The Commission goes on to note, however, that not once in almost seven decades of policy making on the subject have such underlying assumptions been systematically evaluated and a broad, coherent foundation for policy making been established. Long-term strategies designed to minimize irresponsible drug use have not been pursued. The Commission has sought to formulate a policy-making process that includes all of the important variables and that disentangles various crucial issues.

The Commission's recommendations can be summarized as follows: American society should continue to discourage any drug use that occurs outside of medical channels or established social patterns. The primary concern of drug policy should be with disruptive or dysfunctional drug use. Drug policy should not be limited to illegal drug taking but should include dysfunctional use of legal substances, including alcohol and barbiturate drugs. Law enforcement can reduce but

not eliminate the illegal supply of drugs. Elimination of the illegal market can occur only when a vigorous enforcement of the drug laws is joined with an effective program of reducing demand for drugs. The Commission further recommends that all drug-control functions of the federal government be consolidated in the hands of one agency—a recommendation which has since been partially realized by the creation of the Drug Enforcement Administration. Finally, the Commission cautions that the barbiturate drugs of today have become the functional equivalent of the freely available opiate drugs of the turn of the century. They constitute "America's hidden drug problem."

The final report of the Commission on Marihuana and Drug Abuse is to be praised for several reasons. To begin with, it correctly points out that public policy must reflect the complexity of drug-using behavior, including differences in the drugs themselves. The tendency to regard drug use as a unitary phenomenon has resulted in confusion and ineffective individual and governmental action. There is no one cause of drug use; rather, the phenomenon results from a complex interweaving of sociological and psychological factors.

The Commission report correctly indicates that crop eradication programs will not eliminate the availability of opium, cocaine, or marihuana. The United States by means of such programs may reduce the supply of drugs entering the international illicit market, and, therefore, reduce the supply entering this country. Total elimination of opium, cocaine, or marihuana cultivation, however, is impossible. It should be remembered that there are compelling political reasons for the government's insistence on a crop eradication program. It is difficult to imagine a president launching a serious effort to constrain heroin use while ignoring the foreign cultivation of opium as well as the foreign manufacture and importation of heroin.

The Commission's final report can be criticized for contending that the present system of drug control should be retained, with adjustments, because the present controls result in a net benefit for society; i.e., they result in a reduction in the social cost of drug use that outweighs the cost of control itself. This is debatable. Efforts at the social control of drugs, including reducing the supply, increasing the social or economic cost, and instituting stiffer penalties for use do have an impact on use patterns but they may well result in far more serious secondary social maladies. The Commission itself seems to recognize this possibility by stating in another context that public policy "as presently designed, is premised on incorrect assumptions, is aimed at the wrong targets, and is too often unresponsive to human needs and aspirations" (p. 8).

The Commission recommends that the government continue to prohibit heroin maintenance as a treatment modality. It can be argued, however, that there are sound reasons for trying heroin maintenance on an experimental basis. An argument in its favor is that heroin maintenance might attract two important groups of opiate users: (1) those who do not succeed on methadone maintenance, and (2) those who do not initially volunteer for methadone maintenance. If heroin maintenance were instituted, addiction-related crime would surely decrease because the costs of the heroin habit would be eliminated. Moreover, the lessened chances of infection, overdose, and imprisonment would represent an important benefit to the addict. It is also possible that heroin maintenance would remove the impetus for market expansion caused by the high profits of illegal distribution.

Many commentators on the American drug scene, including the authors of the Commission's final report, are convinced that the laws which govern drug-using behavior in the United States have important consequences for the prevailing patterns of drug use. Bruce Johnson's study, *Marihuana Users and Drug Subcultures*, is based upon 3,500 questionnaires sent to students and friends of students at 21 colleges located predominantly in the New York City area. These questionnaires sought to explore patterns of drug-using behavior through the use of subculture theory. Each chapter of the book employs empirical data to demonstrate what the consequences of the present drug laws are on drug-using behavior.

The theme of Johnson's book is that involvement in cannabis selling, and not the use of marihuana, is the principal factor which leads to increased participation in subcultures of drug use. It is participation in drug subcultures which explains why marihuana is linked with various unconventional activities. Johnson believes that the present drug laws in the United States "play a fundamental role in structuring and organizing subcultures of drug use" (p. 205).

The relationship between cannabis and the use of more potent drugs is another major aspect discussed in the book. Johnson concludes that there is nothing to indicate that the use of marihuana is a causal factor responsible for the use of more dangerous substances. Rather, the social needs of the individual must be examined in order to understand why some individuals do use other drugs. According to Johnson, it is the sale of cannabis which facilitates association with subcultures that are involved with dangerous drugs. Transition to the use of such drugs would be a

consequence of this association rather than an inherent effect of cannabis. After the individual has become a seller of cannabis and has become immersed in a drug subculture, the drugs which he chooses to experiment with are determined primarily by socioeconomic and sociocultural factors. The factor of being a seller of cannabis is important in determining the degree of a person's involvement with and commitment to the use of other drugs.

Johnson believes that the present drug laws, which define marihuana use as illegal, are an important element in holding drug subcultures together. Legalizing cannabis would, he believes, undermine both the illicit drug market and the social processes by which students are recruited into, and maintained in, drug subcultures. The first report of the Commission on Marihuana and Drug Abuse called for a decriminalization of marihuana use. This recommendation recognized, as does Johnson, that there are secondary social maladies that can result from the implementation of archaic drug laws.

The final report of the Commission on Marihuana and Drug Abuse, as well as Johnson's book, should be of interest to everyone concerned with finding solutions to America's multifaceted and complex drug problems.

JOSEPH L. ZENTNER
University of Southwestern Louisiana

People of Paradox: An Inquiry Concerning the Origins of American Civilization. By Michael Kammen. (New York: Alfred A. Knopf, 1972. Pp. xvii, 316. $8.95.)

This ambitious volume represents still another quest for the American national character or, as the author prefers, "style." Unlike most earlier similar undertakings, however, it is concerned less with questions of definition than with the problem of origins, and all but two brief concluding chapters are devoted to a discussion of seventeenth- and eighteenth-century developments. Erik Erikson's proposition, in *Childhood and Society*, "that a nation's identity is derived from the ways in which history has, as it were, counterpointed certain opposite potentialities" (p. 97), supplies a ready-made frame of reference, while the central argument, that Americans have from the beginning been "uniquely a people of paradox" (p. 106), repesents an elaboration of Erikson's further suggestion that the United States has subjected "its inhabitants to more extreme contrasts and abrupt changes" than most "other great nations" (p. 97).

Why Americans "have been unusually and profoundly perplexed by ambivalence and contradictory pulls" (p. 107), may be traced, according to the author primarily to several related conditions that, he suggests, distinguished the English colonies from those of other European nations in America. First, the English colonies were "offshoots of a deeply divided society" (p. 139) that was "peculiarly racked by tensions and contradictory tendencies" (p. 113) that were inevitably transported to America. Most of the contradictions had been resolved in the parent state by the early eighteenth century, but two powerful conditions combined to give them continuing force in America. First was the problem of establishing the legitimacy of institutions, patterns of belief, and standards of behavior in all areas of colonial life, a problem that presumably was greater in the English colonies because of looser controls exerted by the metropolis and a more rapid pace of social change. Second, and much the most important stimulant of ambiguity and conflict, was the "bewildering, fluctuating pluralism" (p. 94) that, increasingly during the eighteenth century, resulted from the growing demographic, cultural, and economic diversity of the colonies. By exacerbating longstanding dualisms and spawning a welter of new biformities, polarities, and paradoxes, this diversity, the author argues, had made colonial society profoundly unstable by the eve of the Revolution. Subsequently, the Revolution created as many incongruities as it helped to resolve, and over the next century "antimonies inherited from colonial times endured" while "a new garden of dualisms blossomed," as disengagement from Europe permitted the American "penchant for ambi-tendencies to continue apace" (p. 250).

This series of hypotheses is engagingly set forth and interlaced with a number of valuable and original insights about specific aspects of early American cultural development. The case made for them is not, however, a compelling one. The author leads the reader so hurriedly past so many biformities and paradoxes that one puts the book down without any certain sense of how all of these illustrations, many of them quite cursorily explored, fit together or what they might mean. More important, the author's reading of the tensions and contradictions does not always help to dispell the doubts created by these occasional failures of elaboration and analysis. Thus, his assertion that the colonies "became *less* stable with the passage of decades" (p. 63), so crucial to his argument, may in some ways be true for some colonies but is clearly not true for most; and it rests on no firm data base (indeed, one has yet to be systematically put together) but only on the general assumptions that diversity is usually productive of social tensions which in turn result in social instability—an assumption, of course, that must be tested in every specific situation. For the colonies, an equally strong case could be made

for precisely the contrary view: that, in the aggregate, tensions arising out of diversity were usually not dysfunctional and that, to borrow a happy characterization from the author, the "invertebrate" character of society helped to provide a functional release from anxieties and frustrations generated by the constrictions of tradition and social space that continued to exert a powerful influence in the more settled and less open societies of western Europe. By pushing the colonists toward compromise and hybridization, moreover, pluralism may well have helped them to learn, perhaps much more quickly and with less emotional pain than their European contemporaries, to cope with the vast social changes generated by the economic and social expansion of the early modern western world and thus to become a peculiar source not of weakness but of strength.

This is not to suggest that the colonists fashioned a homogeneous utopia out of such diverse materials but only that they did produce a society with a high degree of stability and a remarkable capacity for adapting to change, and that the existence of so much diversity could only have accelerated the process. In his conclusion, the author suggests that over the last century the counterposition of the many contradictory tendencies within American society has given it "the ultimate stability of an arch" (p. 297). But that achievement, like most of the tensions which underlay it, was already vividly manifest during the last years of the colonial period and the era of the American Revolution.

JACK P. GREENE
The Johns Hopkins University

Essays on the American Revolution. Edited by Stephen G. Kurtz and James H. Hutson. (Chapel Hill, N.C.: The University of North Carolina Press, 1973. Pp. 320. $12.50.)

Bernard Bailyn, fittingly accorded first place in this excellent collection of essays by eight historians, begins his essay with this true statement: "The American Revolution not only created the American political nation but molded permanent characteristics of the culture that would develop within it" (p. 3). This book demonstrates that those from whom we can learn about this crucial political event are historians not political scientists.

The first and last essays, by Bailyn and Edmund S. Morgan, deal with the broad questions of the causes of the Revolution and its consequences. The other six essays are by Jack P. Greene, Richard M. Brown, John Shy, H. James Henderson, William G. McLoughlin, Rowland Berthoff and John M. Murrin, and deal with more specific aspects of the Revolution. No attempt is made to be comprehensive, nor does a single, consistent interpretation emerge. But the essays do speak to each other, though not overtly or argumentatively, so the book is more than a collection of essays related only by an overarching theme.

To demonstrate how this exchange takes place (and since it is clearly impossible to do justice to the essays in any summary fashion), let me examine one theme announced by Bailyn that runs through several of the essays: the question of the role of ideas in the Revolution, "how ideas and beliefs relate to what men do" (p. 5). Bailyn believes that particular ideas understood in a particular way "may be understood to have lain at the heart of the Revolutionary outbreak and to have shaped its outcome and consequences" (pp. 11–12). These ideas are not so much the "noble ideas of the Enlightenment and the abstracted details of constitutional law" but an "elaborate pattern of middle level beliefs and ideas" that came to have an "instrumental grasp on [the colonists'] minds" (p. 8). In particular, this pattern of ideas derives from Great Britain, from "the voluminous writings of the Exclusion Crisis" and "the literature of the Glorious Revolution." Bailyn is reluctant to point to Locke as the fountainhead of these ideas and instead emphasizes the importance of "the writings of the early eighteenth-century" that opposed the government of Walpole (p. 8). These ideas "would most specifically determine the outbreak and character of the American Revolution . . . " (p. 9). Bailyn does not see ideas as "epiphenomenal, superstructural, not the shapers of events but their rationalizations . . . " (pp. 10–11). The ideas and beliefs of the colonists *did* shape their response to British "acts of power," and under the pressure of events these ideas were refined and made more coherent and consistent. The final result of this process of distillation is the crucial theoretical passage of the Declaration of Independence, which begins with the announcement of the self-evident truth of man's equality.

Jack P. Greene also speaks of "a hard core of unchallenged beliefs . . . [that] formed the central premises for an emerging colonial perceptual system" (p. 53–54). Greene correctly if cautiously suggests that "it is probably somewhat misleading" for Bailyn to argue that there is a sharp distinction between the ideas of the Glorious Revolution and those of the opponents of Walpole: "[the latter are] really only a more pure, intense, and uncompromising version of the same central ideas . . . " (p. 53). The line of descent may at times be unclear, but the Declaration of Independence is nonetheless the offspring of Locke (and the political works of Locke provide the most adequate gloss on the Declaration and on that pattern of ideas and beliefs found in the pamphlets of the pre-Revolutionary period).

Greene, however, finds the key elements of the "basic substructure of [colonial] expectations" not in the colonists' speech but in their deeds. In their "actions" and "behavior," Greene finds a radical individualism which sees political society as protective of the individual and his interests but also as "facilitative and positive in that it encourages . . . the individual to pre-occupy himself with his own goals without forcing him to be much concerned with . . . the community as a whole" (p. 57). According to Greene, the colonists do not speak their views because of the public unacceptability of such a position. But of course, the position had been publicly stated by John Locke, although stated in such a way as to seem acceptable.

Greene locates another "elusive component of this implicit structure of expectations" (p. 59). The good man is the automonous man. "Wilderness conditions . . . had early forced men to rely heavily upon their own resources. Mastery of environment . . . had to an unusual degree required mastery of self . . . " (pp. 59–60). Virtue is therefore defined as mastery of "one's own passions," a means for the mastery of nature. Although Greene does not draw the connection, virtue so understood was not only a necessity of "wilderness conditions" but a part of Calvinist teaching. As William G. McLoughlin shows in his essay, "The Role of Religion in America," even after the Revolution, "Americans were clearly committed to the establishment of a Protestant Christian nation" for "they were convinced—all Americans were convinced—that the safety and prosperity of the nation depended upon inculcating into its citizens the fear of God and the love of one's neighbor" (pp. 209, 210).

These are the central elements of what George Grant has called "the American primal": the new political and moral science expounded by Locke and distilled in the Declaration, the new physical science by which man will master nature, and that Protestantism which embraced both the new political and physical science and provided the needed support for man's mastery over himself (virtue). McLoughlin is aware of "the scientific challenge to the heart of Protestant faith" (p. 254) but just as serious a challenge was posed by the new political science. As he says, "Hosts of people simply refused to believe any longer that the duly constituted authorities in church and state knew best what was right for them . . . " (p. 201). But as Tocqueville knew, it is only one step from the doubt of human authority (because all men are equal) to the doubt of all authority, human or divine. It is no accident that the supreme doubter, Descartes, a founder of modern science, is the first philosopher to proclaim in 1637 the equality of man. A similar pronouncement is made fourteen years later by the founder of modern liberalism, Hobbes.

Edmund S. Morgan concludes the final essay by speaking of America's commitment "to the creed of equality," a commitment "that invited conflicts and still invites them . . . " (pp. 308, 309). Morgan is correct, but the conflicts he envisions are between those seeking "their right to equal treatment: and those blocking such access." The more profound conflicts inhere within "the creed of equality" itself. What are the imperatives of that creed? Berthoff and Murrin in their essay force us to consider one problem inherent in the creed of equality: is the goal only "political parity" or is it also "the social equality of persons" (pp. 282–285)? That problem is still with us and constitutes the most important debate taking place in America today. Some Americans have "face[d] the possibility that their liberty —their freedom to compete—was undermining their equality" (p. 283). To be "truly equalitarian" it may be necessary to be less committed to one of the rights that all men share equally, the right of liberty.

The Revolutionary period is of penultimate importance because the creed of equality which is both America's glory and her fate was formulated then. These essays provide an excellent introduction to our understanding of that glory and fate.

RICHARD CROSBY
Colgate University

The Rise and Fall of the People's Century: Henry Agard Wallace and American Liberalism, 1941–1948. By Norman Markowitz. (New York: The Free Press, 1973. Pp. 416. $8.95.)

This book is neither a comprehensive biography of Henry A. Wallace nor a satisfactory explanation of the rise and fall of an imaginary people's century. With only a brief survey of Wallace's life and career prior to 1941, which synthesizes superficially what others have covered more thoroughly, Professor Markowitz concentrates upon those years the Iowa agrarian served as vice-president, secretary of commerce, editor of the New Republic, and presidential candidate of the Progressive Party. In the context of Henry Wallace's total public career, this period constitutes his less than glorious years.

In the realm of agriculture Henry Wallace was an expert with few peers. Knowledgeable in matters of farm policy, and an adept phrase maker, he could transform his political rhetoric into practical reality. Abstruse terms such as "ever normal granary," "economic democracy," and "social discipline" were given concrete meaning while he was secretary of agriculture. The first was implemented via the storage of food and fiber by the Commodity Credit Corporation; the

second was achieved to a degree by the county committee system of the Agricultural Adjustment Administration; and the third succeeded by using the acreage allotment-price support mechanism to demonstrate to farmers the collective advantage of limiting their individual decision making in favor of considerable federal management of agriculture.

It was another story when it came to matters of foreign policy. In this area Henry Wallace could not transform clever words into meaningful programs. His catchy phrase "the century of the common man" was a slogan without substance. Attainment of this abstract ideal was as elusive as Franklin D. Roosevelt's "Four Freedoms"; Wendell Willkie's "one world"; or Woodrow Wilson's world made "safe for democracy." To achieve a modicum of international stability, let alone a century of peace, required a type of tough diplomacy practiced by Henry Kissinger and not the actions of an amateur like Henry Wallace.

For all his good intentions and years of public service, Wallace was not a good politician. His personal Watergate was in allowing those around him in 1948 to get control of the Progressive Party machinery and, in so doing, force him into untenable positions before the American electorate. Wallace so deeply believed in the Sermon on the Mount as a moral guide, both in personal and international relations, it was difficult for him to consider the possibility that men and nations do deceive one another.

The author hails Henry Wallace as the leader of "social liberals" (p. 2) who desired the attainment of world peace and universal abundance. These noble goals were thwarted, according to the author, because of the obstruction of turncoat New Dealers, Cold-War liberals, red-baiting anti-communists, Harry S. Truman, and assorted fascists. On the domestic scene even F.D.R. comes in for censure since "social liberals were in fact prisoners of New Deal broker-state politics and the Roosevelt personality cult" (p. 325). Markowitz reacts uncritically to the problem of credibility confronting Wallace arising from his constant need to apologize for the bellicosity of the Soviet Union or to defend the dogmatic pro-communist stance of the Progressive Party.

Henry Wallace deserted his so-called Gideon's army over the issue of the Korean War, and as a result his armor seemed a bit tarnished by those awaiting the advent of the elusive people's century. Thus the author comments: "Henry Wallace, spending his final years in virtual exile at his South Salem farm, would observe the ensuing foibles and dilemmas of the cold-war liberals with faint interest, preferring instead to return to God, strawberry planting, and a celebration of the eternal verities of rural life" (p. 297). While this description is deprecatory in tone and overlooks his useful work on behalf of undeveloped nations, it does, however, point out why Wallace did not fit in with the urbane and sophisticated clique that constituted the far left of that era.

In this book the author makes use of other studies as well as a host of manuscript sources and interviews. Despite its polemic style, doctrinaire assertions, and unwarranted attacks on liberals who opposed the Progressive Party, this work still has value. It does reveal the mentality of the radical left of the post-World War II period and explains in part the rationale for their ideology. Further investigation might indicate that the "1948 Progressives" were in fact the advance guard of today's New Left.

EDWARD L. SCHAPSMEIER
Illinois State University

Another Look at the Twentieth-Century South. By George E. Mowry. (Baton Rouge: Louisiana State University Press, 1973. Pp. ix, 90. $4.95.)

This brief volume contains three lectures presented as part of the Louisiana State University Walter Lynwood Fleming Lectures in Southern History. Not being a specialist in southern history, Mowry proposes to comment upon "some relatively unexplored corners of southern culture from, perhaps, an angle of vision not ordinarily utilized by the many distinguished historians of the section" (p. vii).

In the first chapter, Mowry challenges the notion that southern history is distinctive—a contention no doubt novel and pehaps unwelcomed at a southern history lecture series. Mowry argues that excluding the presence of blacks, the South was similar to the western Middle West (Iowa, Kans., Minn., Mo., Nebr., N. Dak., and S. Dak.), especially between 1900 and 1940. The similarities noted are agrarian economies, predominance of fundamentalist religious sects, poverty, one-party politics, galleries of demagogues, and common features of folk culture and literature. While a number of likenesses are demonstrated, the presentation is unlikely to convince readers that the politics or history of the South is not unique since it is questionable whether the presence of millions of blacks can be discounted in understanding the region, as Mowry seeks to do. Nor does he consider the impact on the region of losing the Civil War and subsequently experiencing Reconstruction. Perhaps the thesis presented in skeletal form in this book will inspire someone to undertake a thorough investigation of the similarities of southern politics with those of other regions, rather than concentrating on differences. Research along those lines would advance an understanding of the impact of blacks and the Civil War on regional politics.

While the first chapter poses an interesting

proposition, the last two chapters are less evocative. In them Mowry explores what he calls the paradox of southern political conservatism. In chapter 2 the political careers of three southern senators are used to illustrate the gradual disaffection of the southern wing from the national Democratic party. Mowry concludes, not surprisingly, that the public stands of these men were conditioned less by ideological or partisan bonds and more by concern about re-election and a desire to maintain the socioeconomic and racial status quo in their region.

The author suggests that southern support of New Freedom proposals and most New Deal measures prior to 1937 represented (1) a trade-off for regional control of racial matters, (2) a trade-off for regional gains from other programs, or (3) a response to constituent demands. While the author seems inclined more toward the first possibility than the others, he presents little documentation for any of them. Nor does he adequately explain why southern support for the New Deal declined after 1937.

By raising the possibility that southern support for reforms altering the nature of American federalism occasionally came in return for northern Democrats' acceptance of southern racial and labor practices at variance with democratic norms, Mowry seems to undercut the logic of his first chapter. What sense does it make to show that the South is similar to seven midwestern states, once blacks are excluded, if the primary independent variable used to explain the political and cultural attributes of the South is the presence of a large black population? Indeed the thesis of the first chapter is at least partially refuted by the material in the second and third chapters.

The chief value of this slim set of lectures will be realized if they prompt political scientists and historians to undertake analyses of the propositions so fleetingly presented.

CHARLES S. BULLOCK, III
University of Georgia

The Role of Analysis in Regulatory Decision-making: The Case of Cable Television. Edited by Rolla Edward Park. (Lexington, Mass.: Lexington Books, 1973. Pp. 112. $9.50.)

Cable television transmission began simply as an expeditious technique for the dissemination of an improved television signal to receptive customers. As the infant cable industry took hold, several changes in its scope and operating procedures were to force a reluctant FCC to intercede in its development. The expansion of cable's activities into such areas as program production, brokerage of air time, pay-TV, and the importation of distant TV signals clearly called for government regulation.

Despite its present status as a viable communications medium, cable television functions against a supercharged backdrop of conflicting claims, controversy, and tightening government controls. Industry advisories proclaim that franchises are being awarded in increasing numbers, whereas trade and financial publications report cutbacks in the construction of cable systems (especially in large cities), sluggish subscriptions and mounting opposition to pay-TV by open circuit broadcasters. Adding to the turbulent atmosphere are the prospects of a new and perhaps restrictive copyright law and the imposition of more stringent federal, state, and local regulations.

Rolla E. Park, senior economist at RAND, has attempted an orderly review of the chaotic growth of this communications system, which is trying so hard to live up to its public relations releases. He has asked eight economic experts—from government and research foundations, from the FCC, and from the industry itself—to share their views on the role played by economic analysis in the drafting of regulations governing cable television. The volume is an outgrowth of a panel discussion held at the Western Economic Association in August, 1972.

Among the questions Park has directed to this panel are: What impact has cable had upon the broadcasting industry? How has your own analysis changed over time and because of the influence of other observers? To what extent has FCC policy been shaped by various analyses? How does the analysis of an "interested" party compare to that of an outside expert?

Not all of the questions are answered by each of the eight contributors, but most are touched upon. Park's summary of their conclusions indicates that the 1972 FCC regulations, which were to be crucial to cable's growth and control, were only slightly influenced by the findings of economic analysts. The regulatory agency tended to regard their data with suspicion because many analysts were directly involved in the industry and therefore were incapable of producing objective reports. Their analyses were frequently tailored to favor the company who hired them and to undermine its competitors.

As evidence of the minimal role played by analysis in the decision-making process, Park cites the fact that final formulation of the FCC regulations was based primarily upon compromises between individual commissioners and contending parties. Furthermore, in opting to implement its cable rulings on a short-term basis, subject to frequent revision, the Commission clearly chose to ignore long-range, reasoned analysis.

On the plus side and in summary, Park notes several ancillary contributions made by economic analysts. Their reports provided a framework for FCC deliberations and an empirical foundation for policy formulation. Their meticulous research

lent respectability to the industry, broadening awareness of its importance. And their analyses helped to dispel the FCC's concern that cable transmission would seriously harm UHF stations.

This volume may serve to help continue the necessary research and study to bring about stability and orderly growth to an industry that in itself contains unmeasured potentialities.

NATHAN SHOEHALTER
Rutgers University

Implementation: How Great Expectations in Washington are Dashed in Oakland; Or, Why It's Amazing that Federal Programs Work at All, This Being a Saga of the Economic Development Administration as Told by Two Sympathetic Observers Who Seek to Build Morals on a Foundation of Ruined Hopes. By Jeffrey L. Pressman and Aaron Wildavsky. (Berkeley: University of California Press, 1973. Pp. xviii, 182. $7.50.)

When the Free Press of Glencoe, a fledgling publisher of social science, published Edward C. Banfield's *Government Project* in 1951, the blurb declared it "an astonishing fact that this is one of the very first studies to describe the impact of big government on the lives of ordinary citizens." There has been many a government project since then and many a government contract awarded to a consultant to study the results, yet political science still has had astonishingly little to say about government in action, at the point where it finally touches the putative beneficiaries of its work.

When social scientists do get interested in government at work, they seem to be drawn to instances of its not working very well. Professors Pressman and Wildavsky's *Implementation*, like Banfield's *Government Project*, is not a success story. It recounts the failure of the federal Economic Development Administration to create jobs for the ghetto poor with grants and loans for public works in Oakland in the late 1960s. After five years, most construction projects either had not been completed or had not yielded jobs for the hard-core unemployed. In the authors' key term, EDA's program was not "implemented." Starting with a "policy," which points "to a chain of causation between initial conditions and future consequences," EDA was able to translate policy into a "program," the first link in the causal chain, but it lacked "the ability to forge subsequent links in the causal chain so as to obtain the desired results." This ability is implementation (p. xv).

EDA came to Oakland in the spirit of the impatient, agitated liberalism of the 1960s—guilty about the squalor of the ghettos, drawn by the potential for riot, and too eager to claim, when riots did not occur, that federal good works had prevented them. EDA's Oakland project epitomized its time. More than $20 million in federal funds were to be concentrated in a middle-sized city to "demonstrate" results. The poor were to participate. The city government was kept at arm's length to establish federal *bona fides* with local blacks. Social science was enlisted via a $400,000 contract with Floyd Hunter to study the city's power structure. Bureaucracy was to be circumvented. Indeed, it is misleading to describe the project as an undertaking of EDA. It was more the personal commitment of Eugene Foley, an assistant secretary of Commerce; Amory Bradford, a former vice president of the *New York Times*, who was Foley's special representative in Oakland for the first few months of the project; and a small task force they assembled. EDA had been spending its funds mainly in rural areas and small towns, as Congress intended. It was Foley, newly appointed in 1965, who sought to take it into the inner city, and he left office the next year.

All of this raises the question of how much is to be learned from the case. In some measure, *Implementation* is a tale of the times. The moral of the tale, which was widely drawn as the 'sixties gave way to the 'seventies, is that people should not expect too much of government. The authors help the reader to adjust his expectations downward. They say they would consider their effort a success "if more people began with the understanding that implementation, under the best of circumstances, is exceedingly difficult." Were this seen to be so, people would be pleasantly surprised when a few good things happened (p. xii–xiii).

The core of the demonstration of how difficult it is for government to achieve objectives is Chapter 5, "The Complexity of Joint Action." The authors identify thirty decision points from the time Foley decided to take EDA into the cities in the fall of 1965 to the letting of contracts in Oakland for construction of a hangar in June 1971. A "decision point" occurs each time an act of agreement has to be registered for the program to continue. The cumulative total of agreements needed, taking account of the number of participants in the decisions as well as the number of decisions, was seventy, on modest, reasonable assumptions about both participants and decisions. Assuming an 80 per cent probability of agreement at each decision point, the probability of success after seventy clearances is .000000125. Assuming a 99 per cent probability of agreement, the probability of success after seventy clearances is still less than .50.

After this ingenious demonstration, one wonders how anything gets done. Pressman and Wildavsky acknowledge that this is a puzzle, inasmuch as the landscape is full of government programs that have evidently been implemented.

Yet in the end they argue that among federal programs involving localities, "The EDA public works program is a bedrock example: almost any other one we could think of would involve a greater conflict over ultimate ends or more public dispute, less adequate resources in view of the end contemplated, and other features that make it evident why programs fail" (p. 123).

They explain the existence of many apparently successful programs as follows: "The years of trial and error that led to the present state of operation are lost from view. The huge amount of resources that may have been poured into different alternatives before one caught on is conveniently part of past history for which we are not responsible. Programs that started out to accomplish one set of objectives end up accomplishing another for which, long after the fact, we give undue credit for implementation." In short, "A basic reason programs survive is that they adapt themselves to their environment over a long period of time" (p. 116).

More precisely, the formal organizations that administer the programs adapt. I suspect that the distinction is important for interpreting the case. If federal programs involving localities are to flourish, a cooperative link must be forged across the formal division of the federal system between permanent bureaucracies pursuing common goals at different levels of government. The EDA had no local bureaucracy as a counterpart and ally in Oakland. Moreover, the project was an aberration for EDA, requiring a redefinition of constituencies if not of objectives. To the extent that programs depend on politics, EDA's Oakland project was indeed a bedrock example. There was no controversy. Implementation should have been easy. But to the extent that programs depend on the performance of bureaucracies, difficulty was highly predictable and perhaps more likely than usual. The federal administrative organization was not committed to the project, and there was no local administrative organization. The authors acknowledge that comparison with other programs would be desirable, but say they were "hampered in this respect because we have been unable to locate relevant research. None that we know of attempts to deal with the problems posed by long sequences of decisions involving multiple participants, each dependent on what has gone before and unable either to control the others or to predict the probability of successful outcomes at succeeding points . . . " (p. 124).

Two additional analytical chapters follow that on the complexity of joint action. Chapter 6, "Learning from Experience," criticizes "going outside the bureaucracy" and "coordination" as nostrums and closes with a plea for policy makers to take the difficulties of implementation into account. The final chapter criticizes the economic theory underlying EDA's intervention in Oakland and argues the merits of subsidizing labor rather than capital as a way of raising employment in the inner city.

This is a salutary book at a time of rising interest in policy studies. It is a reminder that the distinctive competence of political scientists lies in explaining the processes by which public purposes are, or fail to be, defined and achieved. It focuses in a systematic way on the achievement of policy, which American political scientists have given short shrift by comparison to formulation, in domestic affairs at least. The authors plainly mean to be breaking a path, and if the path is not wholly new (I find it hard to distinguish between "implementation" and "the administrative process"), it surely needs more mapping of the kind that is usefully begun here.

The book is the second in the Oakland project series, following Arnold J. Meltsner's *The Politics of City Revenue*—the Oakland project, that is, of the University of California, under Wildavsky's direction, through which faculty members and students have worked in city government.

The illustrations are by Rube Goldberg.

MARTHA DERTHICK
The Brookings Institution

The Politics of Environmental Concern. By Walter A. Rosenbaum. (New York: Praeger Publishers, 1973. Pp. 298. $9.00.)

This is an important book about an important public issue. Professor Rosenbaum argues persuasively that environmental issues are the most critical concerns facing this nation and indeed every national political system. His analysis of "ecopolitics" follows the traditional Lasswellian formula of "who gets what, when, and how." The author shows great sensitivity to the political dilemmas facing ecologists and what he calls the pikes upon which politicians may impale many of the demands of environmentalists, e.g., the tradition of incrementalism, the role of organized interests in public policy, the diffusion (or fragmentation) of power, and the burgeoning "administrative state" as ultimate interpreter of public policy. The book also provides important insights into the larger public policy-making process of which ecology is but one application, as well as an informative examination of the politics of air and water pollution, timber, surface mining, and solid waste. The analysis provides a fine historical perspective on the scope of federal government activity in these issues.

Despite these laudatory efforts, however, I think the book has two basic limitations. One, notwithstanding the intrinsic importance of the environ-

mental issue and the major public policy advances of the 1960s and early 1970s, this concern is quickly losing ground to America's cultural dependence upon the "unlimited" availability of power and energy. Rosenbaum's book, therefore, although written (or completed) in 1972, has already become dated, in 1973, as the "energy crisis" very rapidly seizes central prominence in American politics—unless one views the current energy crisis as reflecting the same fundamental concern of ecologists (both new and old) for the wasteful depletion of our limited natural resources. (In this way, the semanticist might effectively argue that the energy crisis is merely one manifestation of the environmental decade.)

These two crisis situations (energy and ecology) are on an apparent collision course and policy makers must inevitably choose between policies that will further develop certain sources of energy (e.g., soft coal and shale oil) at the expense of environmental interests and policies that will permit (or even encourage) a decreased utilization of energy and adhere to a strict environmental credo. I think one could have predicted that public policy resulting from such confrontation would proceed in a direction counter to the environmentalists. Although Rosenbaum does acknowledge the possibility of a displacement of the ecology issue as a major public concern, "especially when the costs, direct and indirect, become more tangible" (p. 16), his description of the 1970s as the "environmental decade" characterizes very clearly the book's tenor and theme.

The second limitation stems from the exaggerated importance accorded by Rosenbaum to the concept of "planning." In examining the complex array of organized governmental and nongovernmental interests that have a stake in environmental policy, the author consistently decries the lack of coordination and comprehensive planning, suggesting that "national environmental planning that is comprehensive, coordinated, and sensitive to government's own environmental impact constitutes the framework of the ecologist's most sweeping political objectives" (p. 255). Although he correctly points out that there are massive political obstacles to the implementation of environmental plans, he shows little appreciation of the limited state-of-the-art of planning as a means of ameliorating social and political problems.

In the field of health policy, for example, we have come to realize that planning has certain inherent limitations even under optimal political conditions. Thus despite the enactment of federal legislation in 1966 establishing an elaborate system of comprehensive *health* planning, individuals concerned with the issues of quality, cost, and the availability of health services are still groping for a mechanism to counter the extreme fragmentation in the health care system. Rittel and Webber have succinctly captured the essence of this problem in a recent article on "Dilemmas in a General Theory of Planning," *Policy Sciences*, 4 (June 1973), 155–169. They point out, for example, that there can be no definitive formulation of a "planning-type problem": "The process of formulating the problem and of conceiving a solution (or re-solution) are identical, since every specification of the problem is a specification of the direction in which a treatment is considered" (p. 161). Moreover, there can be no immediate or ultimate test of a solution to a planning-type problem because "any solution, after being implemented, will generate waves of consequences over an extended —virtually an unbounded—period of time" (p. 163).

Rosenbaum's oversimplification of a very complex process is compounded by his recommendation for establishing a cabinet-level federal agency to "coordinate" national environmental policy. Apparently, he bases this on the assumption that federal *departments* are better able to bring about a rational and coordinated national policy on substantive social issues. That bureaucratic pluralism and overlap do not disappear with reorganization and establishment of a new superagency is clearly evident in the Department of Health, Education, and Welfare—where periodic internal reorganization has become a way of life.

Thus, while environmental issues are admittedly grave and pressing, it is shortsighted to expect that their resolution will take place in a manner generically different from that of other political issues, i.e., external to the traditional system of functional checks and balances. To the extent that planning is proffered as a means of addressing environmental issues in light of our national values and goals, it must be recognized as a limited tool essentially unable to delineate the boundaries of the environmental problem. I do not advocate sitting back and waiting for the technology to develop. But certainly it would have been more relevant if the author had demonstrated greater appreciation of the impracticality of expecting major new policy breakthroughs as a result of comprehensive environmental planning.

HARRIS S. COHEN

Washington, D.C.

S.D.S. By Kirkpatrick Sale. (New York: Random House, 1973. Pp. 752. $15.00.)

Students for a Democratic Society (SDS) was the best-known and the most influential organization in the New Left. The author describes the evolution of SDS from its infancy as a stepchild of the League for Industrial Democracy in 1960 through the tumult of that decade to its disinte-

gration into warring vanguards by 1970. He provides color and dimension by presenting a wealth of new detail about the personalities of SDS leaders and the complexity of a volatile national group of early reformers and latent revolutionaries. The influx into SDS of Communist Progressive Labor Party members and disaffected youth with a pronounced anti-intellectual bent after 1965 is noted in some detail by the author. Moreover, his account of the community organizing and anti-Vietnam war phases of SDS history is factual and comprehensive. The book gives the reader many insights into intraorganizational factions and stratagems as well as new and original material about the extreme variation in administrative competence within SDS, and it explains how successive ranks of members viewed SDS and its mission. Sale's bibliographic references are exhaustive and complete. The appendices include the early history of the League for Industrial Democracy, a chronological roster of SDS officers, and the organization's constitution with appropriate additions and deletions.

Although the author brings to his task the gift of vivid exposition, his analytical ability does not keep pace with his journalistic skill. This work contains minor and major flaws of interpretation and evaluation. Sale says, for example, very little about the philosophical motivation of SDS members—the books they read or the mentors who framed their views of society and helped stimulate a regeneration of American radicalism. Yet the few intellectuals who called attention to political and social issues were widely read by the young men and women who subsequently formed SDS as well as by students who did not join. Three of the most prominent were David Riesman, Erich Fromm, and especially C. Wright Mills. Despite the importance of their publications, Sale mentions them only in passing. This abbreviated treatment is more serious in the case of C. Wright Mills who more than any other single individual contributed to the ideological shaping of SDS. Similarly, Sale gives scant attention to Herbert Marcuse though he was widely read and quoted by SDS members in the mid-1960s and nourished the seeds of elitism in the Movement.

A more serious problem of analysis is the author's imbalanced view of certain complex issues. For instance, Sale stigmatizes the university as an accomplice in the perpetuation of social "evils" (p. 262) and brands S. I. Hayakawa, former President of San Francisco State College, as "megalomaniacal" for his leadership in ending the campus strike of 1968–1969. While quick to condemn Hayakawa for calling off-campus police to quell unruly pickets and their supporters, Sale is curiously insensitive to the rights and welfare of students trying to continue their education unhampered by intimidating strike tactics. The most noteworthy example of Sale's unbalanced assessment of substantive issues is his incomplete explanation as to why SDS shunned reform for revolution and abandoned coalition politics for estranged elitism. According to Sale, the transformation of SDS can be explained by the corruption of American society: its racism and poverty, the complicity between government and business, Southern violence against black people, the assassination of its leaders, the cultural demoralization of its youth, police brutality, and its unresponsiveness to the pleas and programs of the New Left for reform. This is the New Left position and Sale does not step beyond it. Had he done so, he might have entertained the notion that SDS turned to revolution not only because it recoiled from the negative aspects of American society but also because its programs and strategies had failed while it shifted from a deep concern for the victimization of others to outrage at the imagined injustices done to alienated youth: a transformation that led ultimately to political isolation. The alternate case that Sale does not make is that SDS took refuge in ineffective revolutionary vanguards rather than acknowledge its own shortcomings. It was unable to overcome its administrative ineptitude (which Sale discloses) and emulate the perseverance it so admired in SNCC (Student Non-Violent Coordinating Committee). After less than three years as an exponent of reform, SDS condemned government progress toward the solution of social problems as "tokenism," and proceeded to exchange ideological independence and altruism for subservience to Marxism-Leninism.

If the reader is not looking for profound insights into the social and political history of America in the 1960s, but is seeking a well-documented, thorough, and absorbing account of the day-by-day evolution of SDS, this is the book to buy.

EDWARD J. BACCIOCCO, JR.
Hoover Institution, Stanford University

Who's Who in American Politics: 1973–1974. Fourth edition. Edited by Paul A. Theis and Edmund L. Henshaw, Jr. (New York: Jacques Cattell Press, 1973. Pp. 1216. $40.00.)

Here is a reference work which political scientists should certainly know about, though few are likely to purchase. Theis and Henshaw have collected biographical data on 18,000 American politicians currently holding public office or still active in politics. Included at the national level are all elected officials plus major presidential appointees and the major officials and staff of the political parties. But these persons are included in other biographical collections. What makes this

volume indispensable are the state and local entries. Included are all governors, lieutenant governors, major state officials, state legislators, mayors and councilmen of major cities, and officials and principal staff of the political parties.

The entries are based on questionnaires and thus there is the inevitable unevenness in material included. Still the volume is a godsend for political scientists interested in career development. The editors have even provided a geographic index, thus easing the task of comparative analysis among states or regions. Unfortunately, however, though they designate party affiliation in the index, they do not provide a simple code for current political position, if any. Perhaps this might be provided in future editions as an aid to the scholar.

Paul Theis is public relations director of the Republican Congressional Campaign Committee; Edmund Henshaw is executive director of the Democratic counterpart. They are assisted by an advisory committee which includes the American Political Science Association executive director, Evron M. Kirkpatrick, as well as Richard M. Scammon, director of the Elections Research Center, Governmental Affairs Institute.

CHARLES O. JONES

University of Pittsburgh

Letters of Louis D. Brandeis: Volume I, 1870–1907, Urban Reformer; Volume II, 1907–1912, People's Attorney; Volume III, 1913–1915, Progressive and Zionist. Edited by Melvin I. Urofsky and David W. Levy. (Albany: State University of New York Press, 1971, 1972, 1973. Pp. xlii, 610; xxiv, 750; xxii, 705. $20.00 per volume.)

Writing shortly before his appointment to the United States Supreme Court in 1916, Louis Dembitz Brandeis argued that "no law, written or unwritten, can be understood without a full knowledge of the facts out of which it arises and to which it is to be applied." The publication of large portions of his correspondence, under the able editorship of Melvin I. Urofsky and David W. Levy, illuminates Brandeis's passion for knowledge and understanding of the political, economic, and legal issues of his time. Alpheus T. Mason, Brandeis's principal biographer, tells us that even though the reform lawyer, later Supreme Court justice, was a factualist and stickler for details, particular "emphasis should be given to the coherent and purposeful social-political philosophy which underlies his profound factual knowledge" (*Brandeis and the Modern State* [Washington: National Home Library, 1936], p. 233). Although there are no startling discoveries, the published correspondence gives added insight into the progressive temperament. And, as the editors point out, the letters help us understand the techniques and strategy of reform movements on all levels of American society.

Assembling the Brandeis correspondence was an arduous task. Mason incorporated much significant correspondence into *Brandeis: A Free Man's Life* (New York: The Viking Press) in 1946; yet the Brandeis papers remained scattered. The editors began at the University of Louisville Law School, the principal depository. Then, through other manuscript depositories and private collections, Urofsky and Levy sought additional material; from 14,000 letters they made publication decisions. Only two known sources remained closed: the personal letters which Brandeis wrote to his fiancee Alice Goldmark—letters now in the possession of Brandeis's daughter, Mrs. Elizabeth Brandeis Raushenbush; and letters in the warehouse of Brandeis's old law firm, Nutter, McClellan, and Fish. Mrs. Raushenbush allowed the editors to quote extracts from some letters. The lack of cooperation from the present partners of Brandeis's firm is especially unfortunate, for there are gaps in the published correspondence dealing with strictly legal matters. For example, one finds little on *Muller* v. *Oregon* (1908), the Supreme Court case in which Brandeis presented his now-famed sociological brief. In addition, Brandeis destroyed much of his private correspondence.

These three volumes yield little insight into private beliefs and personality factors which often aid political scientists and historians alike in making general assessments of career patterns and subsequent judicial behavior. The editors appropriately conclude that "the figure who stands revealed in these letters will seem, very often, the austere and efficient public man" (I, xix). As Mason has said, one should not reach the conclusion that Brandeis was cold and impersonal. He fervently guarded his own privacy, not surprisingly for one who contributed the important right to privacy in contemporary constitutional law. The letters document Brandeis's insatiable quest for facts—facts he could digest and transform into arguments for social action. Other qualities emerge: integrity, courage, and, occasionally, moral indignation against business practices that he felt needed to be changed. Warmth and deep affection for members of his family emanate from letters to his father, his sister Amy, and his brother Alfred. His correspondence with Alfred, in fact, gives rare insight into personal feelings and decisions; it is thus much like the useful correspondence between Chief Justice William Howard Taft and his brother Horace.

The editorial work is first-rate. Students will find particularly helpful the complete, interesting identification of persons addressed or mentioned

in the letters. The explanation of issues or problems—such as the "sliding scale" principle in the gas industry, the Pinchot-Ballinger Affair, or the right to privacy—raised in the text of letters is of consistently high quality, a tribute in itself to the scholarly ability of Urofsky and Levy as American social and intellectual historians. The editors based publication decisions upon the historical value of each letter, feeling that "erring on the side of generosity was preferable to erring on the side of brevity" (Vol. I, p. xviii). One's inclination is to agree: some letters, taken alone, appear to have little inherent merit. Yet on second thought one sees that the entire body of published correspondence, including pieces that seem trivial or repetitive, creates a coherent whole, from which broad themes and intellectual contours emerge.

Volume I, *Urban Reformer*, shows Brandeis's evolution as an urban progressive, fighting the abuses of the Boston street railways and gas companies, in quest of social reform. He became an unusually successful local reformer, having achieved a major success in the passage of the Savings Bank Life Insurance Law in Massachusetts. The letters in Volume I show a growth of confidence and an increased willingness to exercise social and political leadership in Boston and Massachusetts. In the years encompassed by Volume II, *People's Attorney*, 1907–1912, Brandeis's attention was drawn more and more to Washington, though he did not fail in his commitment to Massachusetts. He was a leader in numerous national crusades: an opponent of the proposed merger of the New Haven and Boston & Maine railroads, and a spokesman for orderly development of natural resources in the Pinchot-Ballinger Affair. During this period Brandeis and Senator Robert M. La Follette were drawn closely together, serving thereby as important intellectual forces in the progressive movement of the 1910s. Between 1913 and 1915, as the letters in Volume III, *Progressive and Zionist*, indicate, Brandeis was at the zenith of his career as a reform lawyer. An important policy adviser in the Wilson administration, Brandeis had a major effect upon banking and antitrust programs. In rate hearings before the Interstate Commerce Commission, he assumed the unique role of counsel for the public. His efforts against the New England railroad merger—seven years in argument—prevailed.

Of the five proposed volumes, these three provide us with rich insights into the pre-Court years of Mr. Justice Brandeis. They represent a storehouse of important data regarding the possible effects of background factors upon later judicial decision making. For the student of judicial recruitment, the letters contain important implications regarding career development and patterns of political influence.

More than anything else, these three volumes show us Brandeis's love of the law. His basic acceptance of social and political institutions, plus his reverence for the law as a mechanism of change, make Brandeis a Burkean conservative, as Urofsky argues elsewhere (*A Mind of One Piece: Brandeis and American Reform* [New York: Scribners, 1971] p. 17). And, at the same time, because of his zest for reform, Brandeis emerges as a major figure in the new liberalism of the Wilson era. The letters indicate again and again that Brandeis sought change, but not radical change. As Mason says, "he did not hate capitalism; he deplored its abuses" (*Brandeis: A Free Man's Life*, p. 105). Through the medium of law, social change was possible. And the necessity for such change had a resultant effect upon characterization of the law. The work of legislators and judges alike was to bring the law more in accord with life. Out of such ideas came the acceptance of sociological jurisprudence, one of the indirect legacies of Brandeis's career.

ROBERT G. SEDDIG
Allegheny College

Black Political Parties: An Historical and Political Analysis. By Hanes Walton, Jr. (New York: The Free Press, 1972. Pp. xi, 276. $7.95.)

This book covers a great deal of ground, describing not only black political parties, but also other types of black political organizations and movements. The substantive core of the book is a national overview ranging from the first annual Negro convention in 1830 to the Gary convention of 1972, a summary look at state parties from the Black and Tans to the contemporary United Citizens Party of South Carolina, and an intensive treatment of black political activity in contemporary Mississippi and Alabama, with special attention to the Mississippi Freedom Democratic Party, the Lowndes County Freedom Organization, and the National Democratic Party of Alabama. The book is copiously documented with footnotes, tables, figures, and maps. It begins in a context defined by James Madison and ends in one defined by V. O. Key, William Hesseltine, and others.

While there is no lack of information in this book, there is, unless it escapes me, little real analysis. Some useful distinctions are drawn, such as that between independent black political parties, and satellite or parallel parties, which are usually state adjuncts to a major national party. Yet other than being used as a basis for organizing the book, little is done with this distinction.

Comments on the achievements (or more often lack of achievements) and consequences of black parties are scattered throughout the book, but they are usually isolated comments that are not

threaded together into a sustained theme. The standards of political "success" are nowhere clearly delineated, and most of the successes noted are in terms of votes mobilized or offices won.

While Professor Walton is keenly aware of the obstacles to black political success, he contributes little to the development of a theory of the limitations on such success and the conditions under which they may be overcome. What can be expected when a minority organizes to advance interests that are opposed by a majority? What patterns of opinion distribution, opinion intensity, or combinations of multiple issues would enhance the influence of black parties? What institutional arrangements help or hurt the most? Is there an identifiable threshold of declining political discrimination beyond which concrete gains can be made? To what extent does an oppressed minority have any control over its fortunes through democratic politics?

Good questions are raised in the beginning of the book, and there are conclusions at the end, but the conclusions are familiar and do not benefit much from any systematic analysis of the empirical materials brought together in the book. The volume's most important contribution is in bringing together a great deal of descriptive information, and giving an historical perspective on black parties in America.

WILLIAM R. KEECH
The University of North Carolina at Chapel Hill

In Pursuit of Price Stability: The Wage-Price Freeze of 1971. By Arnold R. Weber. (Washington, D.C.: The Brookings Institution, 1973. Pp. 137. $5.95, cloth; $2.50, paper.)

Arnold Weber—the administrator and one of the architects of the 1971 wage-price freeze—summarizes his perception of the background, implementation, and lessons of this policy in this short book. The book will be a valuable manual, rather like deSegur's history of Napoleon's Russian campaign, to those who decide to impose another such freeze. The book does not contribute to an understanding of the effectiveness of a wage-price freeze.

Weber's perception of the background of the freeze is conventional but flawed. He contends that "from the outset of the Nixon administration the suppression of inflation had been a central concern" (p. 1). In fact, the administration pursued a strongly inflationary policy; after 1969, the rate of increase of both federal spending and the money supply increased for three successive years to record peacetime rates. He suggests that the traditional ways to reduce inflation had proved ineffective or unacceptable; in fact, they had not been tried. He concludes that the administration's earlier resistance to controls somehow increased their inevitability and scope. He does not explain how a president who was elected on a platform critical of controls, whose chief economic advisors resisted controls, and who a few months earlier stated that he would not impose controls came to determine that the 1971 freeze would be either good economics or good politics. Weber reports, without comment, that the minimal prior staff work was limited to administrative details.

Weber regards the 1971 freeze as a bureaucratic miracle. He attributes the credibility of the program to "the performance of the leading players and the skill of the stagehands" (p. ix). The administrative policies that most contributed to its success, according to Weber, were the secrecy of prior planning, the drama of the August 15 announcement, calculated uncertainty about the duration and future scope of the controls, a casual disregard for due process, the intimidating effect of using IRS agents as inspectors, the use of extralegal pressure, etc., and he amply documents the use of these policies. One wonders whether these lessons for improving bureaucratic performance are generalizable to a constitutional democracy.

In the final chapters, Weber suggests several criteria for judging the 1971 freeze:

> Did it reduce inflation during the freeze?
> Did it reduce inflationary expectations?
> Did it permit "the development of more substantial economic policies?" (p. 129).

In fact, the rate of inflation of wholesale prices declined from an annual rate of 4.9 per cent in the six months preceding the freeze to a rate of minus .4 per cent during the freeze, and interest rates declined by .5 percentage point. It is difficult to attribute these changes to the freeze, however, because of coincident changes in other economic policies. Most importantly, the rate of increase of the money supply declined from an annual rate of 9.7 per cent during the prior six months to a rate of minus .5 per cent during the freeze, and this fact is not mentioned by Weber. Some crude estimates of my own (published in the *Journal of Money, Credit, and Banking* in May, 1973) suggest that the controls probably reduced interest rates temporarily in 1972 but had no significant effects on average prices, wages, and unemployment in either 1971 or 1972. For all that, the controls may have been good politics.

The longer-term record of the controls developing from the 1971 freeze, of course, has been disastrous. In 1973, the U.S. economy experienced record rates of inflation and interest and widespread shortages, and there is no near-term prospect of "more substantial economic policies." Grayson, the head of the Price Commission in Phase II, has recently urged the immediate termi-

nation of the control system. Arnold Weber, apparently, has had no such second thoughts.

WILLIAM A. NISKANEN
University of California, Berkeley

The Politics of the Ocean. By Edward Wenk, Jr. (Seattle: University of Washington Press, 1972. Pp. 590. $14.95.)

This book overwhelms the reader ranging as it does through every facet of U.S. ocean policy over the past twenty years. The book is also a primer on bureaucratic, administrative, and organizational politics and, while it does not approach this subject in a systematic way, it represents keen insight from firsthand observation. Dr. Wenk assesses the strengths and weaknesses of every interest group, cognizant agency, congressional committee, White House officer and the general public as they bear on ocean policy.

The book is organized into three broad subjects. It first examines the generation and development of U.S. ocean policy; a middle section covers the principal substantive issues that occupied the attention of the Marine Science Council, of which Wenk was Executive Secretary; and the final part of the book projects implications and postulates future management avenues for marine policy. These include the author's assessment of problems facing the recently created (1970) National Oceanic and Atmospheric Administration and extensive suggestions for U.S. leadership in the international development of ocean resources.

Wenk, like J. Clarence Davis, III, gives much credit to Congressmen for spearheading the creation of environmentally related policy. Highest praise is given to Hubert H. Humphrey who chaired the MSC and lent the prestige of his personality and vice-presidential office to the Council which, in fact, did a unique job of coordinating agency interests and developing policy priorities. Wenk reserves his most weighty brickbats for the Bureau of the Budget (later the OMB), pointing out procedures which preserved narrow policy alternatives and frustrated officials who had already "suffered both the uncertainties of delay and anguish" (p. 124) in making policy. The author makes many tart observations about government operation, and at one point we are told how Vice President Agnew, failing to manage a debate in the MSC, ordered that the next Council meeting be staged with a complete scenario on hand (p. 163).

The substantive focus of the book relates to the problems of identifying and developing the resource potential of the oceans. For example, the U.S. fishing industry is examined to show how antiquated practices reduce the catch and how a disorganized industry cannot make an effective impact on the policy process except to protect the outdated methods of many of its members. He criticizes the divisions and jealousies in the U.S. scientific community—divisions which restrain the national impetus to explore the oceans for both scientific and practical purposes. He relates in detail the attempt to create a Decade for Marine Development and to establish a food protein program from ocean sources. Energy concerns are discussed also, and Wenk warns that the glamorous promises of ocean wealth must be verified by further exploration and study. While he disagrees with the National Petroleum Council about how far out to sea a nation should claim exclusive rights to offshore minerals, he believes that the petroleum industry can tap ocean oil resources successfully and safely.

The book also covers matters of interest to scholars of international affairs. Wenk examines the development of the latest U.S. Law of the Sea proposals; for instance, he reveals how the U.S. trusteeship concept was developed by government consultants—particularly Professor Louis Henkin. In this proposal the U.S.A. has sought to compromise between the international advocacy of Maltese Ambassador, Arvid Pardo, and the protectionist interests of many developing countries and segments of American industry as well.

No review could begin to do justice to the wealth of information contained in this volume. Those interested in ocean policy will pursue this major contribution with great interest. The reader interested in a case study of policy making and implementation will find the work impressive and utterly thorough, but not organized by any obvious models. The book also contains extensive appendices and notes which will be helpful to the research-oriented scholar. Yet through all of the vast material on a complex issue, one still looks for a clear perspective on ocean policy. Because this book is written from a partisan point of view, we want to know more about why ocean policy should receive priority among other national needs which are equally pressing. Wenk gives a partial answer to this in his description of the oceans as a world resource; but perhaps a more precise answer is obscured in the details of policy memos, jurisdictional battles, meetings and continuing efforts to be mentioned in the next presidential message.

This examination of ocean policy development reminds us of the difficulties any policy proponent must face in seeking and holding the attention of all the necessary parties—from the public to the President and back again. In the last part of the book Wenk focuses upon the future of oceans use and proposes the oceans as an area in which the U.S.A., by virtue of its democratic institutions, scientific know-how, and technological capabilities, can do much to encourage sound manage-

ment for the benefit of the entire world. These suggestions are inherently visionary but completely in line with the author's obvious impatience over the complex and distracting (though admittedly unavoidable) forest of detailed political reality through which his insight has been projected.

DONALD E. MILSTEN
University of Maryland, Baltimore County

The Guarantee Clause of the U.S. Constitution. By William M. Wiecek. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 324. $12.50.)

This analysis of one of the most sterile provisions of the Constitution is intended to persuade us that it has an "awesome potential" for social prophylaxis. It is Professor Wiecek's thesis that Congress and the Supreme Court, especially, ought to convert the guarantee of a republican form of government to the states into a requirement that they reform both their governmental and "social institutions" so as to become truly republican (p. 302). He admits the difficulties involved in activating this clause. They arise from the Delphic character of the constitutional language and the desuetude into which the clause has fallen since 1842, when, he argues incorrectly, it was last applied. Nevertheless, he has subjected the development of the clause to an historical analysis covering the period from 1787 to 1962 to determine its origin, purposes, and uses.

In a country where Bourbon is merely the generic name of a whiskey, and not the surname of royal pretenders, it is not surprising that until now the meaning of this clause has been viewed as one of those barren legal questions which, Justice Frankfurter once remarked, leads to political dialectics and conclusions unrelated to reality. But even when one assumes that some sound purposes of scholarship are served by this study, it is very disappointing to find at the end nothing more than the author's hope that the Supreme Court will "evolve substantive standards of republican government and . . . enforce these (sic) on state governments" (p. 289).

In addition, Wiecek's treatment of his findings is highly tendentious. He has found that on those rare occasions when the meaning of the clause was debated seriously, it was given both "conservative" and "reformist" interpretations, to no one's surprise. Unfortunately, he is determined to prove that these scattered debates reveal the clause to be bulging with latent power, for good or evil if it be construed as broadly as both its critics and admirers have feared or wished. This obsession leads him to confuse separate elements of Article IV, Section 4: "The United States" shall guarantee to each state a republican form of government and, in addition, protect it from invasion and respond to proper requests from a state government to quell domestic violence. These are three distinguishable circumstances, although they may not necessarily be unrelated on all occasions. In seeking to define "republican" government, Wiecek sometimes links the guarantee and domestic violence clauses to demonstrate that the clause was intended to be used to "repress" the downtrodden of various sorts. In his search for proof of this contention, he has included even the Whiskey Rebellion as evidence. He separates these two clauses of section 4, however, when he argues that the guarantee was truly defined by the Abolitionists and radical Free Soilers who incorporated into it the Declaration of Independence and various bills of rights. The latter interpretation supports his reformist goals, of course, but it leads him to the irritating practice of drawing unwarrantable inferences from generally sound data.

In tracing the genesis of this clause Wiecek argues correctly that there was no "single and universal denotation" of republican government in 1787 (p. 13). It was even a "revolutionary ideology," but it was not devotion to it which moved the framers to insert the guarantee of a republican form into the Constitution. It was the fear that Shay's Rebellion would be only the first of many armed assaults against men of property. Consequently, this guarantee was defined by the domestic violence clause as twin elements of a "contemporaneous process of delegitimizing the resort to violence by the people as an extra-legal means of defending the community interest" (p. 42). Despite this highly dubious conclusion, Wiecek concludes his study with the assertion that the framers intended this clause to provide "federal supremacy over the states" and to command the latter "to realize the ideals and aspirations they had embodied in the Declaration of Independence, the preamble to the federal constitution, and the libertarian elements of the state constitutions" (p. 291).

Wiecek draws equally unwarrantable conclusions from his examination of the treatment of the guarantee clause during Dorr's Rebellion and Reconstruction. He concludes that President Tyler's view of the extent and limitations on executive discretion to implement section 4 during Dorr's Rebellion "has prevailed to this day because it is basically sound" (p. 105). Wiecek treats Tyler's words and actions as an interpretation of the guarantee clause, but, in fact, Tyler explicitly refused to rule on the lawfulness of the government of Rhode Island and its challengers. He denied that any president has the authority to determine whether or not a state's government is republican in form. Instead, he refused to send federal troops to a state upon mere rumors and threats of violence and he counseled the parties to resort to all peaceable and legal means of changing the state's government. Dorr's supporters in

Congress tried, but failed, to gain agreement from others that the established government in Rhode Island was not republican. They had no more success in the Supreme Court which refused to treat the dispute as justiciable in *Luther* v. *Borden*. From these events Wiecek draws two conclusions: they support Hamilton's "repressive" definition of the clause in *The Federalist* No. 21, and Taney's opinion was mere *dictum*, "absolutistic," but "defensible" (pp. 122, 124). He was too cautious in restricting judges to " 'fixed constitutions and laws, given them by others, and not provided by themselves' " (p. 121).

Wiecek requires eighty pages to demonstrate that Congress rejected this clause as the legal basis for Reconstruction. Nevertheless, he remains warmly sympathetic to Senator Charles Sumner's contention that the clause is a "sleeping giant" which confers upon Congress unlimited power to remake state and local governments. Incredibly he concludes at one point that even in rejection the clause was greatly expanded in "scope and character" (p. 166). Later he laments that by 1877 it had become a "deadletter" because Congress had "abdicated" responsibility for it (p. 225). Yet, he recites the occasions on which Congress considered and rejected claims that various Southern governments violated the clause.

He admits that implementation of his position would destroy federalism, if pushed to the limits. He has some doubts that judicial reconstruction of political institutions is consistent with his equation of republicanism and self-government. But he overcomes these doubts with his conviction that the Court ought to scrap the doctrine of political questions and adopt the guarantee clause to undertake reform. This strategy would extend the reach of the Court's power because it would not be restricted by the concept of state action embodied in the due process and equal protection clauses of the Fourteenth Amendment.

Protagonists of judicial activism may welcome this addition to the literature dealing with the guarantee clause. Others who might seek illumination of the meaning of republicanism in the eighteenth century, a modern reinterpretation of it, a fresh understanding of the Constitution, or even a serious justification for the reform of republican institutions by robed guardians will find little instruction in this book.

ROBERT J. MORGAN
University of Virginia

Rural Politics and Social Change in the Middle East. Edited by Richard Antoun and Iliya Harik. (Bloomington: Indiana University Press, 1972. Pp. 498. $13.50.)

Conference papers make notoriously bad books, but this volume is a happy exception. The coherence of the book is probably due to careful planning by the organizers of the meeting, though it is also correct that this neglected subject has rarely been treated comprehensively, at least among students of the Middle East and North Africa. There are a number of important lessons to be learned by political scientists from this study.

First, we have on the whole been a bit simple-minded in our use of anthropology (and other disciplines) to rescue political science. Reading the theoretical chapters by Bailey, Fernea, de Planthol, and Antoun will relieve any such illusions, and contribute to a more intelligent use of anthropological concepts in political science, especially in the study of local politics. Second, the seven case studies go a long way toward conveying to our discipline the remarkable interest of anthropology in political questions, as noted by Geertz in his summary. To read about parties, administrators, national elites, elections, and rural development as they affect political change in villages and towns is a sobering experience for scholars who may think our discipline actually encompasses politics. Anyone interested in sub-national politics can learn a great deal from this book.

The first theoretical paper is Bailey's and closely follows ideas developed in his *Stratagems and Spoils*, illustrated with Barth's work on the Swat Pathans. Because the possible applications of transactional analysis to politics are many, it is unfortunate that Barth's views were not more thoroughly discussed. Some of the problems with the highly relativist position Bailey advocates are raised in the next essay by Rosenfeld. In his general commentary on anthropology he refers to the difficulties of getting a time or problem focus on change using anthropology (p. 53) and the general aversion of anthropologists to dealing with conflict (p. 60). Most important, he notes the "failure to explain the system that creates the lives the peasant lives" (p. 73). Fernea makes a highly suggestive addition to the theoretical section in his paradigm involving tribal, urban, and productive factors as they relate to change in villages. Antoun to some extent contradicts Rosenfeld by presenting a thoughtful outline of the contrasting demands for order and competition at the local level.

The case studies are uniformly of very high quality and distinctly political in their implications. Peters writes of the transfer of local and external influence from traditional to professional elites in Lebanese villages. Khuri concentrates on change in Lebanese suburbs. Rosen presents a well-written account of competition for office among Arabs and Berbers in the Moroccan town, Sefrou. Meeker's essay on the adjustment of the Turkish Aghas of the eastern Black Sea region

comes to life as he related how the nationalizing election of 1950 led to local political change. In Abu-Zahra's essay on Sidi Mateur in Tunisia, there is an excellent account of party and administrator intervention in local affairs and its effects. Harik writes of the basic contradiction of Nasser's policy aimed at restoring popular control to communities, while engaging bureaucratic forces at the local level; and Abu-Lughod outlines the effect of migration of social and political questions in Egypt. Though there have been some fine monographs on local-level change in the Arab world, I know no more detailed and suggestive array of cases to illustrate what remains to be done.

Given the rediscovery of policy in political science, many political scientists will be interested in the final section on economic change and reform. Harik writes of the introduction of a domestic market in Egypt in the absence of capitalism. Keddie has a useful summary of Iran's record with land reform. Owen writes of the dual agricultural sector problem which troubles growth in many countries beyond the Arab world, and Simmons has a finely detailed account of the Tunisian farmer in the Sahel region. These are all penetrating studies, and contrast favorably with much of the writing on policy in industrial societies, which often appears so abstract one can only sympathize with anyone trying to use it in making a real decision.

In reviewing the volume, one cannot avoid wondering why the area-studies scholars took so long to begin to demonstrate the relevance of their work to both theory and policy. It may be that the more traditional cast of much work on the Arab world impeded this kind of effort. An equally satisfying explanation may be that the concentration on the pinnacle of power, especially in the fragile states of the Middle East, has been too readily transplanted from more fashionable comparative analysis dominating our research. Whichever view one finds comforting, the volume of Antoun and Harik suggests that the Arabists may have been too timid and the comparativists may have been asking the wrong questions.

DOUGLAS E. ASHFORD
Cornell University

Anarchism Today. Edited by David E. Apter and James Joll. (New York: Doubleday Anchor Books, 1972. Pp. 274. $1.95, paper.)

Edited collections of essays are invariably difficult to review, and *Anarchism Today* is no exception. Despite the efforts of David Apter and James Joll to tie the contributions together in a short introduction and conclusion, the volume has an amorphous quality which, in the end, is quite frustrating.

For one thing the book's focus is unclear. Both Apter and Joll are primarily concerned with the student movement (or the "New Left") which they see, in part at least, as a revival of anarchism with a difference. But while some of the contributors deal with themes related to this one, others develop either general or detailed studies of aspects of traditional anarchism in particular countries. The problem, of course, is that neither traditional anarchism nor the New Left variant of it are all of one piece. An attempt to study either movement would be difficult enough; a relatively brief volume with essays treating both movements, and sometimes their interrelationships, can easily become a potpourri.

In describing the relationship between traditional anarchism and the New Left, the editors perceive similarities in the attack of both on the whole apparatus of political organization *per se* The key differences seem to lie in part in the New Left's lack of concern with theory or history or both. Traditional anarchism, as compared to socialism, produced very little in the way of theory or good historical analysis, but the New Left (so the authors feel) is likely to produce even less, for the movement is self-consciously "anti-rational." More important, while the New Left can be identified with one wing of anarchism in its espousal of the propaganda of the deed, it nevertheless differs from traditional anarchism both in its "playfulness" and, in some cases, its view of violence as liberating. In fact, Joll feels that the New Left, consciously or unconsciously, is inspired by Sorel more than by any other classic theorist. After all, most traditional anarchists saw violence as a means toward social liberation rather than as a basis for self-fulfillment.

Finally, both editors contend that the New Left version of anarchism borders on "nihilism." Its purpose is less to create of a "free" political order than to end social roles of whatever kind. Needless to say Apter and Joll are less than fully sympathetic to that purpose, and Joll, at least, argues that anarchism in both its traditional and New Left forms can only be understood if one adds to sociological analysis some examination of the psychological makeup of the movement's adherents.

None of the essays in the volume really attempts this task, with the possible exception of Michael Lerner's discussion of the American student movement. His effort, however, is basically directed to giving readers a sense of the *feelings* of upper middle-class students (including his own) during the 1960s. Most of the other contributions are broadly historical or sociological. Some of them, such as Colombo's essay on anarchism in Argentina and Uruguay, are general histories of the anarchist movement in these countries. Others,

e.g., Mauria's "The Spanish Case," deal primarily with particular historical periods and problems. In both Spain and Latin America, of course, the anarchist movement has had a relatively long history. In countries where this history has been less significant, the emphasis of the contributors is on the New Left, sometimes in its relation to the anarchist movement, sometimes without even discussing anarchism. While some of this latter group of essays are broadly descriptive (e.g., Tsuzuki on Japan), others like that of Lerner on America or Gombin on France are primarily concerned with *meanings*, with Gombin focusing on new developments in working-class thought and action in relation to the events of 1968.

In general, the broadly historical essays are too brief to add much to our understanding, and the more specialized accounts too narrowly focused for undergraduate or even most graduate students. As for the interpretative essays, while they are well written, they are primarily concerned with repeating the conventional wisdom about post-industrial society, alienation from the self, etc., which has flooded our eyes and ears for the past several years.

For this reason perhaps the essays dealing with the history of traditional anarchist movements are the more satisfying group despite their limitations. At least they treat concrete historical phenomena in terms with which social scientists trained in the 1940s and 1950s can cope, e.g., social class, historical traditions and the like. Those who see the emergence of the student movement as related to the phenomenon of "postindustrial society" may be right, but other variables may also be at work, and they need to be explored in depth. *Anarchism Today* helps us little if at all here, despite Apter's skill as a comparativist. For example, the Japanese student movement has a long history, as do student movements in Latin America and France. On the other hand, for all practical purposes the American student movement is new, and students in the United States are certainly responding to very different kinds of perceived ills than are students in other countries. One would expect a volume like this one to throw some *comparative* light on the differences, and to aid us in understanding why, despite them, the ideologies of these movements resemble each other so closely. However no real effort is made to do so.

Perhaps more importantly, the development of the student movement in the 1960s is probably the first instance in recent history in which American ideas and attitudes have served as a spark for movements in Europe. Why is this so? It is hard, on the face of it, to believe that the issues which theoretically led American students to question their own society (superaffluence, Vietnam and the race issue) have the same salience in France, Germany, or Italy despite the rhetoric of the student leaders. Perhaps the answer is partly that during the 1950s the United States became something of a model for European youth. For better or for worse, young people began to see their own future as previewed by America, and to see themselves as part of a common culture of which America was the leading representative. Thus, they may feel that their own countries stand condemned by the "evil" in America even if they have not actively participated in that evil. This is only a possibility, but it would be worth exploring if only as a corrective to more apocalyptic visions. Worth exploring, too, is the very important role played by Americans of Jewish background in the early years of the American student movement, when they may have accounted for as much as 60 percent of the membership of SDS. (In France and England students of Jewish background also seem to have played a role in the leadership of the student movement far beyond what one would have anticipated from their proportion of the student population.)

These and other questions have yet to be tackled, and one suspects that they will not be dealt with until we gain more perspective. In the meantime the student movement in the United States and England at least seems to be relatively moribund. If Lerner, Keniston, et al. are right, why the sudden collapse? Or is this just the calm before another storm? The collection being reviewed here offers no guidelines.

My remarks so far have been rather critical. Let me note, however, that most of the contributions to *Anarchism Today* are of high quality, and that a number of the essays are quite useful for particular purposes. For example, the essay by Nicolas Walter on "Anarchism in Print: Yesterday and Today" is a very good annotated bibliography on the subject. Further, if the editors of the volume are less than fully sympathetic to anarchism in its traditional or New Left variant (they are certainly not completely unsympathetic), most of the contributors range from mildly to enthusiastically supportive. The combination makes for a very nice balance.

STANLEY ROTHMAN
Smith College

Asian Development: Problems and Prognosis. By John Badgley. (New York: Free Press, 1971. Pp. 210. $6.95, cloth; $2.95, paper.)

This is a first-rate work of political analysis which examines the problems that political development in Southeast Asian countries poses to the countries concerned and to the United States. I am sure the author himself would disclaim that the book deals with the problem of development in the whole of Asia. There are some very per-

ceptive comments regarding the political development of India and China, but the material from these two countries is used occasionally by the author in his attempts to explain or establish his central thesis relating to Southeast Asian countries. His central thesis is that most of the states in Southeast Asia (and the principal ones he deals with are Thailand, Malaysia, the Philippines, Indonesia, and Burma) can only be regarded as states by international courtesy. These countries in their precolonial state lacked key institutions like legal codes, secular organizations in metropolitan centers, bureaucracies, and universities. Then Professor Badgley tries to show that the legacies that may be discerned during the postcolonial period are not substantial in terms of legal codes, political parties, interest groups, bureaucracies, and universities. Impressed by western values and practices, political leaders in these countries have tried to build unitary state systems by invoking certain national values and aspirations. These attempts have resulted in alienation of important minorities, political violence, and eventually in the seizure of some of these systems by the military.

Professor Badgley keeps suggesting throughout the book that the central flaw in the analyses of writers like Karl Deutsch, Rupert Emerson, and Lucian Pye was that they tended to regard these developing polities as nation-states when both history and sociology clearly indicated that they were not. Like Professor Badgley, Samuel Huntington in *Political Order in Changing Societies* has also argued that these developing nations lacked some of the basic political institutions and that therefore, in order to overcome problems of rising frustration and social unrest, they should undertake systematic institution building. Huntington extolled Ayub Khan of Pakistan as the new "Solon or Lycurgus or 'Great Legislator' on the Platonic or Rousseauian model" for having built institutions like Basic Democracies, reorganized the political parties, and introduced a new constitution. Professor Badgley, with the hindsight available after the overthrow of Ayub and the formation of Bangladesh, might gleefully point out that his central thesis in the book has been confirmed, namely, that in these heterogeneous or pluralist societies imposition of ambitious nation-building straitjackets result in either political violence or disintegration. In other words, emphasis on nation building defeats or distorts political development. Therefore, Professor Badgley's sovereign remedy is "devolution of decision making, education, business activity, and cultural control away from the primate cities, that is Rangoon, Bangkok, Djakarta, Manila, Pnom Phen, Saigon, and Hanoi" (pp. 181–2). The author, however, does not appreciate that this remedy is a very complex and in some cases an almost impossible undertaking, fraught with possibilities for political disintegration. What political leaders with what perceptions and perspectives will be able to decide what local communities should receive what kind of stimulation or encouragement for purposes of institution building in local communities? How are these disparate or divergent efforts attending to disparate or divergent community needs to be coordinated under some kind of overall central planning and scale of priorities?

Writers of different ideological persuasion would argue that nation building in some of these societies has gone awry because the political leaders adopted without too much modification western strategies of economic development, namely, encouraging massive industrialization either through private enterprise or without attending to the problems of economic and regional redistribution. This brings us to a significant shortcoming in a book otherwise full of invaluable insights. Professor Badgley now and again concedes that the Communist efforts at nation building through their particular methods have been more successful, but he does not explain analytically the reasons for their success. Nor does he seem to come to grips with the question of what should be the appropriate American stance, given that certain nations are willing to embark on such experiments and prepared to pay the costs.

KHALID B. SAYEED
Queen's University, Canada

Revolution in Peru: Mariátegui and the Myth. By John M. Baines. (University: The University of Alabama Press, 1972. Pp. x, 206. $7.50.)

José Carlos Mariátegui is an important figure in the development of Peru's contemporary political ideology. Professor Baines in a well-written, well-researched study has combined a biography of Mariátegui, a review of Peru's political development during the early 1900s, and a thorough analysis of Mariátegui's writings to produce a definitive study which will be of value to all trying to understand contemporary Peru.

Mariátegui was a poor boy who never received much formal education. At 14 years of age he went to work as a copyboy for a Lima newspaper and from that humble beginning he soon became a proofreader, then a journalist, an essayist, and through his contacts at the newspaper he was drawn into the fringes of the group of young intellectuals who became known as the Generation of 1919. This was a period of ferment in Peru as the opening of the Panama Canal brought the country into more direct contact with the world. The Latin American Student Revolution came to

Lima; trade unions became active, and Mariátegui became involved in some of these activities. In 1919, he took a "scholarship" from the dictator Leguía, and went to Europe where he was able to study and formalize his thinking for four years.

Mariátegui was most influenced by the Peruvian, Manuel González Prada, Henri Barbusse, the French intellectual, Miguel Unamuno y Jugo, the Spanish thinker and Georges Sorel, the French Syndicalist. When he returned to Peru in 1923 he combined ideas from these men with his private version of Marxism to create a philosophy of revolution for Peru. By then his ideas were set, as Professor Baines points out, into a "fixed pattern" (p. 36): What Mariátegui produced was a combination of Peruvian nationalism with an idealized socialism. Mariátegui thought he was a Marxist, but his was an attempt to interpret the spirit rather than the letter of what he called "revolutionary Marxism." Mariátegui saw a religious character to Marxism; he thought revolutionary activity was a way of transforming a person into a new man. To him, revolutionary activity was more important than the achievement of the revolution.

Professor Baines tells us that through various periodicals of which the most important was *Amauta* (32 issues, 1926–1930), Mariátegui promoted his idea that Peru needed a revolution to create a type of socialism based on indigenous institutions preserved by the Indian section of the population through the centuries since the conquest. Because Leguía had exiled practically all of the young intellectuals who eventually were to create the Aprista movement, Mariátegui was according to Baines, "the most prominent oppotion leader left in Peru" (p. 51) to propagate the "Revolution." In the late 1920s Mariátegui split away from the bulk of the other young revolutionists who were creating the Aprista Movement and founded a Socialist Party which became affiliated to the 3rd International. Baines points out that Mariátegui was lavishly praised by the Russian and other Communists at this time as a great Marxian theoretician, not so much because of his contributions to Marxian theory, but because his mixture of Peruvian nationalism and idealized socialism fit the party line the Russians were pushing at that time. When the Russians changed their line, Mariátegui was out of step and was censured at the First Latin American International Communist Congress held in Buenos Aires in 1929. Had he lived he probably would have been openly attacked by the Communists but he died soon after at the early age of 35. But as Prof. Baines points out he "has become [for the Communists] an infinitely more valuable figure than he ever was alive" (p. 140).

The backward state of Latin American studies in the United States is reflected by the fact that it took 42 years before a biography of Mariátegui was published in the U.S. Mariátegui remains an important figure in Peru, and his ideas about "National Marxism" have been discussed all over Latin America. His writings are included in the series of books "The Thought of America" published in Mexico, which includes such figures as Alfonso Reyes, Rómulo Gallegos, Sarmiento, Arguedas, and Ingenieros. His book *Seven Interpretive Essays on Peruvian Reality* has been republished many times in all parts of Latin America, and an English translation was published by the University of Texas in 1971.

Mariátegui died just as the effects of the world depression helped to destroy the Leguía dictatorship and the Aprista, Communist and Socialist Parties began to function openly in Peru. All three claimed Mariátegui as their own, and since then his reputation has continued to grow as he became a symbol representing "Revolution in Peru."

Extensive footnotes to sources, a bibliography, which is practically complete for Mariátegui and three appendices increase the value of this book. It is recommended as a very good piece of research.

HARRY KANTOR

Marquette University

The Collapse of British Power. By Correlli Barnett. (New York: William Morrow & Co., 1972. Pp. 655. $15.00.)

Correlli Barnett, the British military historian, tells us that it took him five years to write this major study of the fall of Britain's power from her position of *primus inter pares* of the European great powers of the pre-1914 system to that of second rank or perhaps third rank today. We can well believe him. His book reaches back to the end of the Napoleonic War, when he thinks the collapse of British power began, and ends with the total British isolation of 1940. It covers almost every aspect of British life, from education to military organization, from literature to government. Its sources are varied: memoirs and biographies, state papers and reports of Royal Commissions, and include Cabinet documents recently made available in the Public Record Office in London. It is a crisply written, witty and rather sardonic book: British Ministers in the mid-1930s are presented as "like elderly Victorian cavalry officers trying to conduct a great tank battle," British business men of that period as "more interested in getting their golf handicaps down than their profitability up."

The book argues that the true sources of the decline of British power were not physical or

geopolitical (as perhaps were the sources of its rise) but psychological, or rather intellectual. It was, the thesis runs, the ascendancy among the nineteenth-century British ruling class of a persistent evangelical moralism and idealism which made the managers of British industry flabby, self-satisfied, and unenterprising, and made government ministers more inclined to the fostering of goodwill than the tough assertion of national interest. The rude truculence which gave Britain the mastery of a quarter of the globe degenerated more than a century ago into a soft, acquiescent cult of generosity to the other side which the realists of the Continent from Bismarck to Hitler exploited. The forcing houses of unrealistic evangelism, Barnett writes, were the public schools, the churches and chapels, instilling in the descendants of Palmerston and Canning the smug neo-Machiavellianism of the Sir Edward Greys with their maxim "to do the right thing is always the right thing to do."

Correlli Barnett says that this moralistic humanism, excellent as it may have been for the secure Britain of the 1850s, was unfitted for the challenging world Britain faced from the 1880s on, when first there was the industrial competition from Germany and the United States, later the naval rivalry with Russia and Germany, and then the well-nigh impossible task in the 1920s and 1930s of keeping order in the Far East against ambitious Japan and holding the balance between France and Germany in Europe at the same time. The main part of the book, "Covenants without Swords," a single chapter of 350 pages, recounts once more the dismal tale of British foreign and defense policy between the two wars and lists, with full documentary references, its many tragic blunders: the opposition to France and siding with Germany in the early 1920s, the strange belief that the signing of many declarations by many nations consolidates peace; the alienation of Mussolini in the mid-1930s by a League sanctions policy aimed against his attack on Abyssinia, "a primitive African state which counted slavery among its institutions"; and finally Neville Chamberlain's childish illusion that he had turned Hitler into a respectable man while Hitler was despising him as a senile old fool.

Barnett maintains that the physical effects of the First World War on Britain were much exaggerated and that the war's results were more psychological than physical. It was the spate of memoirs, novels, and plays about the miseries of the Western Front which appeared in the late 1920s, he contends, which filled the British middle classes with loathing for the trenches just at a time when strong national attitudes toward Germany were called for. But the miseries of war, he goes on, were essentially a middle-class experience. If the Siegfried Sassoons and Edmund Blundens had known anything of normal working-class conditions in England at the time they might have been as uncomplaining as the working class (according to Barnett) was during and after the war.

But it is when Barnett proposes alternatives to British policies of that time that those policies become harder to condemn. One such alternative is that Germany in 1919 should have been "divided and permanently weakened in the very springs of power" (p. 318). But could Britain, after so many years of fighting, have provided the troops for a permanent occupation? Again, Britain, he writes, should have made a deal with Japan in the 1920s which would "avert her hostility and if possible concentrate her ambitions on Manchuria" (p. 303). But why should Japan make a bargain with Britain when she could have it all her own way, and but for a fluke might have done? True, it is hard not to agree with Correlli Barnett that a great imperial power like Britain in the interwar years cannot act as though the international system is a mutual aid society. But these notions intensified rather than caused Britain's plight. The fact is that after 1871, certainly after 1890, Britain could no longer organize the European balance of power from her own resources; indeed, European power could not be balanced from its own resources. British "fair play" perhaps helped somewhat to civilize international politics, but those politics were shaped by deeper social forces of which mental illusions were as much the reflection as the source.

F. S. Northedge
London School of Economics and Political Science

City Politics and the Press: Journalists and the Governing of Merseyside. By Harvey Cox and David Morgan. (New York: Cambridge University Press, 1973. Pp. 159. $8.75.)

Yes, Virginia, there really are local newspapers in England, and not just those behemoths of Fleet Street and quality productions like *The Times*. The Cox and Morgan study concerns a half-dozen such local papers during the 1960s, all of them published within the Liverpool conurbation. (A useful word to recall, conurbation, whenever our British brethren twit us for employing social science jargon.) Perhaps their most salient characteristic is a heterogeneity in intended distribution patterns, and therefore in expected audiences, the Liverpool morning paper having a regional focus, the Liverpool evening paper a somewhat narrower metropolitan emphasis, and the Liverpool weekly a more exclusive concentration on complaints from certain working-class areas of the city, while the other three papers—*Wallasey News*, *Birkenhead News*, and *Bootle Times Herald*—mainly hold forth, once or twice a week, on happenings in

their particular communities of origin. Clearly, variability in treating local events and issues is not a mere consequence of ownership, since all but the Liverpool weekly are affiliated in one way or another, but instead reflects the functional (or dysfunctional) work that each paper performs within the overall Merseyside communicative system and that its editorial side sees, or feels, appropriate for it in its circumstances.

As usual, the title of the book is too ambitious and the subtitle a far better guide to its topic. Indeed, in a fairly crucial way, the title may be downright misleading, since most of the press coverage surveyed—in all but the *Liverpool Weekly News*—is about governmental actions, singly or in series, and far less is about expressed demands and supports among the populace at large. (See especially pp. 48–50 and 58–61.) Moreover, as those alert to nondecisions would quickly observe, suppressed demands go virtually unnoticed, exception made perhaps of implications in the "constraining factors" on reporting local affairs and in the suggested development of "a local equivalent of the national lobby-correspondent/political columnist system" (p. 148) on which the concluding chapter touches. It is upon certain chronological indicators of governance (council minutes, committee papers, annual reports, facility inaugurations, and the like) that the local papers and their academic analysts chiefly lean—and not on politics in its richer and more complex senses.

Although Cox and Morgan explicate and utilize a formal, if rather simple, kind of content analysis (chap. 5), the bulk of their factual presentation is discursively verbal. Besides copiously delineating case histories of "What the Papers Said" (chap. 6) in order to "make these dry [statistical] bones live" (p. 72), they also relate in summary fashion what they learned in interviews about how local editors view their role and the surrounding scene and how local politicians in turn evaluate the press (chap. 7 and 8). These four core chapters, whose substance almost wholly derives from the authors' having immersed themselves in nearby, readily available, and relatively inexpensive written and oral information, are (naturally enough) without footnote citations.

By contrast, the encapsulating chapters, which seek to place these data within an enhancing sociological and geo-economic context and to draw from them certain politically ethical consequences, contain precisely 100 secondary references. The explanatory theory drawn upon is that of media sociology. Most of this is unexceptionable enough. For example:

> A series of stages leads up to the central decision-act, and a series of outcomes flows from it. The process involves the interplay of individuals and groups at a variety of levels, and in a variety of structures. It involves, too, their perceptions, dispositions and resources, the constitutional framework and the power relationships within which they operate. Since communication is an essential element in the system, the output of the media may be examined as it reflects the different elements and stages of the process, and as it makes its own independent contribution to the play of persons, pressures and policies (p. 21).

Yet just as the press perhaps "gives local government a duller image than need be" (p. 26), statements like these may make theory a blander farrago than it need be, too.

The conclusions of the study are suitably modest, urging a more interpretively informative press—"not more reportage of specific events but general discussion of their significance in the light of local government trends and community changes" (p. 150)—accompanied by a renewal of local democracy. Indeed, as befits both the subject itself and the analytic methods brought to it, the entire presentation is modest throughout. The only immodest attribute of this slim volume, in fact, is the price which Cambridge University Press, following its recent custom, has chosen: nearly six cents a page for a book without graphs and with footnotes clustered at the back.

MORRIS DAVIS
University of Illinois at Urbana—Champaign

Modern Revolutions: An Introduction to the Analysis of a Political Phenomenon. By John Dunn. (Cambridge: Cambridge University Press, 1972. Pp. xiii, 346. $14.50.)

The explosion of radical and aspirant revolutionary episodes which shook many parts of the world in the late 'sixties has subsided, but we are now experiencing some of the academic fallout in a spate of books analyzing the revolutionary phenomenon by one means or another. The approach adopted by the present writer, of wrapping a series of essays on specific twentieth-century revolutions inside a general introduction and conclusion, has of course been adopted by a number of previous scholars, the most successful being Eric Wolf. It is far from an easy undertaking, demanding the capacity to extract from a great mass of detail those aspects of the revolutions selected which appear to express both common characteristics of modern revolution and the discrete features of each particular upheaval, and then to draw all the strands together within the framework of an overall conceptual schema.

Professor John Dunn has taken eight case studies—Russia, Mexico, China, Yugoslavia, Vietnam, Algeria, Turkey and Cuba—as the corpus of his book, and endeavored to relate them to a broad framework of explanation. He is strongly critical of comparative studies of revolution in general, regarding them as "evasive" in the face of "the actual character of a number of

twentieth-century revolutions" (pp. ix–x), and believes that by concentrating on the understanding of causality his book "may make it slightly easier for sociologists or political scientists with an interest in revolutionary phenomena to develop greater delicacy in their grasp of the intricate problems of specifying precisely what aspects of what revolutions they wish to explain and thus of relating theoretical categories to historical social phenomena" (p. x).

One can readily agree with him that the study of revolutions leaves a lot be to desired, especially in its tendency to promote one dimension of a highly complex series of processes as the answer to the theoretical problem of grasping the logic of those processes. Dunn's critical comments on Chalmers Johnson's *Revolutionary Change* are appropriate in this context, even if they have been anticipated by others before him. But it would be little short of amazing if Dunn were to succeed, within the span of a collection of essays none of which runs to as much as thirty pages, in shedding substantial new insight upon revolutionary occurrences over each of which in its own right gallons of ink has been spilled. In fact, in comparison with other works of similar intent, Dunn's analysis of specific revolutions is decidedly disappointing. His essays do not come anywhere near coping with the problem of causality upon which he undertook to concentrate. Where we would expect to be focused upon the dynamics of national and social breakdown under defined stresses, we are all too often given discursive accounts which, without any clear explanation of the rationale, accentuate in one case the significance of ideology, in another the importance of organization, and in yet another the salience of class antagonisms. Where the author does take the socioecononomic background as his focus (as in the case of Russia, Mexico, and China), the result is the more satisfactory, though of necessity the treatment is partial and terse. In other cases and especially those of Vietnam and Cuba, there is nothing like an adequate indication of the social context in which the revolution occurred. There are often interesting sidelights upon local conditions, but they remain sidelights and do not convey a coherent grasp of the revolutionary process in its totality.

It is notable that in no case is the actual revolution itself dealt with in any depth; mostly it is ignored completely, while in other cases only one part of it is enlarged upon, as is the Zapatist facet of the Mexican revolution. This cannot help but buttress determinist explanations of revolutions, since the subjective and voluntarist factors are passed over. This omission is all the more surprising in view of Dunn's stated view of the components of the revolutionary process, namely, that they consist of the quality of the revolutionaries, the communication of the relevance of their project to the lives of substantial numbers of people, and the failure of social control (pp. 234–39). The scant attention given to the first two components, while precious space is wasted on marginalia such as assimilationism in Algeria or slavery in Cuba leaves his project stranded without the legs it should be walking on.

It is perhaps not surprising in these circumstances that the ultimate conclusion that Dunn arrives at is that modern revolutions are all about nation building in modern conditions, and therefore "simply because styles of integration of national economies and societies into the world economy are experienced for the great majority of nations in the world today . . . as essentially inimical to the maintenance of local identities and the preservation of local interests, there are going to go on being revolutions" (p. 253). I find nothing to cavil at in this dictum, but in itself it does not take us very far. If Dunn had combined this generalized conclusion with a real attempt to apply his three components of the revolutionary process to the case studies, he might have come up with a worthwhile contribution to the anatomy of revolution. As it is, we are no nearer to answering the essential question: Why is it, given the widespread conditions which promote revolutions, that some countries which suffer these conditions have experienced or will experience revolutions and others will not? An "introduction" which performs neither the function of providing basic information nor that of elaborating a reasonably plausible theory, and which in addition is written in a difficult stilted style, does not really fulfull its purpose.

REX MORTIMER
The University of Sydney, Australia

Administration and Development in Malaysia: Institution Building and Reform in a Plural Society. By Milton Esman. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 341. $12.50.)

This book is solid testimony to the value for scholarship of professors engaging in technical assistance endeavors. Milton J. Esman's introduction to Malaysian governmental practice came when he and Professor John Montgomery were invited to survey public administration there and propose reform, and when he was invited to serve as adviser in the implementing of their report. From this intense first-hand experience came a deeper and more sensitive understanding of the complex ways in which politics and planning for development impinge on each other in a racially divided society. The ultimate payoff is a scholarly study which can be recommended to all who are interested in the politics of ethnicity, political development, and development administration.

In recent years scholars have debated about

whether the colonial experience may not have seriously distorted the political modernization process in many African and Asian societies by grossly favoring the strengthening of administrative institutions and suppressing the development of popular participation channels. According to one view, further efforts to strengthen public administration only hampers democratic development. Esman does not equivocate, sensitive to the explosive potentialities in the relationships of Chinese and Malays, he argues that "... a society whose main problem is the management of communal conflict and whose secondary problem is the guidance of far-reaching social and economic change is likely to be better served if its authoritative institutions are stronger than its participative institutions and if the former are able to discipline the latter" (p. 6).

In presenting the social and political background to the current Malaysian context for development administration, Esman has written a vivid and truly masterful summary of the issues and problems of that divided but always potentially promising land. His description of the Malaysian administrative system is not as detailed as Tilman's *Bureaucratic Transition in Malaya*, but it focuses on the critical elements of administration and provides an earthy and realistic perspective that goes beyond anything to be found in the previous literature.

I found most interesting Esman's chapter on the value and belief patterns of Malaysian administrators, which is brilliant in its descriptions but faulty in its explanations. Esman's findings agree with those of James C. Scott's *Political Ideology in Malaysia*, and he accepts Scott's explanation that the orientations of Malaysian officials stem from the "realities of scarcity" rather than from any deeper cultural experience. This explanation is not convincing because Esman fails to explain why Malaysian high officials, many from aristocratic families and all with good and relatively expensive educations, should be so hypersensitive to "scarcity" and products of a "culture of poverty." Furthermore, he acknowledges that there are gross differences between Chinese and Malay orientations, which is an admission that cultural considerations are ultimately the critical ones. As everyone knows, different people react quite differently to the same "reality of scarcity"—some by becoming passive and cautious, others by being aggressive and quick-witted.

The bulk of Esman's book is his analysis of administrative development and the management of communal conflict. Starting with an acceptance of the Melson-Wolfe hypothesis that modernization will intensify ethnic conflicts in a plural society, Esman charts the possible strategies for national development in Malaysia. He points out that Malaysia once relied upon an "avoidance model" for conflict management, but after the riots of 1969, the society had to move toward a "mutual deterrence model," which Esman feels must in time give way to a "guidance model" in which governmental polities can insure that the proper balance of well-being and power is maintained between the communities.

Esman's conclusion in support of authoritative institutions raises both a factual and a theoretical issue. Some students of Malaysian society will argue that Esman has overlooked the extent to which the organization of politics rather than the role of administration was, and in the future is likely to be, critical in managing communal tensions. In their view the character of the Alliance party and the informal understanding among many of the top politicians of all communities helped ease racial tensions, and it was a breakdown in politics, not administration, that provided the fuel for the riots of 1969.

This difficulty in determining the relative importance of the political as contrasted to the administrative system suggests that there may be something wrong with a theoretical formulation which establishes "institutionalization" and "participation" as polar opposites. Why has there emerged in political development theory this notion that there must be one or the other, and not both or neither? Indeed, the Asian experience suggests that optimal conditions for domestic conflict management and effective development require a strong administrative framework and a solid core structure for organizing participation, with only marginal room for partisan controversy. From Japan with its "one-and-a-half party system" and its powerful establishment based on an elite bureaucracy capable of working closely with industry, to India with its dominant one-party system and its traditions of I.C.S. administration, the most successful development pattern has been a parallel institutionalization of both participation and administration, with both directed toward encouraging a sense of advancement. Room for some degree of partisan competition is important, but matters vital to domestic development have to be shielded from whimsical currents and passing emotions. In short, the building of effective authority can go hand in hand with expanded participation if each involves the development of more capable institutions.

The Malaysian race riot of 1969 was not so much a case of participation getting ahead of authoritative institutional development as it was a breakdown of the institutions for participation. Undoubtedly, more productive and more far-sighted public administration can help create an environment which will reduce the likelihood of such breakdowns—and in my judgment Milton J. Esman has outlined what is called for in this

domain—but the institutions for participation also call for independent attention. Too often political scientists, according to their personal preferences, claim too much for the advantages of authoritative institutions or popular participation and thereby fail to appreciate the separate problems and potentialities of each.

In the Malaysian case we now need a study of possible programs for the development of participatory institutions which can reinforce and not compete against this admirable study by Esman of administrative development.

LUCIAN W. PYE
Massachusetts Institute of Technology

Urbanization, Planning and National Development. By John Friedmann. (Beverly Hills, Calif.: Sage Publications, Inc., 1973. Pp. 351. $12.50.)

"We ought to live where all th' good things iv life comes fr'm," says Hogan. "No," says I. "Th' place to live in is where all th' good things iv life goes to." Ivrything that's worth havin' goes to th' city; th' counthry takes what's left . . . th' sunburnt viggytables is consumed be th' hearty farmer boy an' I go down to Callaghan's store an' ate th'sunny half iv a peach. Th' farmer boy sells what he has f'r money an' I get th' money back whin he comes to town in th' winter to see th' exposition. They give us th' products iv th' sile an' we give thim cottage organs an' knock-out dhrops, an' they think they've broke even. Don't have annywan con-vince ye th' counthry's th' place to live, but don't spread th' news yet f'r awhile.

So said Mr. Dooley two-thirds of a century before John Friedmann, though the latter says it differently.

Cities, Friedmann and Dooley say, have a decisive role in national development (Chapter 1). They are centers of innovation (cottage organs and knock-out drops); they are centers of wide opportunity (often at the country's expense); they facilitate the emergence of counterelites and a style of interest-group bargaining and compromise (Mr. Dooley, Callaghan, and the farmer boy). Development is polarized (Chapter 3). "Core regions are territorially organized subsystems of society which have a high capacity for innovative change; peripheral regions are subsystems whose development path is determined chiefly by core region institutions with respect to which they stand in a relation of substantial dependency" (p. 51). Alas, don't spread th' news yet f'r awhile! Thus Friedmann's main policy prescriptions are: promote urbanization and emphasize interlocking core-periphery subsystems for national and regional planning.

John Friedmann is a distinguished regional planner at home in economics, geography, demography, urban studies, and sociology. He joins these literatures here, emphasizing regional spatial planning. In his most theoretical chapter (3), he seeks "to formulate a theory of the development process in its spatial dimension." Consequently, the book stresses core building (urbanization and regional formation and growth) and core-periphery relations. We learn rather less about processes within existing cities and regions. Friedmann is at his best when he emphasizes the often neglected spatial dimensions of change. Friedmann the regional planner and spatial theorist is often interesting and thought-provoking. Friedmann the political analyst is less so. The following partial review addressed to political scientists emphasizes political issues.

Chapter 2 restates what political modernization scholars have come to call the "social mobilization hypothesis," associated with such scholars as Karl Deutsch and Daniel Lerner. But Friedmann places all his eggs in the urbanization basket (without acknowledging cross-disciplinary links). He takes no account of the writings by Nie, Powell, Prewitt, and Inkeles in this *Review* which show that the independent contribution of urbanization toward explaining modernization (including political participation) is, at most, modest. Friedmann has not reflected sufficiently on alternative variables (education, political organization, industrialization) with greater independent explanatory power.

Chapter 4 studies "hyperurbanization" (a prolonged condition of super-heated growth) in Chile. Friedmann's evidence here suggests, however, that Chile was urbanizing at a slower rate than most of the less developed world. Other evidence indicates that Chile's urbanization rate has been slower than that of other Latin American countries for decades. If Chile's urbanization rate is independently responsible for the country's economic middle-development, and if "many countries, including the United States, have experienced considerably shorter (urban) doubling rates over extended periods of time" (p. 91), then no country would be economically developed today. Friedmann's explanation of Chile's recent political difficulties through the hyperurbanization hypothesis, too, takes no account of plausible alternative explanations (rise of political participation and organization). One could argue a precisely opposite case: Chile's rate of urbanization had been moderate enough (comparatively and through time) that governments could handle it. Had other variables and events not intervened, the Chilean rate of urbanization was politically tolerable.

Chapter 9 persuasively argues that statesmen can learn much from regional planning theorists to inform their policy decisions. Friedmann discusses broad policy issues through a dialogue between a hypothetical head of state and his minister

of national planning. Poor Machiavelli, had he given such political advice to his Prince! The head of state faces opposition criticism because the capital region has benefited most from development, draining the periphery; the South threatens secession. Friedmann counsels literacy and urban growth promotion in the potentially secessionist South, the stimulation of "centers of gravity" there, in short, "modernization in the periphery" (p. 167). In contrast, political modernization studies emphasize with rare consensus that modernization is likely to exacerbate such already intense conflicts. This was already happening in Friedmann's hypothetical country. There is also no discussion of the Chief Executive's political bases: If he shifted resources, would he gain enough support in the opposition's turf to balance his probable loss of support in his apparent home base (the capital)? Friedmann's policy advice is insufficiently informed by political research.

Thus Friedmann at his best is still too apolitical. The first part of Chapter 10 is an interesting study of central Chile. Its second part proposes new politically relevant policies (strengthen organized labor, weaken industrial managers, strengthen middle-level cities, weaken central government), but without any political analysis. Could a government elite afford Friedmann's advice? In a fascinating comparison of regional development in the Tennessee Valley and in Venezuela's Guayana region (pp. 275–276), politics are assumed, or stated as outcomes (such as low political participation in Guayana), without more ado. We ache to learn about political support, opposition, stability, and decision making.

Chapter 6 hypothesizes that "local communities differ in their abilities to respond to opportunities in their environment in ways that will promote their economic growth, and that these differences vary systematically with certain characteristics of these communities" (p. 127). This is considered to have been proved later on (p. 246). Yet Friedmann's own evidence—acknowledged reluctantly—indicates that centralization of national power is a more decisive variable. It cancels out the independent explanatory power of the local community variable. Nevertheless, the research emphasizes the challenged hypothesis, and considers political centralization less.

Chapters 1, 7, and 13 state that there is no adequate theory of urbanization. Chapter 3 presents a "theory of polarized development," and Chapter 12 states "an adequate body of theory exists" (p. 251). After Part I ("Theory"), the author warns about forthcoming chapters ("Policy" and "Practice") that "the failure of terminology to correspond precisely to the notions set forth above is owing to the general lack of an established consensus on the meanings of these terms and the fact that the chapters were written at different times for different occasions" (p. 135). The book would have been far more satisfying if, instead of a collection of theoretical assertions at a high level of generality (and Mr. Dooley would have enlivened the tone!), in Chapter 3, followed by essays on diverse and loosely connected topics on theory, policy, and practice, the author had explicated theoretical propositions about urbanization and regional development in the light of evidence in a book-length discussion. A touch of more systematic political analysis would have been welcome, too—and necessary.

JORGE I. DOMÍNGUEZ
Harvard University

The Politics of Urban Development in Singapore. By Robert E. Gamer. (Ithaca, New York: Cornell University Press, 1972. Pp. 263. $15.00.)

Singapore has been a planned city ever since Sir Stamford Raffles arrived in 1819. Many of the central sections are reclaimed from saltwater bogs. Professor Gamer's superb book shows a keen awareness that any organized development or reclamation project operates in a social context. The value of any such enterprise lies in its effect on the human world, and not a bit in its abstract form as a plan.

Gamer's intention is comparative, general, and moral. He found in Singapore's Kallang Basin (as Herbert Gans earlier found on Boston's West End) that redevelopment disrupts economic and social patterns which many inhabitants of slum areas say they like. In some ways, this book is supposed to be a policy study. It criticizes existing procedures and suggests reforms. The most comprehensive recommendation is that Singapore's planning agencies must try much harder to assess the good aspects of what they intend to destroy in the modernization process. Only then can they measure whether specific plans will make for net improvement or not. On p. 65, for example, he asks whether a housing project may actually depress some living conditions, whether it is likely to increase extortion by criminal groups in a neighborhood, whether it will make young people study their homework less well, whether cottage enterprises will be inhibited, and whether the residents of the area are not already "content with their existence."

It is easier to pose such questions than to answer them. With this latter task, Gamer runs into his main difficulties. He tries to plumb the motives of urban ex-peasants who live in reclamable areas. Why don't (or shoudn't) these people want their lives to be modernized? Maybe it is still enough to insist on the importance of

this question without going very deeply into the value problems it implies. That is more than most studies do. But the conclusions of the present book suffer from a reticence to talk as if the available ethics in Singapore had structures, which can be compared as surely as can other social facts that our positivist science admits with greater tolerance.

Specifically, it is not always clear just what Gamer is defending against the encroachments of modernity in Singapore. He has a firm sense of the importance of ethnic divisions in that city, and he occasionally refers to local cultural traditions. But this book is not exactly a plea for the moral maxims of Aw Boon Haw (depicted as they are in dioramas at the Tiger Balm Garden, "behind the artificial mountain containing the bloody war between the rats and the rabbits," p. 5n). Nor is the book written on behalf of any Indian guru, nor for the peace of Malay farm life. Cultural insights (e.g., about garden parties and racial mixing on p. 19) are usually hidden away in footnotes when they are offered at all.

Babel is partly to blame for this problem. Gamer relied on English sources for his documentation; so he had largely to trust the products of an ex-colonial education system. His thesis might have been strengthened, and more fully dramatized with examples, if he had delved into the large available files of non-English Singapore newspapers. The effects of this omission were lessened by his own surveys and unusual perceptiveness. Still one can question any rigid distinction between "Chinese-speakers" and "English-speakers." Actual language use may be better correlated with situations than with persons, and these categories anyhow mesh better with a belief in developmental dualism than with Gamer's thesis about the uncertain effects of planning. Most "English-speakers" in Singapore are Chinese or Indian in background, and there is a spectrum (not a two-part division) of the degrees to which they like selected parts of their ancestral cultures now. Singapore groups may object to further modernization on the basis of transformed traditional values, and Gamer's argument would be more complete if the book could show these viewpoints better.

Tradition is only the first line of defense in this book against redevelopment. Gamer's liberalism also comes into play. On p. xxiii he specifies that stable environments should be created "for individual citizens." P. 192 suggests that the political structure should serve "personal needs." Yet in other places (e.g., p. 204), Gamer sounds like a communitarian. He comes out for strengthening small groups within the big city; but this hope is quickly dispelled by his realistic fear that new intermediate powers would then organize these small communities, and the new leaders would stultify individuals' desires just as surely as government bureaucrats now do. Trying to solve this dilemma between social cohesion and efficient leadership, he concludes (p. 207) that, "It is a riddle."

Existential uncertainty may even become Gamer's main cause. He defends Singapore against the smugness of technocrats who manipulate people's futures. He agrees with a UN report that this dense economy and society should be "treated with something more subtle than a steam shovel." But clearly he does not think it should not be treated at all. Social conscience, and a desire to organize more physical and political resources for poor city dwellers, effectively prohibit that. Gamer says, "Bureaucracy is keyed to dealing with only pieces of human life. . . . Industrialization has brought in its wake much economic suffering and social decay" (p. 206). As an antidote to excessively certain, specialized progress, Dr. Gamer's main prescription seems to be an open mind.

And half a teaspoon of revolution: "The world of factories and mines and plantations may be working at cross-purposes with the world where the majority of the earth's people live. As cities like Singapore and Kuala Lumpur progress in modernization, the possibility diminishes that their governments would ever desire to backtrack and take the road of Havana. So the gap widens as industralization progresses" (p. 189). If we do not ask too specifically about actual plans in Havana or the general popularity of backtracking, this statement still organizes some facts. Modernization is a painful, unsettling, and oppressive experience for most members of the societies where it occurs. But it is difficult to prove that "the poor will never achieve the power needed to live in big houses and dine regularly on fatted calves" (p. 192). They may eventually cease to be poor; and power may then not be handled in such a zero-sum way—even if these facts do scant little to help their present plight.

This book is not written at the fury level of Elsie Elliott's *Avarice, Bureaucracy, and Corruption in Hong Kong: Volume One*. It shows more ambivalence about development, about the impetus to plunge on with plans despite uncertainties in the result. Maybe a Carlo Levi novel, or the *Bhagavad-Gita*, or Nair, Hobsbawm, or Lanternari, could be included in the long and useful bibliography. This book should be read by all students of resettlement housing. It will be of interest to political scientists who study large cities where planning takes place. Finally, it is a primary source for anyone looking into post-technological values. There can only be agreement with the report on p. 127 that, "Many questions remain

unasked and unanswered. One searches in vain for men with a broader vision. . . . " But some fine beginnings have been made.

LYNN T. WHITE, III
Princeton University

Competition and the Corporate Society: British Conservatives, the State and Industry, 1945–1964. By Nigel Harris. (London: Methuen & Co. Ltd., 1972. Pp. 384. $12.75.)

The title of the book leads one to expect a discussion of the Conservatives' economic policy—in particular their attitude toward managed economy and the welfare state, and the industrial relations, and their informal relationship with the big business. But the book is mainly a discussion of Conservative economic thought in the period between 1945 and 1964.

Several interesting points emerge from Mr. Harris's account: The first is the great diversity of opinions expressed by the Conservatives. Mr. Harris gives the impression that the opinion expressed within the Conservative Party covered almost the entire spectrum of political opinion in Britain. There were neoliberals, the radical corporatists, and the pluralist corporatists. The neoliberals harked back to the classical economy—to nonintervention, competition, and market mechanisms. By contrast, the radical corporatists pressed the case for expert planning and maximum state control of the economy. The pluralists occupied somewhat the middle ground between these two extreme positions, but they were at times indistinguishable from the neoliberals.

Mr. Harris, however, has not systematically spelled out what cluster of attitudes characterized each of his three categories—neoliberalism, pluralism, and corporatism. For example, he rightly points out that corporatism was no single unified doctrine and that there were many versions of this as it derived from various traditions such as those of Saint-Simon and Robert Owen. (He does not, surprisingly, mention the Marxist tradition.) But he does not clearly indicate the limit of this corporate thinking within the Party. On the other hand, he tends to give the impression that the advocates of corporatist thinking in the Party were at times more radical than those in the Labour Party. Surely no sensible conservative spokesman has ever taken the orthodox socialist position. This failure to define precisely the three categories and select representative spokesmen for each category and deal with their thought in detail and depth seems to have made the discussion general and diffuse.

The second interesting point highlighted by the book is that there was very little correspondence between the opinions expressed and the actual policies pursued by the Conservative governments. Even *The Industrial Charter* (1947), which was the Conservatives' response to the Labour Government's corporate and welfare measures, did not play any explicit role in the Conservative electoral campaigns. Nor were the Conservative governments at all influenced by the notion of "property-owing democracy" which Eden, Macmillan and others publicized with much fanfare as an alternative to Labour's so-called state socialism. Mr. Harris points out that the two parties differ more in rhetoric than in the policies they actually pursued when in power. He goes on to say, "an economic history could present a coherent picture of Government policy before and after 1951 without mentioning the change of government in order to explain what happened" (p. 135). And this, indeed, has been done by several authors such as Andrew Shonfield and Samuel Brittan. One therefore wonders whether Mr. Harris should not have set out right at the beginning the consensual nature of British politics and then organized the discussion of the Conservatives' intra-party debate in the context of economic policies pursued by the Labour and Conservative governments.

The third point that emerges out of Mr. Harris's discussion is that the Conservatives did not have a clear and coherent ideological position. They did not spell out what they wanted to preserve. Nor were they explicit about the sort of society that they were looking forward to. Mr. Harris correctly suggests that this lack of a coherent ideological position was partly due to the inherent difficulty in ascertaining which way society was moving at a time of rapid social change; it was also partly due to the coalitional nature of the Party and the reluctance on the part of the leadership to commit the Party to any detailed scheme which could prove embarrassing later.

In addition to being general and diffuse, and at times sweeping and obscure, the book lacks a proper perspective. There is no rigorous and sustained discussion of what set of basic attitudes characterized British Conservatives as distinguished from Liberals and Labourites. And this should not have proved difficult as in recent years several scholarly works have been published on 19th century Conservatism, in particular on Disraeli and tariff reforms controversy in the Party. Because of the failure to take serious account of these works, the author has mistakenly given too much prominence to Disraeli. But the fact is that the Tory Party that Disraeli led was a Peelite party rather than a Disraelian party. (Mr. Harris seems to have realized this shortcoming and has made an effort to overcome it in an appendix.)

Despite these serious limitations, the book, read along with Andrew Shonfield's *British Economic Policy Since the War* (1959) and Samuel Brittan's

Steering the Economy: The Role of the Treasury (1969), could be useful as it also highlights at least indirectly the consensual nature of British politics and raises implicitly the important question of the relevance of party ideologies in understanding the actual performance of British government.

S. C. GHOSH
The University of New South Wales, Sydney

Popular Government in an African Town: Kita, Mali. By Nicholas S. Hopkins. (Chicago: University of Chicago Press, 1972. Pp. 246. $9.50.)

This is an admirable example of the growing literature on local politics in contemporary Africa. Hopkins gives us a detailed and spirited analysis of political life in a moderate-sized rural center in mid-1960s Mali, a poor West African country whose central government was vigorously pursuing a "revolutionary socialist option." The book concentrates exclusively on the town of Kita and its immediate environs, but Hopkins fully intends that his study should "cast light on other parts of Mali . . . and thus on the whole of the country" (p. 226). Under Hopkins's skilfull inquiry, the town proves an ideal microcosm.

As a social anthropologist Hopkins writes with refreshing disregard of most of the conceptual apparatus political scientists have brought to bear on the politics of underdeveloped countries and thereby teaches us much about the utility (or lack thereof) of our standard formulations. Hopkins begins by taking Kita, an evident arena of political activity in which men try to regulate their public lives; he then looks for the relevant boundaries, institutions and external links, and asks who is active in what ways, with what goals, using what strategies and operating under what constraints. This procedure leads him quickly to eliminate "ethnicity" as an important political variable. Kita is a multi-ethnic town, but the overriding identity is geographic. One is or is not a Kitan; under that level "solidarity based on residence or religion is more relevant for identity than the real or imagined differences between the various ethnic groups" (p. 52). Overall, an individual's site and length of residence, education, job, and personal abilities and connections are what count politically. Of ascriptive characteristics, only sex is of lifelong political importance. "Tradition" in the ideal sense of being opposed to "modernity," is just not a useful explanatory concept. For one thing, as Kita's history has shown, "in the political realm the introduction of new structures tended to imply the abandonment of the old" (pp. 16–17). In Kita, as in Mali as a whole, politics repeatedly involved a confrontation between those espousing "pragmatic" versus "ideological" or revolutionary approaches to political problems, but there is no useful sense in which one or the other of these approaches or the individuals espousing them could be considered more or less "traditional." Rather, these are personal orientations to political life, in kind not distinct from those found in most other countries, which in turn suggest strategies for political action and themselves become more or less attractive as a person's situation, constraints, and opportunities change. As Hopkins concludes, "The explanatory model that works best is one that assumes that each individual in the [Kitan] political field was able to decide what to do on the basis of his own personal interpretation of his advantage, and acted accordingly" (p. 219).

In his title Hopkins adopts a Malian political cliché, *gouvernement populaire*, and shows the basic sense in which the cliché is accurate. Effective and legitimate government in Kita and most of Mali for many, many years, has meant participatory government. Like all surviving political "traditions," participatory government is open to instrumental use at the same time that it sets limits on people's power. As Hopkins notes, "The legitimate Malian technique for obliging someone to do something is to make sure that that person participates in the decision to act" (p. 223). Hence, the frequent reliance on the ultimate organizational weapon, the mass meeting. Everyone attending one of these could be held responsible for the final decision, even if his role had been to question its wisdom; the cautious and calculating would thus contrive excuses for absenting themselves from meetings whose outcomes they feared. Hence also the flowering of political and parapolitical organizations: the various levels of the single political party organization; the civil service; the government-sponsored cooperatives; the youth movement with its middle-aged officers; the Vigilante Brigades; the athletic and theatrical groups, etc. Over all of these the Party held titular leadership, but it could ignore the strong desires of other affected organizations only at the peril of its own long-term effectiveness.

Political competition occurred as much within as between organizations and when left to develop inevitably took the form of two opposing factions each built around a powerful individual. This factional structure is characteristic of the Western Sudanic area and underlies the politics of regimes of widely varying ideology and purpose. Kitans would react to agents of the central government in part with a view to how their presence and policies would affect the ongoing local structure of political competition. Kita and Kitan politics thus posed a sharp challenge to the central government and its policies of rapid socialist transformation. While the local tradition of popular govern-

ment fitted well with the central government's rhetoric and its sincere desire to involve the population in the execution of its plans, it also meant that the Kitans simply were not prepared to accept direction thrust upon them from the center. Political abstention, individual passive resistance to governmental policies, and subtle encouragement of the short-term payoffs from shifting one's factional allegiance greeted increased central government pressure to conform. When, in 1968 a military coup overthrew the government at its center, the government had long since lost legitimacy in most of Mali's Kitas.

Micropolitical studies, particularly when they are done with such skill and sensitivity as this one, enrich our understanding of African politics enormously. The monographs on the new African politics which appeared in the mid-1960s and concentrated on the center and the national level of government, can now be appropriately revised, thanks to our greater understanding of what in fact was going on out in the boondocks. Obviously, in Mali as elsewhere the analysis of national politics can never be reduced to amalgamating the micropolitics of all the country's Kitas. One finds, for example, no trace of the military in Hopkins's Kita. Perhaps the least explored and most difficult subject is the complex interaction between local and national politics. Bamako was a major actor in Kitan politics, but in its own way Kita was a major actor in the politics of the capital. Analyzing the links and conflicts between the two and the political uses people made of them at each level is a task that remains to be done. Anyone attempting it, for Mali or any other political system, can profit from reading this exemplary study of local politics.

WILLIAM J. FOLTZ

Yale University

The Arabian Peninsula: Society and Politics. Edited by Derek Hopwood. (Totowa, N.J.: Rowman and Littlefield, 1972. Pp. 320. $11.50.)

The Changing Balance of Power in the Persian Gulf: An International Seminar Report. By Elizabeth Monroe. (New York: American Universities Field Staff, 1972. Pp. 79. $4.95.)

The Monroe book, with its assessment of increasing industrial world dependence on oil, considers the implications of The Report of an International Seminar at the Center for Mediterranean Studies (Rome) on which it is based as critical in the extreme. It is not difficult to see why as the Arab oil boycott, prompted by the 1973 October War, precipitated the "energy crisis," a now familiar label for a phenomenon that was virtually upon us at the time this book was compiled and that will be with us into the next century.

"A nation that is not dependent on oil is not a great power," according to one commentator (Monroe, p. 4). Perhaps this may not be true fifty years from now, but its truth for present-day Western Europe, Japan, and the United States is knowledgably summarized and interpreted by Elizabeth Monroe, Fellow of St. Antony's College, Oxford and author of *Britain's Moment in the Middle East* (1963). Western oil consumption (unfortunately) and Persian Gulf oil production (fortunately) have been badly underestimated, says Monroe. Americans, whom former Interior Secretary Udall once labelled as "energy pigs," now require for example what were the 1980 estimates. In the past the United States relied on its own and Western oil production, but today's figure of only 10 per cent oil imported from the Eastern hemisphere will grow to 50 per cent by 1980, notwithstanding my reading of Nixon administration energy policy.

There are implications in these few pages for the American balance of payments, overseas investment, overseas military commitments, our historic friendship for Israel, and even tourism. Alternative energy sources—nuclear, solar, geothermal, hydroelectric, coal gasification—cannot be substantially relied upon until about the year 2000 if then. Until adequate substitutes are found and developed, says Monroe, the flow of oil must be maintained, principally from the Middle East with Saudi Arabia as the swing producer. Americans, consuming one-third of the world's energy, will by 1980 face an annual bill of $70 billion for external oil. Demands by O.P.E.C. (Organization of Petroleum Exporting Countries) particularly Saudi Arabia, with the world's largest proven reserves, but also Iran and Libya, are for participation all the way to the pump. These demands pose political as well as economic questions, for Western Europe and especially Japan have few alternative sources or hydrocarbon reserves. The United States (Alaska included) even by estimates more optimistic than those reported by Monroe, has only ten years worth of oil reserves if it continues to consume at an exponential rate.

What is the political, social and economic impact of increasingly massive oil purchases on the producer countries? A background for Monroe's analysis is found in Derek Hopwood's collection on the Arabian Peninsula, which is increasingly important as an energy supplier. Hopwood suitably organizes papers presented by British, Arab, and American scholars at the Centre of Middle Eastern Studies at the School of Oriental and African Studies, University of London, and the Middle East Centre of St. Antony's College, Oxford during 1968–1969. These papers show how past circumstances condition much contemporary political behavior in the Middle East. In identify-

ing forces for social change in this book's four sections, one can trace their integrative and disintegrative potentials.

History: Ahmad Mustafa Abu-Hakima limits himself to certain aspects of development while stressing Kuwait. Much of the internal policy of the oil rich shaykhdom of Kuwait, says Professor Abu-Hakima, has been dictated by the state of affairs in neighboring countries. The two-thirds of the population who are non-Kuwaitis, often Palestinians, create internal pressure. An hereditary amirate, Kuwait legitimizes its existence for its radical Arab socialist neighbors by Arab League membership and aid through the Kuwait Fund for Arab Economic Development (Hopwood, p. 39). In the Trucial Shaykhdoms, oil-rich Abu Dhabi and Dubai confront the regional powers of Iran, Iraq and Saudi Arabia in the wake of the 1971 British departure. The Union of Arab Amirates marks the Trucial Shaykhdoms' political effort to resolve historical differences and provide a defensive alliance.

George Rentz provides excellent background on Saudi Arabia, a development-oriented state operating in a largely traditional, partyless, religio-political framework. Saudi Arabia, as Rentz indicates, owes its unity to fundamentalist Wahhabis who to this day have provided a religiously based nationalism (Hopwood, p. 66). The author does not address himself to some important questions, however. King Faisal has ably continued the cautious modernization begun by his father, it is true, but can an Arab monarch, particularly in an age of radical Arab nationalism, be the vehicle of modernization? Can expenditure of the enormous oil royalties for a modern infrastructure within a staunchly Muslim framework with only token regard for Arab radical goals obtain national commitment?

Writing on Oman in the southeastern part of the strategically significant Arabian Peninsula, Wilkenson, Bathurst, and Kelly are keenly aware of social change and international intrusions antedating oil discoveries by centuries. The context, they generally conclude, may remain largely traditional, but contending elites in recent times have sought outside help in their struggles from such diverse sources as Arab socialists, the United Nations, London and Peking.

Political Developments and International Relations: A. B. Kelidar identifies three pressure zones challenging political stability. First is the familiar U.S./U.S.S.R. rivalry which as Monroe stresses may be compounded by Chinese influence. Second is the Arab-Israeli conflict whose resolution would entail unacceptable concessions by both sides. Third is the less well understood intra-Arab conflict whose Palestinian component Kelidar ignores.

Though Iran is a non-Arab regional power, says R. M. Burrell, it cannot escape great power rivalry or regional conflict. Iran has in the past occasionally exhibited expansionist tendencies. Its sizable but untried military machine may as the struggle for oil hegemony intensifies in the future engage in combat with Iraq, Kuwait or Saudi Arabia, this reviewer speculates.

Frank Stoakes applies structural-functional analysis to the oil-producing Gulf principalities. His most pessimistic position is that existing governmental structures inadequately cope with the stress induced by rapid change: "Social security passes with the old structures . . . it may take time for patron-client groups, political parties, social clubs and perhaps religious brotherhoods to fill the gap . . . " (Hopwood, p. 191). An optimistic position, to which he rightfully leans, is that ample oil revenues may make possible public acceptance of the political community and the regime itself. Material conciliation, he goes on to say, abounds for the general public and, of course, the well-cared-for security forces. Also, many traditional rulers are learning to play the populist game at home and to meet sensitive ideological foreign policy requirements.

Sociology and Culture: P. A. Lienhardt describes the inferior status of traditional women in Oman, while Thomas W. Shea usefully documents the rise of a Saudi, Aramco-trained middle class. He should be read along with William Rugh's "Emergence of a New Middle Class in Saudi Arabia" (*Middle East Journal*, Winter, 1973). Ali Muhammad Luqman, studying Southern Yemen, finds little commitment even in recent times to education and a vigorous press yet is optimistic about their role in liberating women and introducing democratic government.

Economics: Edith Penrose maintains that oil revenues have given area governments a dominant role leaving little room for private entrepreneurship. Since the oil industry is not labor-intensive, and since foreign corporations (instead of the host governments) collect most of the revenues, it is up to the government to develop distributive skills. Professor Penrose asserts that tough negotiations, resulting in what is perhaps the world's fastest growing store of capital, emphasize the need for extensive reorganization in state structures. (An estimate that is making the rounds now is that if the Arabs spend at least one-half of their oil revenues they will still have over $100 billion in gold and foreign exchange by 1985. The fact that the present *world* figure is $150 billion gives the picture. See, among others, *The Wall Street Journal*, November 1, 1973, p. 16.)

Yusif A. Sayigh highlights the disparities in oil and non-oil resources, human skills, population and land area. He proposes a broad federation of

all ten political units on the Peninsula and the "rational" distribution of the oil wealth.

Industrial world policy makers preoccupied with maintaining the oil flow will benefit from these books. Many of the conclusions, of the Monroe book in particular, are shared by James E. Akins in "The Oil Crisis: This Time the Wolf is Here" (*Foreign Affairs*, April, 1973). Political scientists interested in comparative political systems about which there is little information will benefit from the Hopwood work. For instance, the contributors agree that religious authority has its uses in this supposedly secular age, especially since Islam and the state do not lend themselves to European style dichotomy.

There is a broader reason for our interest: expectation of dramatic change in Africa south of the Sahara has not been fully justified because of the comparatively modest capital inflow. In the core area of the Middle East, however, systemic transformation fueled by oil revenues may take place before our eyes on a scale heretofore unrealized in the so-called Third World. After reading these two worthwhile books with their picture of increasing abundance impacting on the oil producing nations this reviewer is prompted to ask: What will the effect of scarcity be on the democratic industrial users—at least until the arrival of workable fusion reactors in the next century? The question is: Can we in the West make systemic adjustments to a shrinking pie?

JOHN G. MERRIAM
Bowling Green State University

Political Elite in Bihar. By Shashishekhar Jha. (Bombay: Vora & Co., Publishers Pvt. Ltd., 1972. Pp. 332. Rs. 25.)

Why is it that most of the empirical studies of Indian politics by Indians have proved disappointing, especially if they have been written and published in India? While a case can be made for the paucity of research support and the absence or publishing pressure in most Indian institutions. I think the reason lies deeper than this. With some few notable exceptions, Indian empirical political research has a *pro forma* quality about it. It is as if a methodologist, trained in the United States, told all his colleagues that they must have samples and interview schedules or questionnaires, and they must have frequency tables if they want to be respected in their field.

Methodologically, Professor Jha's empirical study of Bihari state legislators has a mechanical quality. The "sample" was intended to cover all legislators, but as should be expected, the author was unable to interview about a third of them, and the sample became an arbitrary one. Professor Jha writes about a "questionnaire" but does not include it, and one is tempted to conclude from other things he said that he simply had wide-ranging unstructured interviews. His treatment of these data are perfunctory and his use of descriptive statistics and cross tabulations is repetitive, confusing, and achieves negligible results. It may be that Indian society is too complex for such manipulations. However, computers are virtually inaccessible to most Indian scholars. Therefore, this may be an effective demonstration of why the simple transfer of American survey research methods to the Indian academic context often proves inappropriate.

On the other hand, in the historical portion of his book, Professor Jha has amassed a wealth of material about group, community, and factional politics in the state. His description of factional dynamics, though confusing, is perhaps the most insightful portion of the book. He demonstrates the incredible complexity of factional structures in India in which caste cleavage is no more important than personal ties, intracaste conflicts and intercaste alliances. He also indicates why factional groups are so transitory. "Two leaders of the same caste in the same rank inside one factional group (is) . . . impossible" (p. 93).

The book recounts the historical background of factional politics in Bihar and describes the role that caste groups, Muslims and tribal peoples have played in the state's shifting factional alignments. The political elite which Professor Jha describes statistically is limited to members of the state legislature, however, and excludes other political elites such as the higher civil service and Congress party organization leaders. Data are presented on the social origins and demographic characteristics of the legislators and some insight into their political opinions is also offered. To link his findings about legislators to political achievements in the state, Professor Jha attempts an analysis of the politics of agrarian reform in Bihar. The result is a case history only remotely related to the activity of Bihari state legislators. That might be Professor Jha's most telling point, although it appears to be unintended.

While Professor Jha's Indian English syntax will raise the hackles of most American readers, it is his handling of empirical data and his general lack of organization that makes this book difficult to recommend. A good source of material on the poor but important state of Bihar is long overdue, since it has a greater population than most countries (more than 47 million people). Unfortunately, this book only partially fills that gap. Furthermore, although recently published, the research for it was completed in 1964, before all the great upheavals attendant upon Nehru's death.

DOUGLAS C. SMYTH
Florida Technological University, Orlando

The Caciques: Oligarchical Politics and the System of Caciquismo in the Luso-Hispanic World. Edited by Robert Kern. (Albuquerque: The University of New Mexico Press, 1973. Pp. 202. $10.00.)

This is a stimulating series of generally first-rate essays devoted to exploring the meaning of a concept used by many political observers of the Hispanic world. *Cacique* is an indigenous word originally meaning "chief." If there is a consistent contemporary meaning, it is "local political boss," with a shading that emphasizes his monopoly of control and willingness to use violence.

The variety of leaders to which it has been applied is clear from the essays in the book. Karl Schwerin and Charles Gibson discuss the pre-Conquest and Colonial Indian leaders who evolved from autonomous chiefs to brokers totally dependent on Spanish authority. Robert Kern contributes a study of the stressful politics of late 19th-century Spain in which prominent figures condemned the cynical manipulations by leaders through local bosses called *caciques*. Eul-Soo Pang describes the role of local bosses in backlands Brazil called "colonels" (*coronels*), plausibly included as a Brazilian version of *caciques*. Howard Karno narrates the history of a freebooting frontier entrepreneur and political boss in Amazonian Peru who became involved in international as well as national political deals. Antonio Ugalde applies the term *cacique* to the strong personalist leaders who emerged in a Mexican village after the Revolution and whose followings were gradually institutionalized into party factions. Wayne Cornelius describes some powerful individuals who control patronage and distribution of resources in some squatter settlements in contemporary Mexico City. Enhancing the book's exploration of meanings is the long, three-part essay by Marshall Nason, analyzing the manner in which the term appears in the literature of Spain and Latin America.

The reader will come away with a lively sense of the evolution of the concept and its various manifestations, and, I think, a sense of an underlying, constant phenomenon. He will not, however, get much help in defining that phenomenon nor in learning how to use it theoretically. In his brief and inadequate introduction (there is no conclusion), the editor urges the highly questionable proposition that *caciquismo* is distinctive to the "Luso-Hispanic world" and defines it in such a way that it refers to a large-scale system of politics (such as he is describing in Spain) rather than the local phenomenon which most authors in the volume describe. Nason does try systematically to analyze the use of the term in literature, but no one tries systematically to order its analytical content. Ugalde and Cornelius offer overlapping definitions, but there is no exploitation of the diverse essays in a way that would facilitate comparison.

There are parallels in the essays however, which might be the basis for comparative analysis. The most common explanations for the occurrence of *caciquismo* in the essays is the lack of a government presence in the boss's domain. The intervening factors emphasized vary from personal insecurity in the absence of legal guarantees, or lack of countervailing controls on the (naturally?) rapacious local boss. The weakness of the government may stem from the newness of the political unit in question (as in squatter settlements or frontier areas), the distance from national authorities, or the lack of determination on the part of authorities in policing local leaders.

A second sort of explanation often complements the first. The people or the local leaders or both are somehow felt to be especially prone to *caciquismo* either because of their Hispanic culture or backwardness. Earlier writers were much given to formulations about the ignorance of the people and the barbarousness of local leaders—especially those not from the traditional elites. More recent writings, including some in this book, are more apt to emphasize lack of socialization in handling modern political processes. This line of argument tends to assume that the central government as the repository of either aristocratic virtue or modern values is the logical agency to discipline the barbarous or backward *cacique*. Kern's notion that *caciquismo* had its heyday in the Liberal period of the end of the 19th century fits in with this view, making it appear that *caciques* were particularly prominent in the period of transition when mass politics was emerging and elite, aristocratic controls decaying, but modern political institutions not yet elaborated. But how then to account for the continued occurrences of *caciquismo*? They might be thought of as taking place in exceptional pockets where governmental services and laws are not effective. William Brisk, in a final article, tries to make a broader case for a "new *caciquismo*" by reference to the work on Latin American politics by Charles W. Anderson and others, suggesting that many Latin American nations do not have a strong, legitimate authority. Brisk suggests that various interest groups and institutions come to act as *caciques* in these circumstances, but this is stretching the concept out of recognition.

A search among the essays for help in assessing the significant consequences or role of this type of political structure is less rewarding. By and large, despite the diversity of situations and manifestations, most of the authors appear to find *caciquismo* simply terrible. Although Ugalde and Cornelius have more detachment, most of the authors seem to reflect the polemical and pejora-

tive use of the term which was very strong around the turn of the century. With *caciques* come corruption, instability, brutality, and exploitation. Perhaps the term is too loaded to have a serious theoretical function. That it is considered "typically Latin" and retains such a loading is doubly unfortunate for a serious treatment of Hispanic politics. But that is not an uncommon situation. Perhaps *The Caciques* will stimulate others to articulate a more precise and flexible view of this type of politics. There is much in these essays to show the need and give an initial push.

DOUGLAS A. CHALMERS
Columbia University

Politics in India. By Rajni Kothari. (Boston: Little, Brown and Company, 1970. Pp. 461. $4.50, paper.)

If there is any one Indian political scientist who has been closely associated with American social scientists in the 1960s it is Rajni Kothari, the author of the book reviewed here and the director of the Center for the Study of Developing Societies in Delhi. Kothari is the co-author (with this reviewer) of a book on Indian voting behavior; he collaborated with Sidney Verba in an international comparative study of political participation; he was associated with Philip Jacob's project on the cross-cultural study of values; he collaborated with Samuel Eldersfeld and the Michigan Survey Research Center on an international comparative study of voting behavior; he spent a year at Stanford in the Institute of Political Studies and at the Center for Advanced Study in the Behavioral Sciences; and finally, the book reviewed here is part of an important series in comparative politics edited by Gabriel Almond, James Coleman, and Lucian Pye.

This book reflects these multiple influences. Its central concepts are those currently employed in the study of political development and comparative politics: a focus on modernization, the growth of political and governmental institutions, the role of the party system, the social infrastructures of political life, the impact of political culture and political socialization, and the problems of state and nation building in a multiethnic environment. These concepts are all brought to bear upon the central proposition of this book—that there exists a distinctive Indian model of politics. This model is characterized by Kothari as noncoercive, open and competitive, with high and expanding levels of political participation and a well-developed administration into which the political structures and values of the country have penetrated. Within this broad framework Kothari examines how the political system works, then considers some of its limitations and "costs." He differentiates the Indian case from that of 19th-century Europe where "during the phase of rapid industrialization and social change, political participation was confined to the upper classes of society" (p. 9).

Kothari evidently takes pride in the accomplishments of the Indian political system even while he laments its problems and failures. Although he emphasizes the distinctive qualities of the system and its successes—at times defensively—he maintains a critical stance as a social scientist examining the workings of his own political system. Indeed, in some respects the book is organized around the contrasts between its successes and failures.

Kothari demonstrates that India has been successful in coping with a variety of political crises since 1947. The system has absorbed new elites from lower and middle castes; it has incorporated elements that had hitherto not taken part in village, state or national affairs; its national government has coped with linguistic demands for states' reorganization and a national dispute over language policy that threatened the country's unity; it dissipated secessionist tendencies in South India and in the Northeast; it has sustained a national party and with it a degree of central authority lacking in many developing countries; it successfully coped with two "succession crises" with the deaths of Nehru and Shastri; and Mrs. Gandhi, in a political tour de force, rescued the Congress party from internal factionalism and a loss of voter support that threatened not only the party but also the maintenance of stable central authority.

If the measure of system performance is the management of political conflict, then surely the Indian system ranks high, especially when one takes into account the enormous potential for both conflict and demand making in such a large, complex, participant system.

Why, then, is an elite that is so skillful in political accommodation and so responsive to conflicting interests and demands so unsuccessful in the management of the economy? Kothari, in his review of the government's economic policies, quite properly points to its accomplishments, even as he is highly critical of its failures. But a theme throughout is India's lost opportunities. Most of the agreed-upon economic problems, he writes—the low utilization of industrial capacity, the problem of unemployment, the high rate of population growth, the inadequacy of the educational system, and the misuse of the country's educated youth as a resource—are all inadequately handled by the Indian government.

Curiously, Kothari is unable to find an adequate explanation for the paradox of political accomplishments and policy failures. He suggests that "the requisite energy and drive that are needed are lacking" (p. 381), and after reviewing what needs to be done to stir the inert administrative structure he laments that "there seems to be

a frightening lack of imagination" (*ibid.*). Earlier, he suggests that there are tensions in the system between "the objectives of growth and justice, of production and accumulation on the one hand and distribution and equity on the other" (p. 340). Neither of these two explanations is satisfactory, for neither rapid economic growth nor greater distribution and equity have been achieved; and while it is plausible that the simultaneous pursuit of both objectives prevents either from being maximized, it is also evident that neither objective has been pursued very vigorously. As for the lack of "energy and drive," the leadership has demonstrated that when it wishes to act "decisively" as it has on numerous occasions in both international and domestic political affairs, it can do so.

Perhaps Kothari could have found a more satisfactory answer if he had asked why the government has pursued economic policies that are so evidently wrong. Surely, it is not because the country lacks able economists or the present leadership has not received good advice. An examination of the noneconomic objectives of economic policies, the ideologies and beliefs that underlie these objectives, and the interests which they serve might provide more satisfactory answers. The argument that the government is committed to greater equity will simply not wash, not when government is so thoroughly engaged in policies that expand the size, salaries and functions of the bureaucracy to the detriment of both equity and growth.

What I find disappointing in this otherwise excellent work is that it does not do what an Indian social scientist is in the best position to do. It tells us a great deal about how politics works but very little about how government works; it tells us a great deal about how conflict is managed but little about how policies are formulated; it tells us much about what programs and policies have been adopted but little about why they do not succeed. Even if American political scientists were free to conduct research in India, as they are not, these are not questions to which foreigners could easily address themselves. If there is to emerge a creative and independent Indian political science it is with such questions, however delicate, that Indian scholars should be concerned.

MYRON WEINER
Massachusetts Institute of Technology

Social Groups in Polish Society. Edited by David Lane and George Kolankiewicz. (New York: Columbia University Press, 1973. Pp. xvi, 396. $11.00.)

This is the best scholarly book on contemporary Poland that has been published in recent years. It is composed of a number of sociological portraits of what the editors consider to be social groups in Polish society today. In each of these essays the historical development of the group is discussed first. The attention is focused on the group's involvement in various intra- and intergroup conflicts in the general political structure of the society.

The book is a pioneering effort in two ways: first because it attempts to present a new view on Poland, a country split by internal divisions and conflicts in spite of the rigid control of the authoritarian Communist regime; and second, because it is based on an extensive review of the vast Polish literature on the subject. Most of the data and some analytical perspectives are derived from it by the authors of the chapters in the book. In the final essay, David Lane comments on what the book has covered stating that the modern Polish society is not only divided into a number of diverse large groups but each of these developed a consciousness of its own group identity and separate interests. By making claims on the political systems these social groups play a political role in the system. He admits that the writers of the essays while showing that groups articulate demands, have failed to demonstrate how demands are aggregated in the political order of the regime (pp. 317–318).

David Lane develops the theoretical and linking framework for the other studies in two essays: the introduction ("Structural and Social Change in Poland") and the conclusion ("The Role of Social Groups"). These essays, though interesting, are not as innovative or revealing as the others. As in the literature published in Poland, Lane is evasive about whether or not the groups he outlines are in fact units of social stratification. The attempt to tear away the "blinders" of the "totalitarian" model is so strong in parts of these two essays that one gets the impression that the author is discussing a kind of Western democracy rather than a structure within an authoritarian regime which suppresses all forms of institutionalization of group interests not approved by the Communist Party.

The shortcomings of Lane's general theory have no bearing on the other chapters in the book. Paul Lewis's illuminating essay on the Polish peasantry is an important new link in the chain of works on that topic, studied in the past by such writers as Thomas, Znaniecki, Chalasinski and in the postwar literature by B. Galeski. Lewis analyzes changes in the Polish village. He is especially interested in contemporary conflicts between peasants and the Party. He shows how the State, unable to force the peasants to join the collective farms and unwilling to allow them to develop private enterprises, is attempting to "freeze" the situation for the time being. Polish agriculture, claims

Lewis, is still based on a peasant economy, which is neither capitalist nor socialist.

In "The Polish Industrial Manual Working Class" George Kolankiewicz considers two topics: the relations in industry between workers' councils and other organizations such as unions, the party, the administration and management of industries; and the living standard of workers as compared to that of nonmanual employees. He tells us that though the Party claims to represent the working class, this class has practically the lowest status and standard of living in the society.

Today as in the past, writers in Poland are seen as spiritual leaders and the embodiment of the national conscience. Nowadays, however, the Party claims to perform these roles. In George Gömöri's "The Cultural Intelligentsia: The Writers," we learn how writers today are still struggling to implement their inherited functions. In another essay, George Kolankiewicz undertakes a study of the technical intelligentsia. Again, the study focuses on changing conflict in society. In the past, the technical intelligentsia was polarized into technical experts and political activists. Today the conflict between a bureaucratic administration and innovative engineers and managers is becoming more important. Kolankiewicz also considers differences in the status of, and rewards for, nonproductive and productive activities.

In "The Local Political Elites," Ray Taras discusses the institutional structure of local authority and what may be regarded as rudimental institions of participatory democracy in the Polish Communist state. He describes the institutional vehicles of democracy at the local level which operate in Polish society. Even at this level, however, the Party imposes stringent controls, either directly or through various administrative branches of the local government. In addition, the same people who hold important positions in the Party or governmental institutions are often elected or nominated to positions in local councils. Still, the writer tells us, some semblance of democratic activities exists on the local level of societal organization.

The book does not give a general overview of Polish society and its current problems. Some very important and influential social groups—such as the military, the police, faculty and students, artists, the Catholic clergy, and the political apparatus of the Party—are not considered. This is understandable, however, since these groups are not often covered in Polish sociological literature either. The book also does not analyze such an important factor as the "Sovietization" of society, which is undoubtedly affecting the structure of social groups in Poland. In any case, it is a very important book in the field of East European studies, both as an introduction to the accomplishments of Polish sociologists, and as a fresh, innovative approach to Polish Social groups. It presents a good understanding of social and political mechanisms and of cleavages in Polish society rarely considered in Western literature.

SZYMON CHODAK
Sir George Williams University

The Middle East: A Political and Economic Survey, 4th Edition. Edited by Peter Mansfield. (London: Oxford University Press, 1973. Pp. 566. $19.95.)

Studded with ethnic, religious and linguistic minorities and seared by a large number of interstate and intrastate conflicts, the Middle East is perhaps the most difficult region on earth for political scientists to analyze. Sunni and Shii Moslems, Catholics and Protestants, Jews, Kurds, Copts, Maronites and Armenians coexist uneasily in a region characterized by unstable and frequently changing governments and periodically convulsed by fighting between Arabs and Israelis, Iraqis and Kurds, Lebanese Christians and Moslems, Sudanese Arabs and Blacks, Jordanians and Palestinians, and North and South Yemenis. In a well-organized and well-written volume of 566 pages, replete with charts and statistics, Peter Mansfield seeks to make the Middle East's complexities comprehensible, and while one may not always agree with the analyses presented by Mansfield, the primary author of the book, or by the other contributors, the book is nonetheless of high quality and clearly superior to the other surveys of the Middle East which have appeared since 1970.

Mansfield's book is divided into two sections: a series of thematic studies dealing with regional issues and problems, and a group of individual country studies covering all the Arab states (except the North African ones), Iran, Turkey and Israel. The thematic studies, of uneven quality and different authorship, deal with: Soviet and American policy toward the Middle East (Geoffrey Wheeler; Robert Hunter): the development of the oil industry (Edith Primrose); Arab political movements (Peter Mansfield); the origin of the Palestine problem (Elizabeth Monroe); and cultural trends in the Arab world (Jabra I. Jabra; Louis Awad). Except for the two very interesting studies of Arab culture, the authors of the thematic studies present a clear British perspective on political developments in the Middle East, one which tends to be both sympathetic to England's colonial policies in the region and highly critical of U.S. policy toward the Middle East, particularly its support of Israel. Indeed, criticism of American policy runs like a

thread through the entire book, and while some of the criticism is valid, a number of the British writers' assertions are difficult to accept. Thus, in describing Syrian attitudes in 1957, Mansfield asserts (p. 470) "Above all, the American attitude toward Israel, which remained *unchanged* (emphasis mine) determined the attitude of the average Syrian toward the West, just as it was the main cause of any pro-Soviet feelings he might have." In making such an assertion, Mansfield seems to overlook the fact that American pressure in 1956–57 had forced Israel to withdraw from territories it captured in the Sinai campaign of 1956 and that the Syrians, still unhappy at the Turkish annexation of Alexandretta less than twenty years before, were very concerned about a possible Turkish invasion. Equally difficult to accept is Robert Hunter's criticism (p. 97) of the active American support for King Hussein's regime in Jordan in September 1970 when it was faced both by an uprising by the Palestinian commando organizations and an invasion of armored units from Syria. Indeed, the firm display of U.S. support for Hussein, when coupled with the Soviet Union's lack of support for its client, Syria, may be considered a turning point for American fortunes in the region, which had been declining since the June 1967 war. Following Hussein's victory, the pro-Soviet Syrian regime of Sallah Jedid was overthrown and replaced by the more independent regime of Hafiz Al-Assad, while Egypt, led by its new president, Anwar Sadat, (Nasser died during the Jordanian civil war) began to move away from the USSR and toward the United States.

Of the thematic studies, by far the best is the one by Elizabeth Monroe on the origins of the Palestine problem. Monroe does a real service to American political scientists and historians studying this knotty problem by making available the results of recent unpublished Oxford Ph.D. dissertations as well as materials from the private papers of individuals involved in the English policy-making process on Palestine. Interestingly enough, however, while downplaying the impact of "Zionist pressure" on English policy, Monroe sees that factor as the major influence on American policy toward Palestine, although she does not go so far as Mansfield (p. 349) who calls U.S. support for the Zionist cause in the 1945–48 period "ill-informed and sometimes irresponsible."

The country studies, all written by Mansfield, comprise eighty per cent of the book and are very good summaries of the main political and economic developments in each country with emphasis primarily on the post-World War II period. While little in any of the studies will be unfamiliar to Middle Eastern specialists, Mansfield makes available a number of very useful statistical tables on each of the major countries, including budget allocations, main trading partners, and major imports and exports by commodity group. The best two country studies are the ones on Lebanon and the Sudan, although the latter is somewhat marred by Mansfield's apparent bias against the Catholic church, which he blames for many of the problems in the southern part of the Sudan (p. 449).

One rather disturbing aspect of the book is Mansfield's lack of sensitivity to the plight of minorities in the Middle East. He treats the Turkish massacre of the Bulgarians in 1876 almost in passing, and gives little emphasis other than in the cursory introductory section to the massacre of Armenians by the Turks in 1915, the slaughter of Assyrians by the Iraqis in 1933, and the continuing persecution of the vestigial Jewish communities in a number of Arab countries. In addition, although the book tends to be pro-Arab in its treatment of the Arab-Israeli dispute, the Palestinian Arabs, themselves a minority in all the Arab countries except Jordan, do not receive a particularly fair or full treatment. In Elizabeth Monroe's study, no attention is given to the split in the Palestinian Arab community between the extremist Husseini faction and the more moderate Nashashibi faction over the White Paper of 1939; nor is mention made of the Husseini faction's active cooperation with the Axis powers in World War II—cooperation that cost the Palestinian Arabs as a whole dearly in the international deliberations on Palestine after the war. Mansfield, in his chapter on Jordan, fails to mention the role of the Palestinian Arabs in the battles for Jerusalem in the Arab-Israeli war of 1948, and gives all the credit for the capture of the Old City to the Jordanian Army.

Nonetheless, despite these areas of weakness, this is a very useful and worthwhile (albeit expensive) book, and it will serve both as a high quality introduction to the Middle East for those unfamiliar with the region and as a very handy reference book for Middle Eastern specialists.

ROBERT OWEN FREEDMAN
Marquette University

The French Colonial Myth and Constitution-Making in the Fourth Republic. By D. Bruce Marshall. (New Haven: Yale University Press, 1973. Pp. 363. $12.50.)

Bruce Marshall has written a useful book. Of all the things which differentiate France from her European neighbors, none has been so strikingly visible as the impact of decolonization upon the domestic political system. Marshall's particular contribution to our understanding of this phenomenon is a history of French ideas about the proper relationship between the metropole and her colonies.

By "colonial myth" Marshall means "the

vision held by virtually all of the French elite of an indissoluble link between France and the colonies" (p. 2). Certainly he is right in seeing as the distinctive quality of French colonialism

> the seriousness with which French leaders embraced the ideal of an ever closer union between France and the colonial territories . . . (and their) conception of a worldwide community of peoples bound together into a single nation by common ties of economic and political interest, embodied in republican governmental institutions, and sharing a common cultural base (pp. 12–13).

Surely Marshall is also right in attributing some importance to this myth in explaining why the French had such a hard time coping with the development of divergent consciousnesses inside the Empire and the desire of peoples everywhere to shape their own destinies. France is after all the only European country whose regime collapsed because of this problem.

The book traces the origins of the myth back to pre-Revolutionary France, where we find both considerations of state and the civilizing mission. The Revolution added the concept of citizens endowed with political rights; the socialists and communists extended it to economic and social citizenship as well. Marshall's scholarship in documenting the interplay of these doctrines is impressive (and the bibliography is very helpful). After two chapters defining different conceptions of the myth, Marshall follows their development through key moments in the formation of French colonial policy: the conflict over the Blum-Violette bill in the thirties; the struggle between de Gaulle and Vichy for control of the colonies; the Brazzaville Conference; and, the focal point of the book, the writing of the Constitution after World War II. Chapter Eight is especially good, covering the years 1944–47, a fluid period when the possibilities for change were greater than usual. Marshall succeeds in juggling the extraordinary and multiple complexities of the French party system, the different Constitutional drafts, and the variety of local colonial situations.

What Marshall does less well is evaluate the importance of the colonial myth relative to other factors in explaining significant outcomes in French colonial history. Economics, domestic cleavages, the party system, international rivalries, *raison d'état*, greed, lobbies—these are not by any means absent from the book, but is it not clear what relationship the colonial myth bears to them. Is it a cause of outcomes? Is it a vehicle for description of outcomes? Is it a symptom of a rationalization, an obfuscation? Marshall does an excellent job of describing differences among liberal, radical, socialist, communist, conservative, fascist, and Gaullist attitudes toward the colonies. Precisely because there were different versions of the colonial myth, how are we to understand which one shaped policy making at a given moment? How does the colonial myth help explain the differing treatments by the same country of Indochina, Tunisia and Algeria, or Guinea and Chad? Why does the colonial myth decline (Chapter 9)?

The causes of colonialism and the causes of its decline, at least in its classic form, are very old issues in social science, with a lengthy history of debate behind them. I miss in this book a more explicit attempt to locate the argument in relation to other schools of interpretation. And I miss a comparative dimension, which would have aided in the ranking of causes. Is the difference between the French and, say, British conceptions of the link between metropole and colonies the most powerful factor in explaining the differences between French and British decolonization? Such is the implication of Marshall's book, though I am not sure he would accept it so baldly put. More self-consciousness about cause and effect would have helped us understand the lessons to be drawn from the vast amount of work and thought that went into this book.

PETER ALEXIS GOUREVITCH
Harvard University

The Emergence of the Indian National Congress. By S. R. Mehrotra. (New York: Barnes & Noble, Inc., 1972. Pp. vi, 461. $22.50.)

Political development still remains fashionable as a concept or—to be more accurate—as a topic. Scholars such as Rupert Emerson and Samuel Huntington now have popularized the phenomenon of political decay in regard to the newly developing nations. The political track record indeed is dismal. Why, in this retrogression of development to decay, is India such a notable exception? Both internally and externally she continues to be plagued by serious problems, both natural and man made.

Dr. S. R. Mehrotra, a specialist in South Asian politics at London University, indirectly provides at least a partial answer. In the laboriously constructed *The Emergence of the Indian National Congress* he provides a detailed account of the coming together in different organizations of Indians in the eighty-year period preceding the establishment of the organization. Thus, in the mid-nineteenth century the reader almost physically can see emergent elites learn the art of political association. Finally, the apex of this early period of development, and the concluding section of the book, is the institutionalization of a national organization which in the following century becomes the major vehicle of the national movement for independence. And the Congress Party continues to provide leadership and cohesion to India today.

It is easy for a political scientist to criticize this volume because the 400-odd-page book ends just as the Indian National Congress is founded. In fact, it originally was intended as a preface to a work on the pre-independence Congress Party. Moreover, a mass of detail and numerous lengthy quotations reinforce the essentially descriptive approach. Thus, it easily can be marked with the pejorative labels of traditional, descriptive, nontheoretical, and historicist. On the other hand, its redeeming features are notable. Essentially, the book is valuable for two reasons centering around the subject matter and its utilization of newspapers.

A superficial reading initially suggests that this is yet another book describing the antecedents of the Congress Party. Mehrotra sets forth the old familiar names of people, provinces, and organizations. But, as the reader grinds away, the picture emerges of the birth of the Indian National Congress not as a skyrocket in the political sky but as an inevitable consequence of steady, unspectacular growth. The political process gradually subsumes compromise, negotiation, accommodation, and a wide range of leadership. Thus I see the subject matter as focused on the foundations not only of the Indian National Congress, but of what becomes the "Congress system" or political process which has so distinguished India from most other newly developing countries.

An important element in the socialization process included British nonofficial residents who were nicknamed the "interlopers" by the British bureaucracy. British lawyers, journalists, teachers, missionaries, planters and traders acted as they did at home. "Their precept and example taught their Indian fellow-subjects the art of constitutional agitation" (p. 4). Indian pupils were sufficiently advanced so that the lessons could be applied in the controversy over the widow-burning issue in 1829–30. The rapid spread of English, particularly among the elite in Bengal, enabled the British interlopers to reach increasingly large audiences. George Thompson came to India in 1843 to argue passionately for the development of voluntary organizations and constitutional agitation. A contemporary newspaper, commenting on the crowded public meetings he addressed, noted that in the recent past only a handful of Indians would have been able to understand the eloquent Britisher. Now, in 1843, "we find hundreds of educated natives listening with admiration . . . " (p. 26). The author agrees that Thompson is appropriately described as the "father of political education in India."

Most famous of all the interlopers in the nineteenth century is A. O. Hume. Students of the period are familiar with his career in the Indian Civil Service, his retirement and subsequent efforts leading to the convening of the first meeting of the Indian National Congress. Mehrotra's contribution does not end for all time the argument over whether or not Hume properly is glorified as the "father of the Indian National Congress." Other individuals, virtually all Indian, already were working in this direction and in time would have served the midwife's function which Hume in fact performed. What is notable in Hume's endless traveling and letter writing is the series of India-wide communication links (see p. 359) which were so well formed by the time the first Congress met in 1885. A rich organizational and human growth preceded the inauguration of the Congress.

Indigenous organizational growth and the development of a civic culture are more the theme of this volume than is the role of "interlopers." Mehrotra provides detailed descriptions of a train of organizations from the Bengal Landholders' Society in 1838 to the culmination in 1885 of the political coalition which originally was to be called "the Indian National Union" (p. 400). Failure rather than success often characterizes the political efforts of the period. Organizations rise and quickly wither and die. But amidst an ever widening elite and increasing politicization, substantial organizations grow in the major cities of India and even begin to reach out into some hinterland towns.

Unfortunately, a rudimentary model developed in the middle part of the book is not followed consistently. Mehrotra states that two different models of national organization were current in the 1870s. One was a notion of centralization with control over affiliated associations. A decentralized alternative proved to be more practical for organizations like the Indian Association which initially desired centralization (p. 295). One hopes that the author will probe the significance of the adoption of this model by the early Indian National Congress in his subsequent volumes.

The voluminous documentation and quotations from contemporary newspapers constitutes the book's greatest asset but also presents a serious problem. A participant's context is accurately created, and an over-reliance on British official records is avoided. But unnecessary repetition results in tedium.

The book's rich descriptions of organizational growth and faithful reproduction of the period recommends it to South Asia specialists. It also can serve other third-world specialists for needed comparative studies on the early development of associational life. Unfortunately, it is priced at an inflationary figure.

PAUL WALLACE
University of Missouri, Columbia

Working Papers on Canadian Politics, Enlarged Edition. By John Meisel. (Montreal: McGill-Queen's University Press, 1973. Pp. 257. $6.00, paper.)

This is not the typical academic book in which an author systematically examines a subject either continuously or in essays. Rather, it is a collection of five apparently unconnected and lengthy articles on recent Canadian electoral and ethnic politics.

John Meisel, an accomplished and well-known student of Canadian elections, claims that he was prompted to present these pieces in this fashion for several reasons: Three of the articles make available some material derived from a national survey of 2767 eligible voters he carried out after the 1968 federal election before his more elaborate projected study on that election is published. They also contain data that might not otherwise find their way into print. Another paper on French and English Canada is a more complete version of an article in the collection *One Country or Two?* (Montreal, 1972). Finally, this edition has been enlarged over a previous one which appeared early in 1972 and contains a new article on the 1972 election.

Meisel does himself an injustice in not attempting to link the articles with an introduction outlining a common theme, for the five essays all relate to each other rather directly in dealing with factors that make Canada the cleavage-ridden society it is today.

Students of voting behavior will probably find most interesting the three papers on the 1968 election—especially the first one. It covers the bases of party support in 1968 in global terms dealing with such topics as the usual social variables, general issues and voters' perceptions of the parties. A considerable amount of data and descriptive commentary is presented, some of the findings showing changes from past behavior, others revealing patterns of Canadian political affiliation that have continued over time. Unfortunately, Meisel does not differentiate between the new and the old either by footnotes or direct textual reference. So unless one were fully knowledgeable about past work in Canadian voting, one might assume, for example, that before 1968 no one had observed the impact of leadership on partisan choice, the marked strength of the Liberals in the major metropolitan areas, the significant working-class support for the New Democratic Party, or the pronounced dislike of the prospect of minority government by large numbers of voters.

At the same time, the author presents some new material and much that is insightful. One especially interesting point is that the heterogeneity of Conservative compared to Liberal support places considerable strain on the Conservative party, particularly on its leaders. Another is that while the Liberals are often seen as the party of French Canada, in 1968 they were far more the party of the English and people of minority ethnic origin in Quebec.

Yet another fascinating observation is that since underprivileged groups tend to vote for the out-parties, the NDP and the Creditistes, the political system cannot be as responsive to them as it might because Liberal and Conservative party and governmental elites are thereby automatically less exposed to pressures from the less-privileged than they might be were there more such groups among the ranks of their parties' voters.

The paper on party images analyzes the parties in terms of semantic differentials and compares public responses to items on each of the four parties as well as an "ideal" party. It also reviews the differences between these results and those obtained from similar techniques in a national survey conducted after the 1965 election. After systematically dredging the data covering eight socioeconomic groups, the author concludes by suggesting that aside from showing that many groups appear to have low expectations of the political system, more work needs to be done with such an instrument if it is to have much explanatory value.

In the last paper of the three directly emerging from his 1968 survey, Meisel relates values, language, and political alignments. He resists describing Canada simplistically as if it were divided into only two linguistic groups. He feels there are five, which he labels "pure English," "partial English," "mixed," "partial French" and "pure French." He then proceeds to analyze these groups in terms of what he calls private values: religiosity, moral values, (for example, reactions to homosexuality and divorce), law and order, and ethnic and religious toleration. He then considers public reactions to some leading political figures, foreign policy, the monarchy and constitutional change, and the place of Quebec in the federation.

The data illustrate the formidable barriers to a countrywide consensus: first of all, they suggest that the ethnic-religious cleavage in Canada continues to dominate no matter what the momentary issues; and, second, that the issues themselves are cut so deeply across ethnic-religious and regional lies that political elites have little leeway in using them to mobilize national majorities.

The essay on French and English Canada and the options confronting the country as it tries to manage the conflict between these two groups is very much a normative piece. Meisel comes out strongly for compromise, but this prescription

does not keep him from a penetrating elaboration of the positions of both French and English.

I consider the final article entitled "Howe, Hubris and '72: An Essay on Political Elitism" the best of the lot, despite its impressionistic quality. It was written during the week following the 1972 federal election, and in 35 pages Meisel manages to capture the essence not only of the just-completed campaign and four years of the Trudeau administration but also of the Liberal party's governmental style since World War II. He concludes that what was largely responsible for the Liberals' close call in 1972 (they obtained 109 seats to the Conservatives' 107, while the NDP won 31 and the Creditistes 15) was not the English backlash against their policies on bilingualism, as Trudeau and many others have claimed, but rather their failure even to appear to be interested in dealing with issues that polls showed Canadians to be concerned about: unemployment, inflation, high taxes, and welfare. Such lack of concern he terms *hubris*—an overweening pride and insolence leading inexorably to downfall—which afflicted the Liberal government in its final years under St. Laurent in the middle 1950s and has continued to characterize the Liberals under Lester Pearson and then Pierre Trudeau. He illustrates his charges of arrogance against the Liberal party by referring to the way it runs the Federal government, the manner in which it recruits to high office, the social background of Trudeau himself, and the general Canadian political culture which reinforces the now ingrained pattern. The article is a *tour de force.*

If there is a general criticism to be offered—and perhaps this is unfair since Meisel claims that much of what appears here is preliminary—it is that the reader is in danger of being overwhelmed with detail. The articles are each extremely valuable but they are marred by the author's reluctance to engage in either generalization or theory testing. Nevertheless, until the projected more comprehensive volume appears, this collection can still serve as a useful up-dating of the literature on partisanship in Canada. And, at the very least, Meisel's insights will undoubtedly spur further research by some of his readers.

PETER REGENSTREIF
University of Rochester

The Soviet Armed Forces, Books in English, 1950–1967. By Michael Parrish. Hoover Institution Bibliographical Series: XLVIII. (Stanford, Calif.: Hoover Institution Press, 1970. Pp. viii, 128. $7.50.)

The development of the Soviet Union into a nuclear "superpower" has been accompanied by unusually frank discussions among Soviet military spokesmen on the implications of this "revolution" for Soviet military doctrine, strategy, and force composition, as well as on the general questions of security and war in the nuclear age. Mr. Parrish's bibliography provides an invaluable reference source and guide for the student and professional wishing to reconstruct this important period of U.S.-Soviet relations or to study the evolution of Soviet views on war and strategy. The 2146 entries, organized under twenty subject headings, cover almost all of the significant publications on military subjects which appeared in the Soviet Union between 1950–1967 and the interpretations put on them by Western scholars and analysts. Because of their limited circulation at the time of their publication, some of these translations and studies, heretofore not generally known, can now be located with the help of this bibliography in any of the U.S. government depositional libraries.

LEON GOURE
Center for Advanced International Studies, University of Miami

Elections in South Vietnam. By Howard Penniman. (Washington, D.C.: American Enterprise Institute for Public Policy Research and Hoover Institution on War, Revolution and Peace, 1972. Pp. 246. $3.50.)

This is a book to inform, not inflame. As suggested at the outset, "Almost everyone has an opinion about the electoral institutions of South Vietnam—and almost no one has bothered to study them" (p. 1). Professor Penniman's study is the most comprehensive account of the seven national elections held in South Vietnam from 1966–1971 yet to appear.

I have rarely seen an author be so fair or modest about an event directly observed. As a consultant to the team appointed by President Johnson to observe the 1967 elections in Vietnam, Professor Penniman in no way assumes the role of an instant expert on Vietnamese politics. The descriptions are straightforward and the period under study thoroughly researched. The myriad of rules and practices affecting the electoral process are systematically presented, campaign periods fully described, and criticisms and evaluations succinctly discussed. The volume also contains the best account of Vietnamese journalism I have ever read. The appendices contain a brief description of the elections held in North Vietnam since 1945, a fascinating account—the fullest I have yet to see in print—of a campaign appearance by Thieu and Ky and the ten other slates in the 1967 presidential election, and three highly detailed tables of election returns. Overall, the volume should make an important addition to the bookshelves of those interested in comparing electoral

processes. It invites and facilitates further analysis.

Substantively, the volume deals with how the nature of the electorate, the level of competition fostered in the elections, and the institutions closely related to the electoral process affected the quality of the elections. In a certain sense, the analysis can be seen in the context of testing Richard Scammon's observation which Penniman quotes, namely, that the elections were "reasonably efficient, reasonably honest, and reasonably free" (pp. 75–76). Professor Penniman concludes that they were. The definition of the electorate maximized universal suffrage, the registration process was inclusive and fair, ballot secrecy was protected, and there were adequate safeguards against voting fraud. Was the count honest? This, of course, has been a persistent criticism of Vietnamese elections and Professor Penniman persuasively argues that it was. Moreover, he says, even if returns were inflated to the full level charged by all groups, the outcome of the particular elections criticized would not have been different. In addition, the elections were covered by an open and critical press and were characterized by competitiveness for all positions.

The institutions that emerged from the elections studied here reflected the complexity of the society at large as well as the weakness of its political party system. The former emerged in spite of the regime; the latter, because of it. In sum, this study illustrates that elections can be democratic (i.e., competitive, open and fair) where political systems are not.

ALLAN E. GOODMAN

Clark University

Technology for Developing Nations: New Directions for U.S. Technical Assistance. By Rutherford M. Poats. (Washington, D.C.: The Brookings Institution, 1972. Pp. ix, 255. $7.50.)

Rutherford M. Poats, an experienced official of the United States foreign assistance program, has written a thoughtful and careful book on the export of technology to and the harnessing of technological skills and strengths in developing countries. He chooses his examples from six key areas in which technology is essential to development: agricultural research, industrial growth, health, population, nutrition, and education.

He has selected from these experiences evidence of opportunities for success. He makes a strong case for applied research and for concentrating U.S. technical assistance on activities "where the requests and needs of developing countries match actually available and superior U.S. capacity to help" (p. 14). At a time when the prospects for major U.S. public investment in foreign technical assistance are dwarfed by the needs for such development, Poats's book offers modest and practical counsel. Throughout the book are useful recommendations, wise advice, and a sensitive understanding of the paternalism which has so often wrecked foreign aid. He encourages programs by such organizations as the National Academy of Sciences, universities, and professional societies on that modest and carefully tailored scale which U.S. political and fiscal realities dictate. One wise counsel he offers, which the United States could well have followed earlier is: "Not until it is clear that the climate and institutional network for technological innovation in the requesting country are being systematically improved should U.S. assistance for public research centers be considered" (p. 80).

Mr. Poats's sense of priority for the most promising targets for U.S. technical assistance deserves attention. He sees, for example, in higher education the need to shift from generalized support to "particular institutional changes aimed at bringing professional and subprofessional training into closer and more efficient association with research and operations in each field" (p. 233). To promote industrial growth he urges technical assistance designed to improve "indigenous capabilities, both in private enterprise and in public institutions, to acquire, adapt, and use industrial and natural resource technologies" (pp. 84–85). Unfortunately, that formulation, while certainly correct, may understate the difficulties of doing it, which Mr. Poats, of course, understands.

Throughout the book are references to underlying political questions, both as to the capacity of the United States to organize and sustain an intelligent and sensitive commitment of resources to technical assistance on a permanent basis, and the political capacity of various developing countries effectively to use and channel modern technology to the benefit of their citizens. The modest-scale effort which Mr. Poats proposes requires greater sensitivity, greater understanding of political forces, far more competent institutions and mechanisms of collaboration and implementation than have existed to date and, above all, wise leadership by advisor and advisee alike. He sees the need for "a coherent national development strategy that brings to bear fiscal, financial, and political tools as well as technical instruments" (p. 8). He readily acknowledges that AID has "tended to downgrade technical and social issues and the staffs representing these interests" (p. 9). He acknowledges in the context of health programs that "AID did put its primary trust in the ultimate health giving powers of general economic development" (p. 109) and notes that "the politics of many developing countries tend to alienate both local and American experts" (p. 238).

But the book doesn't really address these fundamental political questions. AID has been run according to the notion that economic development is central and comes first, and that political and social development are secondary, separate, and come later. This is simply not true. Politics comes first and probably second, too. The setting of a political system—the capacity of a society to organize itself to do things, the spirit, the drive, the purpose, the leadership—that is what makes things happen. One needs only to look at the successes of Puerto Rico and Israel, where politics made economics. But developing nation governments, where corruption is often a way of life and political and bureaucratic institutions are less developed than a primitive economy even, are not institutions in which fine-tuned technological and economic assistance of a sophisticated sort can be effective. Poats has written a useful book which all interested in these important questions should read. But the political dimensions he clearly recognizes still require systematic treatment.

BREWSTER C. DENNY
University of Washington

Tito, Mihailović and the Allies, 1941–1945. By Walter R. Roberts. (New Brunswick, N. J.: Rutgers University Press, 1973. Pp. xv, 406. $15.00.)

After so much bad political history over the past twenty-five years on a complex and controversial subject, this book is as welcome as the morning sunshine following a prolonged storm. It depicts a Yugoslavia overwhelmed by Nazi military might, partitioned, occupied, and soon the scene of a bitter civil war between two resistance movements, forcing the major allies to make difficult policy choices. The author, an associate director of the United States Information Service, does a superb job in summing up the major developments in Yugoslavia (1941–1945), and the way in which Great Britain, the United States, and the Soviet Union influenced those developments.

Ironically enough, as Roberts demonstrates, the Big Three acted in support of what they conceived to be their short-run interests (winning the war), and essentially against their long-term goals (an ideologically preferred political system). For Churchill and Roosevelt (largely at the former's urging), Tito's Communist-led Partisans were killing the most Germans and therefore should be supported. Stalin, on the other hand, believed that Yugoslavia's greatest contribution to the war effort was in avoiding civil war. Consequently, he urged Tito to assist General Mihailović and his Chetniks, who were the first to raise the flag of resistance. Only when it was too late, did Churchill begin to worry about the implications of his actions, as he saw Tito turning against Britain and the U.S. when he no longer needed them.

Perhaps the book's greatest service is in throwing light on the leaders of the two resistance movements. Neither Tito nor Mihailović desired an Axis victory. Both wanted the Allies to win and assumed they would. That being the case, Tito's main concern was to insure that he was in power when it was all over. Mihailović, on the other hand, wanted to prevent that from happening. He would have preferred to avoid the terrible Nazi massacre of hostages (usually 100 to 1) following the killing of German soldiers, and to follow Western Allied instructions to European resistance movements to lie low, to husband their resources, and to await the signal of Allied landings on the continent. But Tito's actions forced him to fight against an imposed political solution, preferring that Yugoslavia's future be determined by the politicians and the people after the war. It is not surprising that the one who fought for a particular political solution, especially when aided by the Allies, should win in such a contest.

Roberts explodes the myth that Mihailović was somehow a traitor because he fought against the Communist-led Partisans and because some of his men made accommodations with the enemy. Moreover, on the basis of German and Partisan documents, the author shows that Tito's men were also negotiating with the enemy, and at a higher level. Three of Tito's top associates (including Milovan Djilas), in talks with Nazi representatives, asserted in writing that the Partisans regarded the Chetniks as their main enemy and if left alone would fight only them. From the point of view of the Nazi leaders in Berlin, however, both Tito and Mihailović were enemies of the Third Reich, and the Nazis offered the same reward for their capture dead or alive.

In the end, Tito won the struggle because (1) he never lost sight of the long-term goal and placed great emphasis on organization to achieve it, (2) he operated in territories not actually occupied by the Germans (the opposite was true of Mihailović), (3) he successfully sold himself and his movement as doing the greatest damage to the Axis in Yugoslavia and thereby built up his military potential with Western aid, and (4) other events and problems progressively made the Balkans seem less important to the West, making it easier for the Soviet Union to assist Tito in consolidating his gains in the fall of 1944 and the spring of 1945.

Specialists may wish that the author had covered some topics more fully, and they may

find some issues to argue about, but they cannot deny the importance of this much-needed book.

ALEX N. DRAGNICH
Vanderbilt University

East and West Germany: A Comparative Economic Analysis. By Martin Schnitzer. (New York: Praeger Publishing, 1972. Pp. 446. $22.50.)

While recognizing that East and West Germany started with different capabilities and have "different sets of priorities," Martin Schnitzer, Professor of Finance at VPI, feels that it is possible to make "penetrating judgments" about "which one has performed better over time in terms of economic development and providing for the welfare of its citizens" (p. v). The author compares the modified market-directed West German economy with the plan-directed East Germany economy in terms of institutional structures, levels and rates of economic development, and patterns of income distribution. Emphasis is placed on the 1960s, with separate chapters surveying the political system, the organization of industry and agriculture, the fiscal system, the monetary system, income distribution, and the role of labor unions in each country. Only the introductory chapter and two concluding chapters are truly comparative.

Among the most useful materials covered in these chapters are a succinct summary of the impact of taxation and social welfare programs on income distribution in West Germany, a description of the East German wage system, and a discussion of the role of the Bundesbank in the efforts to combat inflation in West Germany in the late 1960s and early 1970s—this last a concise, readable account of the difficulties encountered in attempting to use monetary policy to control inflation in the existing international economic climate.

For political scientists the greatest value of Schnitzer's work is that it brings together a vast amount of comparable economic data for two important political systems. The author gives us a mixed picture of economic development and social welfare, with both countries enjoying substantial success, while the gap between them "has not narrowed to any significant degree" (p. 386).

Two major weaknesses limit the utility of this book for the political scientist. First, it contains almost no discussion of the politics of economic policies. For example, there is little mention of the multiple issues and personalities involved in the 1963 New Economic System (NES) reforms in East Germany, despite the fact that Schnitzer deliberately stresses the postreform period in his analysis of performance. Similarly, there is no discussion of the issues, personalities, and decisions involved in the late collectivization of agriculture in the GDR, which took place throughout the 1950s and increased in intensity after 1959. As far as the West is concerned, the fundamental differences in the fiscal and monetary policies of Ludwig Erhard, architect of the "Economic Miracle," and Karl Schiller, Willy Brandt's Minister of Economics, are reduced to three sentences (see p. 163). Thus, one looks in vain for anything resembling the in-depth policy analysis found in George Feiwel's *The Soviet Quest for Economic Efficiency: Issues, Controversies and Reforms* (New York: Praeger Special Studies, 1972), or Abraham Katz's *The Politics of Economic Reform in the Soviet Union* (New York: Praeger Special Studies, 1972), other recent publications in this series.

Second, the author has limited the value of his study by defining system performance in narrowly economic terms. For example, development is measured in terms of output levels, growth rates and efficiency in resource utilization, while public welfare is measured by real per-capita consumption and the distribution of income. While these are legitimate evaluative criteria, values other than growth influence resource allocation—particularly in East Germany, where the maintenance of party supremacy and a socialist system of production and distribution are important values. It is widely recognized, for example, that progress in economic liberalization and rationalization in East Germany, not to mention the Soviet Union, were affected by the events in Czechoslovakia in 1968. Indeed, one wonders how valid is the author's contention that one can make "penetrating judgments" about system performance when priorities differ. By choosing to adopt external performance criteria, rather than to deal directly with the multiple objectives of elites, Schnitzer has sacrificed some measure of scope and political relevance for manageability. In spite of these limitations, the effective integration of economic data in a comparative framework suggests that the volume will serve as a basic reference source for scholars interested in the two German states. It may also be of value to scholars interested in the methodology of comparative analysis.

GERALD G. WATSON
University of Northern Colorado

Latin American Modernization Problems. Edited by Robert E. Scott. (Urbana: University of Illinois Press, 1973. Pp. vi, 365. $12.50.)

Eight contributors representing six disciplines writing about four countries—never more than two essays on the same country or problem—may make a collection; they do not make a book. While the unifying theme purports to be "modernization," this concept remains so amorphous

that thematic unity is more apparent than real. As editor and senior contributor, Robert Scott does have some interesting things to say about the region, but these observations (discussed below) neither underlie his colleagues' research nor grow out of their findings. Indeed, as will be noted, in some instances the case studies contradict Scott's generalizations.

Given such diversity, it is impossible to do justice to the contributors—all of whom are associated with the University of Illinois' Center for Latin American Studies. Suffice it to say that the book contains something of interest for those who pursue modern Brazil, Colombia, Mexico and Peru; race, political culture, external financing, agrarian reform, military government, urban migration and marginal communities. My comments will be limited to Scott's contributions, totaling nearly a third of the volume, which are presumably of greatest interest to readers of this journal.

Those familiar with Scott's writing will not encounter much that is new, except perhaps greater pessimism regarding the prospects for political integration within these nations. Scott continues to assume that Latin America is in a "transition period," "mov[ing] . . . down the road to modernity;" that modern technology operates inexorably to erase regional and national differences; and that the "convergent" institutional responses to this technological imperative constitute "development," "successful modernization," "westernization"—without its mattering much in Scott's eyes which term is employed (pp. 2–5, 21). This view is qualified, however, by such statements as "there is no unilinear line of development," "such observations are at too high a level of generality to provide theoretical proof," the substitution of "may be" for "is," etc. (pp. 5, 21, passim). While some readers may appreciate Scott's caution, I find this "no risk" style of discourse—a style endemic to academic social science—troubling in its elusiveness. It permits the author to convey a point of view without taking a clear and arguable position.

"Most persons in the emerging areas," comments Scott, "view the development process in a rather simplistic manner, based on a misunderstanding of the western models" (p. 10). To the contrary, I find Latin American writers and leaders often more sophisticated than their U.S. counterparts in realizing that "development" need not mean "westernization" or even "modernization." Peru, Cuba, and Brazil are more technologically modern than they were fifteen years ago. They are also more *varied* institutionally (less like each other in important ways), in part because each has opted for a different mode of incorporating that technology. Even in these studies edited by Scott there is evidence of culturally specific, pre-industrial traits surviving the supposedly leaching effects of modern technology and its "required" social relations. Cartegena's race relations continue to resemble nothing seen in most archetypal "modern" nations; Zé Maria's "traditional" values find reinforcement in a modern, urban factory; strong central governments under military auspices suggest a resurgence of traditionally Hispanic forms of authority.

In some matters Latin America will become like us whether or not it chooses to; in others it has a choice; in still others it probably can't resemble us, want to or not. Delineating the scope and content of these categories would seem close to Scott's central concerns, yet it is a question never confronted openly and systematically. When discoursing on "modernization," Scott usually emphasizes the first category: inescapable, standardizing influences emanating from the centers of world production and communication. While describing recent political trends in the region, Scott injects a large dose of category three, leaving the reader with the impression that these Latins will never overcome their parochialism and adopt the modern, the universalistic, the functional way of doing things. Both perceptions may be true, but if so, we need to be shown *how* they intermesh and *which* things fall into which category. Finally, I don't believe Scott takes category two, the area of conscious choice, very seriously. Brief references to a "socialistic approach" as being "economically oriented" "much in the style of the Soviet Union" (p. 11), suggest that Scott has closed his mind to the ways in which Cuban leaders, for one, have modified some of the "immutable" laws of modernization (reversing urbanization, for instance, or narrowing inequalities during the phase of capital accumulation).

In the final chapter Scott provides an overview of recent political trends, revealing his deep familiarity with a region he has studied for a quarter century. As in previous writing, he sees integration as the central problem of countries beset by "contending traditional and emerging interests," each possessed of "particularistic" values and prone to use extralegal methods (pp. 312–3, 355). Popular parties have failed to provide integration; now the military are trying their hand. Scott doubts they will succeed, concluding that integration will have to emerge slowly and painfully from "the disorder of a more open system" (pp. 355–6).

This is a somewhat Perucentric view, that being a country Scott knows particularly well. Brazil hardly conforms to his generalization that military governments "recognize a need to mobilize the popular majorities" and "seek to expand the proportion of lower-class citizens sharing

in the benefits of the national economy" (pp. 350, 335). Likewise the "declining influence" of popular parties, linked to their inability to "cut across regional, social and class lines" (pp. 322, 325), scarcely fits Argentina, where the Peronists have triumphed over the military's efforts to disperse or co-opt them. But any attempt to discern trends in a region as diverse as Latin America must be procrustean, and Scott's is actually less so than most.

This book, then, contains detailed case studies alongside broad generalizations. Both are interesting in their own right, although one suspects they appeal to different audiences. Unfortunately, the promise held out by juxtaposing the two in an expensive book is not fulfilled.

ELDON KENWORTHY
Cornell University

Revolutionary Education in China: Documents and Commentary. By Peter J. Seybolt. (White Plains, N.Y.: International Arts and Sciences Press, Inc., 1973. Pp. 408. $15.00.)

Peter J. Seybolt, assistant professor of history at the University of Vermont and editor of *Chinese Education: A Journal of Translations*, has collected thirty-two transtions of articles written primarily for Chinese consumption and focusing upon recent developments in China's quest for educational innovation.

While the title of the book possibly suggests a broad historical scope, only six of the translations pertain to the pre-Cultural Revolution period. The remaining 26 translations, comprising the focus of this compilation, were written between 1967 and 1972, and pertain to the general educational goals adopted or renewed during the Cultural Revolution, problems of administration, the roles to be played by the teachers, special educational problems created by immediate economic needs or the past behavior of certain of the students, and the directions to be followed in elementary, secondary, and higher education.

Seybolt, in his 51-page introductory commentary, seeks to provide background information about the political, economic, and philosophical contexts from which the documents emerged. For example, he discusses the Marxist goal of ending all forms of exploitation, and the presumed role education can play in coping with the problems produced by China's immense population, the backwardness of its agrarian society, and the lack of suitable communication structures. The author provides a walking acquaintanceship with the red vs. expert, the Mao vs. Liu, or the mass-oriented vs. elite-oriented forms of analysis. The commentary also discusses in terms of the Mao vs. elitest faction struggle, the events of the Cultural Revolution, especially as they affected the administration and direction of the educational system. Seybolt also sketches the changes wrought by Mao in the Chinese educational infrastructure and in the philosophy of education to be followed.

The appendix contains paragraph-length biographies of fifty-odd personalities alluded to in the documents and a useful glossary of basic Chinese Communist jargon.

Professional China-watchers will find this a useful compendium of educational documents, and others in comparative education who are Chinese specialists will find both the introductory commentary and the documents helpful in understanding "Revolutionary Education in China."

ANTHONY W. FERGUSON
Brigham Young University

Vietnam and the West. By Ralph Smith. (Ithaca, N.Y.: Cornell University Press, 1971. Pp. ix, 206. $6.50, cloth; $1.95, paper.)

At this writing, the problems of the United States in Vietnam are far from over, as evidenced by Henry Kissinger's recent meeting in Paris with Le Duc Tho over the breakdown of the Paris Peace Accords and a recent appeal of the American Ambassador to Saigon, Graham A. Martin, for an increased aid package of about one-billion dollars which will include modern sophisticated weapons. After fighting the longest war in U.S. history, at a cost of fifty-thousand lives and billions in national treasure, we find that the question of "what went wrong" in Vietnam continues to elude official Washington and to confound the American public in general. Fortunately, this slender volume provides a partial answer to why the United States has met with so much frustration and failure in Vietnam.

Professor Smith's thesis is that the West in its interaction with Vietnam never fully understood or appreciated the complexity of Vietnamese society and culture. This lack of understanding led the French, and later the Americans, into disastrous policy decisions in Vietnam. Had Western policy makers had a better grasp of society and culture in Indochina, he feels, it is at least possible that the tragic events of post-1945 Vietnamese history might have been avoided.

Smith outlines how the impingement of the French upset the balance between contending forces in Vietnamese society, leading to a breakdown in the Confucian order and a gradual attempt to restructure Vietnam into a modern nation. The dynamics of this process are presented within the context of the major forces shaping Vietnamese society and culture during the last century. The author emphasizes the need to understand the Confucian moral and political structure in Vietnam; the central place of the clan, village, and ancestors in Vietnamese life;

the role of religions, sects, and secret societies in Vietnamese politics; and the struggle for modernization between the Communists and the Western-educated elite in Vietnam. Attention is also given to the contrasts in the philosophical and religious traditions of Vietnam and the West.

Demonstrating an inadequate appreciation for Vietnamese culture, the French established a pattern of colonial governance that alienated large segments of the population. The policies of "assimilation" and "association" had the effect of driving the Vietnamese into secret societies and religious sects, which were traditional organizations of resistance to ineffective central government, while at the same time these policies contributed to the inflexibility of the French response to nationalist demands. In failing to support gradual constitutional reform, the French inadvertently added further to the growth of these subversive organizations in becoming focal points of anti-Western and nationalist sentiment. Unfortunately, the secret societies and religious sects, as well as other political organizations in Vietnam, had a history of division, lacking more than regional unity, and they were unable to coalesce into a nationalist organization strong enough to unite the entire country. This fragmentation of the nationalist movement has, in the author's mind, been one of the most significant factors in the tragedy of modern Vietnam.

That the Americans were probably more ignorant about Vietnamese society and culture than the French is underscored by their steadfast support of Nho Dinh Diem, an ascetic Catholic and a fanatical anti-Communist. Diem's ineffectiveness as a reformer and his ruthlessness toward political opposition clearly contributed to the Communist cause in the South. Rather than hailing Diem's victory over the *Cao-Dai* and the *Hoa-Hao* religious sects, the United States should have been concerned about the Premier's inability to strike a political compromise with his nationalist and anti-Communist opponents. These mistakes in political judgment during and after the Diem regime were the result, according to Smith, of American failure to judge correctly the significance of current facts within an unfamiliar cultural context.

The author concludes with an admonition that the United States must commit a greater share of its resources to the understanding of Asian societies and cultures. Without such an effort, American policy makers will continue to find themselves ill-equipped to deal with problems in Asia, as events in Vietnam have so unequivocably demonstrated. With Washington considering additional technical and military aid to Saigon at a time when government support of academic programs has been curtailed, *Vietnam and the West* deserves careful attention by all who are concerned with American foreign policy in Southeast Asia.

ROBERT L. YOUNGBLOOD
Arizona State University

The Churches and Politics in Germany. By Frederic Spotts. (Middletown, Conn.: Wesleyan University Press, 1973. Pp. 419. $15.00.)

This is the first full-scale study of the political role of the Christian churches in post-World War II Germany (West). The author, a foreign-service officer, has had access to many hitherto unavailable sources—unpublished church papers and governmental reports as well as interviews with key figures in the events and situations described. The result is a book rich in new information that will prove rewarding to anyone concerned with German politics since 1945.

Two background chapters trace the character and organization of German Protestantism and Catholicism. The author shows how the course of the Protestant Reformation led to some of the central features of modern German history: deep religious divisions, the confessional-geographic pattern of society, and the intermingling of religion and politics. The political consequences of the Reformation reinforced the inherent conservatism and patriarchalism of Lutheranism. On the Catholic side, as a result of the small-German solution to the problem of unification, which left German Catholics north of the Alps a permanent minority, there emerged a ghetto mentality and an often obsessive nationalism.

The collapse of the Nazi regime and the aftermath of a disastrous war put organized Christianity in a position of unique authority. Though both churches had largely failed to provide an effective counterforce to Nazism, at war's end they were among the few German institutions that had not been "coordinated" into the Nazi state. Amidst the boundless moral and material rubble of Germany in 1945 they stood as elements of stability and as an important source of values. The major part of Mr. Spotts's book describes how this great authority and influence of the churches in 1945 was gradually undermined and weakened by new forces and attitudes in Germany —the ascendancy of liberal politics and economics, secularism, and the pragmatic politics of a progressively industrialized, technocratic and prosperous consumer society.

As the author shows, the process of change was inaugurated by the mass influx of Germans from the Eastern territories occupied by Poland which put an end to the dominance of a single church in the local community as well as to the de facto "apartheid" of Catholics and Protestants. The way was now open to full social reconciliation

between the confessions, and the laity became freer to ignore or reject ecclesiastical guidance. This development was especially significant in Catholic areas; most of the Catholic hierarchy's activities in the political and social sphere during the past twenty-five years can be seen as a vain attempt to maintain its authority over the individual and the community. The outcome of the bitter dispute over the continuation of confessional schools more than anything else demonstrated that the church could no longer control her members; following this defeat the bishops have been unwilling to risk another showdown over any important issue of social policy.

The disparity between the weakening of the churches' political influence and the continuing substantial legal privileges of the churches is striking. To this day they enjoy a favored position in West Germany that is probably unique in the world. The state provides for the education of the clergy in university theology faculties, the state makes annual cash grants, and state and/or local authorities collect a church tax which at times reaches 26 per cent. In addition to what is explicitly provided by law, the churches benefit from certain unwritten rules of German politics which provide for confessional parity in appointments to the civil service and the various bodies dealing with education and social welfare. "Confessionalism," writes Spotts, "is one of the skeletons in the closet of German politics" (p. 205).

In a thoughtful, if brief, conclusion Spotts evaluates the political record of the Christian churches since the end of World War II. The Protestants, he finds, through their synodal meetings, academies, church assemblies, and countless informal meetings at all levels helped achieve and maintain lay interest in public affairs, and these activities became virtual training institutes for self-government. The Evangelical church thus was a major contributor to the success of liberal democracy in Germany. The Catholic church, on the other hand, tried to mobilize the laity into a political army and generally has sought to transform practical politics into a struggle over ultimate, ideological values, an approach incompatible with the democratic requirements of tolerance and compromise. The church aligned herself unqualifiedly with one political party, the CDU, but in the end the tactics employed undermined the strategy. By 1963 the CDU had come largely under the dominance of Protestants; three years later the CDU entered into a coalition with the Social Democrats whom the church had wanted to keep out of power forever. The CDU today, despite the confessional adjective in its title, is the prototype of an Anglo-American mass party. The Catholic bishops, for their part, have largely lost the political loyalty of their flocks. They still refuse to acknowledge that the interests of a secular society may legitimately differ from the morality of the church, but their threats of electoral vengeance can no longer be carried out.

There is much else in this valuable study that cannot be discussed in limited space: The churches' role under the occupation and their attitude toward denazification, the fate of the Reichskonkordat of 1933, the churches' handling of the problems of reunification, rearmament, conscription and nuclear weapons. The author writes well and lucidly. In all, this is an eminently readable book, which for a long time to come will remain the indispensable guide to the role of religion in contemporary German politics.

GUENTER LEWY
University of Massachusetts, Amherst

Structure and Process in Latin America: Patronage, Clientage, and Power Systems. Edited by Arnold Strickon and Sidney M. Greenfield. (Albuquerque: The University of New Mexico Press, 1972. Pp. 256. $10.00.)

The study of patron-client relations is maturing and developing increasingly useful theoretical models for the analysis of societies. Professors Strickon and Greenfield considerably enlarge the pool of descriptive studies available in this area with their nine-chapter book, the outgrowth of two conferences on the subject held in 1969. The editors open with a brief review of the background to the study of patron-client relationships which is more historiographic than a definition of what patron-client relations are. We should, therefore, be aware that the conceptual framework of patron-client relations addresses itself to exchange relations (1) between two people (dyadic) or networks of dyads, (2) between unequals, (3) between individuals who gain from the relationship (reciprocity), (4) between people over a relatively long period of time, covering a relatively large number of situations (i.e., multiplex, not simplex relations), and (5) between people who are able to interact frequently and personally (i.e., proximity). The introduction explains what variables the authors of the remaining chapters are to examine: "(1) The Ibero-American cultural tradition . . . , (2) the system of formal positions within the institutional system of the nationally organized, complex cultures of Latin America; and (3) the resources available to the specific actors that may be employed by them in the transactional process" (p. 15). The editors conclude that these three variables are to form the basis for analyzing *situations* in which actors relate to each other in pursuit of goals. The book is and was planned to be largely descriptive.

Despite this aim the first substantive chapter, by Professor William T. Stuart, is a discussion of

the application of cultural ecology and general systems theory to the study of patron-client systems. After defining and explaining the utility of systems theory in the analysis of clientelism, Professor Stuart concludes that "weakly centralized" societies are most likely to stimulate patron-client relations. He adds " . . . I wish to suggest that most societies or cultures have been and are able to work out more concrete, substantive, and permanent adaptations to environmental challenges than the short-term and precarious resolutions we are calling "patronage" (p. 32). His conclusions are not entirely unprovocative, since Professor Stuart is arguing that these systems are transitional, only to be liquidated by the " 'rational' bureaucratization of the structure . . . " of the state (p. 39). Except in the most idealized and perhaps unattainable sense, one suspects that no society in our empirical universe would qualify; the author mentions that "wardhealers" (p. 40), and ghetto bosses still remain even in the United States. One might, however, raise the possibility that modern, urban men in highly bureaucratized societies could continue to find the personal character of clientelism attractive in expediting their affairs, and thus the setting, the exchange "currency," the actors, and indeed the objectives of clientalist ties could change but the preconditions and the style of these relationships remain. Professor Stuart's vantage point is amplified in his other contribution to the book "On the Nonoccurrence of Patronage in San Miguel de Sena." Here he analyzes a Colombian *municipio*, concludes that patronage-clientage is relatively unimportant in this district, and argues for a limited definition of the concept and its use only in special instances.

Nancie L. Gonzales, in her article "Patron-Client Relationships at the International Level," while focusing on the interactions between an institution in the Dominican Republic known as the Development Association (client) and foreign lending agencies notably AID (patron), also essentially uses changes in the system's ecology as the crucial condition determining the future of clientelism. Professor Gonzales, speaking about patron-client relationships, says, "The system, highly specialized and highly adapted to the expanding industrial complex of the nineteenth and early twentieth centuries, may prove to retard future development in countries such as the Dominican Republic" (p. 206). This article goes beyond the conventional use of the framework of clientelism by studying cross-national ties. In doing so, patron-client analysis emerges as a potentially crucial concomitant to "dependency theory" which has become so valuable a perspective in international politics studies.

Turning to more familiar ground we find several chapters which deal with microlevel cases of clientage such as Sidney Greenfield's "Charwomen, Cesspools, and Road Building: An Examination of Patronage, Clientage and Political Power in Southeastern Minas Gerais." This piece is an intriguing and often amusing description of the lace-like complexities of pay-offs, the competition and often bitter political struggles for the preferments generated by clientelism.

Arnold Strickon's article "Carlos Felipe: Kinsman, Patron, and Friend," discusses the intricate behavior of a patron as he attempts to manipulate those people upon whom he is dependent and who at the same time are subordinate to him. One of the most significant observations Strickon makes in his study of this Argentine case is that one of the consequences of patron-client relationships (providing links across class lines and minimizing lower-class solidarity) has nothing to do with the motivations of the actors themselves. Thus, in the case he describes, Carlos Felipe became a patron largely to ensure himself a dependable labor force.

Dwight B. Heath's chapter, "New Patrons for Old: Changing Patron-Client Relationships in the Bolivian Yungas," discusses clientelist networks in various specific fields of action and stresses changes in these networks over time. Heath points out that the impact of the Bolivian revolution on forms of social structure is significant for understanding patron-client relations. He argues that traditional clientelism and " . . . a new patron-client type of functional relationship . . . " (p. 133) centering primarily around the *sindicato* (syndical organizations organized by the MNR Party) coexist and that often clients consciously choose to retain the old patron-client relationship. His conclusion is that clientelism most often is role-specific and that one must be careful to look at "crucial individuals" as well as "various statuses" (p. 133) in analyzing patronage/brokerage. Professor Heath cautions the reader by saying, " . . . many relationships that appear, in structural terms, to be impersonal and universalistic are, in functional terms, personalistic relationships of patronage and clientage," (p. 134) thus, apparently siding with those scholars who are arguing for using the patron-client model generously in analysis rather than restricting it to classic situations.

The application of patron-client analysis in an unconventional way is found in Chapter 7 "Ponchos, Weaving, and Patron-Client Relations in Northwest Argentina," by Esther Hermitte. Ms. Hermitte describes the clientelist links in the town of Huarco involving women weavers. Her findings suggest most basically that women placed in the strategic position of having a valuable resource (weaving) can become desirable clients. They also indicate that a client may have more than one patron to fulfill her existing and potential future expectations, a finding which consid-

erably amplifies the more usual indication that a *patron* has more than one client.

In a different vein and addressed to a more general level of the system we find Robert W. Shirley's article "Patronage and Cooperation: An Analysis from São Paulo State" which is an interesting discussion of the conflict between traditional clientelist politics in Brazil and the obstacles it presents to the development and efficient functioning of that country's cooperative movement part of which has been seized by power holders and part of which may apparently be operating under a distinct disadvantage vis-à-vis the loosely structured, clientelist networks of private commercial firms.

I have briefly reviewed the content and conclusions of all the articles because in doing so one can appreciate the range of cases and the varieties of situations which have been studied. Thus it becomes clear that the book represents a major contribution to the growing collection of empirical material available on the subject. Its great shortcoming, however, is that neither the individual contributors nor the editors make an effort to construct comparative insights into the study of patron-client relations. This omission is perhaps in part a consequence of the exclusion from the volume of material by political scientists and other scholars representing diverse disciplines. While anthropology has pioneered in this type of analysis, the range of interest today includes all of the social sciences.

Another weakness of the book is the underrepresentation of urban life in the discussion, a fact which again reflects the tradition of studying patron-client ties in villages, small towns, and rural areas. This omission is especially crucial in light of the assumptions put forth by some that clientelism is a traditional form of interaction, subject to change or disappearance with perhaps the achievement of some optimal level of "development." It is also unclear what effect, if any, the different levels of development characterizing various Latin American countries (comparing Argentina with Bolivia, for instance) might have on the understanding of patron-client bonds. Still another crucial question requiring attention is, "If one finds political clientelism used in a certain social setting to expediate matters, are *all* people tied into networks? If not, what sorts of people and situations are non-clientelist; why and with what consequences?" Moreover, since one of the variables which the volume was to examine consisted of the "Ibero-American cultural traditions" as it bears on the exchange relations under study, one must ask what, if any, conclusions were reached regarding the role of this and how Ibero-American culture would differ in this respect from, let's say, Southeast Asia, which James Scott and Carl Landé have been so intensively studying.

Thus while the book is indeed valuable to Latin Americanists and to scholars working on clientelism, it fails to address itself except in a tangential or accidental way to some of the theoretical questions which political sicentists are eager to begin answering.

STEFFEN SCHMIDT
Iowa State University

Book Publishing in the USSR: Reports of the Delegations of U.S. Book Publishers Visiting the USSR, October 21–November 4, 1970, August 20–September 17, 1962, Second edition, enlarged. (Cambridge, Mass.: Harvard University Press, 1971. Pp. 182. $4.95, paper.)

These reports, prepared by a small group of American publishers following their visits to the Soviet Union in 1962 and 1970, make available to the English-speaking public a limited body of data on Soviet publishing; sixteen sections cover such subjects as the organization of publishing houses, production, marketing, royalties, and foreign translations. Unfortunately, however, the volume gives a rather formal, superficial picture based almost entirely on impressions and Soviet responses during official visits, augmented by some statistical tables compiled from Soviet publications. Most notably, as the delegation themselves were well aware, the crucial subjects of political-ideological control and of the mechanism of censorship were carefully avoided, and the authors were unable to delve into these matters. The section of each report devoted to copyright (pp. 35–38, 168–75) is of particular interest in indicating the evolution of Soviet official thinking and dialogue with Western publishers on the subject, and hence gives some of the background for the recent adherence of the Soviet Union to the international copyright convention; but again, the censorship and ideological dimensions of this issue are overlooked. For reference and readability, it would have been much more satisfactory had the new material from the 1970 visit been integrated with the earlier report. As it is, the 1962 report (previously published in 1963) is merely reprinted (pp. 61–176) unrevised with the separate 1970 report (pp. 1–59) serving to indicate changes and corrections.

PATRICIA K. GRIMSTED
Russian Institute, Columbia University

The Private Sector in Soviet Agriculture. By Karl-Eugen Wädekin. 2nd revised edition of *Privatproduzenten in der sowjetischen Landwirtschaft.* Edited by G. F. Karcz. Translated by K. Bush. (Berkeley: University of California Press, 1973. Pp. 432. $17.50.)

No one concerned with understanding the nature of the contemporary Soviet system can afford to miss this book. This is, first, because Professor

Wädekin's method is a model of genuine cross-disciplinary research. The ideological, legal, political, and economic foundations of private agricultural enterprise in the Soviet Union are treated with impressive thoroughness and analytical sureness, and each aspect is expertly woven with the others into an integrated portrait of this "dark corner" of the Soviet system. Second, the illumination of the private sector in Soviet agriculture affords new perspectives and insights that have implications that extend well beyond the agricultural domain. Third, Wädekin demonstrates, by example, the continuing necessity for, and the scholarly productivity of, painstaking, empirical research for expanding our understanding of Soviet affairs. In order to overcome the paucity of reliable data in readily accessible sources, Wädekin has examined virtually every conceivable source, from contemporary Soviet literature to official statistical handbooks, and he has put these disparate fragments together into an intellectually meaningful and highly readable form. The late George F. Karcz, whose encouragement and assistance are acknowledged as instrumental in the appearance of this edition, and Keith Bush, the very able translator, also merit thanks and congratulations for a job well done.

As Wädekin repeatedly and quite correctly stresses, the private agricultural sector is not merely an historical remnant appended to the public (socialized) sector. It is rather an integral and vital component of the agricultural sector. Although there are areas in which private and public agriculture compete for resources and markets, these two sectors are mutually dependent, and cooperation is therefore essential to the satisfactory performance of either. There is, in fact, a symbiotic relationship between private and public agricultural enterprise, and the exchange of both intermediate and final products is extensive. Specialization is also highly visible in deliveries to the state and to the kolkhoz market. Thus, a certain division of labor has evolved between private and public agricultural sectors that permeates the production, marketing, and consumption of agricultural products. The degree of interdependence is such—and this is one of Wädekin's main points—that any attempt to constrict the private sector necessarily entails the impairment of public argicultural (and distributional) performance as well.

The division of labor that currently exists comprises the following salient features. In the first place, a large proportion of private agricultural output is produced for own consumption, which both reduces the burden on the state distribution system (especially as many of these products are highly perishable and/or expensive to transport) and affords a higher standard of consumption than would otherwise be possible for the population at large. Second, the share of private output that is sold on the kolkhoz market by the producers themselves (as opposed to consumer cooperatives) serves the same ends. Third, private agriculture draws mainly upon manpower sources that it would be difficult if not impossible for state agricultural enterprise to tap, and it utilizes land resources that, to a considerable extent, are not amenable to large-scale mechanized farming. Fourth, private plot farms, and particularly rural household plots, exchange inputs with public farms to mutual benefit, e.g., animal husbandry products against fodder. Finally, household access to private plots and the consciousness of mutual dependence remain powerful incentives to participate in the public agricultural sector.

Although not stressed quite as much by Wädekin as the interdependence of public and private agriculture, the pervasive influence of the private sector in the Soviet economy is equally important and well-documented. According to Wädekin's careful estimates, some fifty million Soviet households—including a growing fraction of both rural and urban nonagricultural workers and employees—participate directly in private small-scale agricultural production. The greater part of the output, composed primarily of livestock products, potatoes, fine vegetables and fruit, is consumed on the private farm. But a nontrivial proportion is also marketed either in extra- or intrarural trade. Thus, in one way or another, virtually every household in the Soviet Union comes in daily contact with the private agricultural sector.

The existence and economic state of the private agricultural sector are of great consequence, therefore, in the planning and regulation of many aspects of the Soviet economy. It is true that the output of the private sector has been shrinking relatively during the last two decades, thanks to the growth of public farm output, but the absolute volume of private output has remained constant. Given the growing sophistication of consumer demand for agricultural products in the Soviet Union and the comparative advantage of the private sector in the production of the goods required to satisfy it, Wädekin is certainly correct in concluding that private agricultural enterprise is likely to remain an economic necessity for the foreseeable future.

Hence the ambivalence of Soviet leadership toward this sector and the kolkhoz market, which Wädekin documents thoroughly. Private enterprise is ideologically anathema, and the pervasiveness of its unwelcome influence has evidently been a source of concern to the leadership. Moreover, its performance stands as a nagging reminder of the incompleteness of socialization of the means of production and trade and of the lagging per-

formance of the socialized agricultural sector as well. Nonetheless, as the unhappy consequences of Khrushchev's attempt to restrict the private sector by administrative measures (reductions in plot size and in allowable livestock holdings) show, increasing the quantity and quality of agricultural products requires fostering *both* the private and the public sectors at once.

Wädekin devotes three full chapters, plus an addendum, to an examination of official policy toward the private sector in Soviet agriculture between 1953 and 1971. These chapters are among the best in the book, and they are interesting not only for what they reveal about the making and implementation of agricultural policy, but also for what they reveal about the character of Soviet politics generally during the post-Stalin period. Brezhnev and Kosygin, of course, immediately reversed the restrictions Khrushchev had imposed on the private sector during the last five or six years of his power. This was, naturally, a popular policy, but it was almost certainly introduced in the hope of reviving both public and private agricultural performance, which implies that the current leadership appreciates the fact of interdependence. Even so, the private agricultural sector and the kolkhoz market are certainly anomalies in the Soviet system and a constant source of irritation on ideological and practical grounds. Thus, whether or not Brezhnev will be moved eventually, as was Khrushchev, to seek prematurely and thus administratively to restrict the private sector remains an open question.

This is a very important book, and it will doubtless remain the standard reference work on the private sector in Soviet agriculture for many years to come. Moreover, it contains both explicit and implicit suggestions for additional research on the private sector of the Soviet economy and its points of tangency with the public sector, and I hope that others will follow Wädekin's lead in exploring these questions.

JAMES R. MILLAR
University of Illinois

The Fourth World: Victims of Group Oppression. Edited by Ben Whitaker. (London: Sidgwick and Jackson Limited, 1972; and New York: Schocken Books, 1973. Pp. 342. $10.00.)

This book does not lack worthy and ambitious intentions. A publication sponsored by the Minority Rights Groups (M.R.G.), an independent and international nongovernmental body headquartered in Britain, it aims to develop on "international conscience" with regard to minorities' treatment, to increase knowledge about the factors creating prejudice and intergroup hostility and, generally, "to help the position of persecuted or disadvantaged ethnic, religious or cultural minorities (or majorities)" (p. 9). But these lofty aims are to a large extent subverted by the way in which the book is organized.

Eight reports on minority rights commissioned by the M.R.G. constitute the core of the volume. These reports examine the position of the Asians in East and Central Africa, the Blacks in the Sudan, the Moslem Eritreans in Ethiopia, the African majority in Rhodesia, the Blacks in Brazil, the Catholic minority in Northern Ireland and the Protestant minority in the Republic of Ireland, the adherents of the Russian Orthodox, the Roman Catholic, the Protestant, the Jewish, the Moslem and the Buddhist religions in the Soviet Union, such nationalities as the Crimean Tatars and Volga Germans in the Soviet Union, and the outcastes of Japan. Most of the studies are competently presented. Special mention should perhaps be made of Anani Dzidzienyo's investigation of the Blacks' place in Brazil and George DeVos's piece on Japan's outcastes. Little, however, in the way of new information or interpretation is provided in any of these case studies; indeed half of these are really only updated summaries of more extensive treatments available elsewhere.

But more frustrating is the lack of any integrating framework or concepts. The individual cases are not presented according to any single scheme, though most authors adopted a historical-descriptive approach. Worse still, the reader is never informed what the various oppressed racial, ethnic, national, religious and caste groups studied have in common, aside from their oppression. How, for instance, is the Asian "problem" in East Africa similar to the position of Blacks in Brazilian society or of Catholics in the Soviet Union? Since no one provides an introductory statement suggesting how the various studies interrelate, the reader is simply left to flounder in a welter of disparate empirical detail. The only piece to offer a general perspective—a four-page "Afterword" entitled "Who's to Blame?" by Richard and Hephzibah Hauser—is not only nugatory in implying that scapegoating is *the* cause of group oppression, but is also confusing in failing to refer to any of the preceding studies.

One is bound to conclude, therefore, that the good intentions of the editor and the Minority Rights Group were aborted by the organization, or lack of organization, of the book. This failure to come to grips with the generic features of oppression is perhaps explicable in terms of an assumption which seems to underlie the M.R.G.'s approach—that a dominant majority (or minority), once it is awakened to its own shortcomings and oppressive conduct by scholarly treatises and "informed world opinion," will relent and reform. As the editor rather naively remarks (on p. 10), the

M.R.G. is "concerned with the problems [of minority rights], not the politics." But any person or group who wishes to change the world cannot avoid a concern with politics, especially with the structure of power and vested interests. The facts of oppression in themselves do not provide an explanatory framework or guidelines for action; these facts do not speak for themselves.

RICHARD SANDBROOK
University of Toronto

Weapons Technology and Arms Control. By W. F. Biddle. (New York: Praeger Publishers, 1972. Pp. 355. $20.00.)

The author, described by his publishers as "a nuclear scientist in British government service," has, while falling short of his apparent aspiration, nevertheless written a useful book. His professed aim (pp. 3f.) was to attempt "to bridge the gap that exists between the scientists and technologists who are involved in the design, procurement, and use of weapons, and the diplomats and other experts who are responsible for designing and negotiating agreements." He goes on to say that although many other studies have attempted to examine the effects of new weapons on international relations, "as far as is known this is the first time that a comprehensive study has been attempted."

Well, the book lacks much in comprehensiveness, and it is certainly stronger on the technological side than it is on the political or diplomatic. One senses its weakness in the latter area with the first substantial quotation in the book, which opens chapter 2. The *aims* of arms control the author believes to be quite adequately stated in a passage by Herman Kahn and Anthony Weiner which reads as follows:

> [the aims are] . . . to improve the inherent stability of the situation, decrease the occasions or the approximate causes of war within the system, and decrease the destructiveness and other disutilities of any wars that actually occur. One may also add to this last, "decrease the cost of defense preparation," but we would argue that this would take a rather low priority to the first three objectives. (quoted on p. 9) Bose, p. 42:

Inasmuch as political scientists have not been very good about re-examining the aims of that arms control to which many of them devote so much attention, one can hardly censure members of the physical science guilds for taking it for granted that the appropriate aims are always in the direction of saving the world. I would on the contrary argue that saving money is usually the most feasible goal and one never to be despised. Certainly the SALT accomplishments thus far have been in the direction of saving money—the only goal that both sides had fervently in common—and no one should disparage those accomplishments on that account. At any rate, the issue here can hardly be dispatched with a sentence or two, quoted or otherwise.

The utility of the book lies in its being simply a layman's guide to some of the military technologies that arms control is all about. It is devoted overwhelmingly to nuclear weapons of all kinds and their associated delivery (including defensive ABM) systems. There is also, however, some modest discussion of conventional weapons and a chapter devoted to "Chemical and Biological Warfare."

One notices a few minor errors. For example, it is not true (p. 179) that the difference between the Sentinel and the Safeguard ABM systems was that the former "was designed as a thin system" and that the latter was designed as a ICBM defense force. The earlier system was called "Sentinel" long before Secretary McNamara made his half-hearted proposal of September 1967 to deploy it thinly against China only, and Safeguard was exactly the same system with a different name adopted for political reasons by President Nixon when he wanted to emphasize a change in the aim. This and similar errors, however, are of no importance to the political scientist who is interested only in getting a better understanding of military technologies, especially those of nuclear warfare.

The price of the book will frighten away all but the most devoted, and along with the dull style will make it likely that the book will be used as a reference book on library shelves rather than as something that one reads straight through.

BERNARD BRODIE
University of California, Los Angeles

The Superpowers and the Middle East. By Tarun Chandra Bose. (New York: Asia Publishing House, 1972. Pp. 208. $6.50.)

This work is an excellent illustration of how *not* to write a book. It contains so many elements proscribed by style manuals that it probably would not have been passed by a Ph.D. committee in any reputable university. It is all the more surprising, therefore, to find that the volume is not the work of a beginner but of a seasoned professional, a Senior Reader in International Relations at Jadavpur University, who undertook "research for the major portion of this book at Harvard University and at the John F. Kennedy Library" as a Fulbright-Hays Senior Professor (the jacket).

The stylistic errors include occasional misspellings as well as other liberties with the English language which should have been detected by professional editors. There is rampant sloppiness and inconsistency, as, for instance, in the use of

capitalization. Fedorenko is Federenko throughout. Mahmud Riyad (pp. 93, 207) sometimes becomes Mahmoud Riad (pp. 134, 145). Whereas on pp. 27 and 34 Nasser is said to have nationalized the Suez Canal, on pp. 32 and 43 it is the Suez Canal Company, leaving the uninformed reader wondering which is correct and why the distinction was made in the first place.

Even more serious is the author's apparent difficulty in distinguishing between partial quotations and paraphrases. A check of only two sources yielded at least four instances in which Bose's language is uncomfortably close to that of the author cited. For example:

Bose, p. 42:
Soviet action during the Suez crisis, as a matter of fact, made a very strong impression in the Arab world where events in Hungary passed practically unnoticed. Nasser officially thanked the Soviet people and government stating that the Soviet help "reflected Soviet-Egyptian friendship."

Laqueur, *The Soviet Union and the Middle East*, p. 239:
. . the Soviet action made a very favorable impression in the Arab world, where the events in Hungary passed unnoticed. . . . Gamal Abdel Nasser, in a talk with Soviet journalists, officially thanked the Soviet people and government: "The help that we received reflected Soviet-Egyptian friendship, . . . "

In addition to stylistic shortcomings, the book contains numerous factual errors. The assertion that Stalin was trying to "subvert" Greece (p. 3) has been refuted by Milovan Djilas. The use of Soviet "volunteers" was threatened not during the Suez war (p. 37) but on November 11, five days after the end of hostilities. The statement that "practically everybody in the Arab world believed" that Moscow's threats had achieved the cease-fire in Egypt (p. 42) cannot include Nasser who, in a speech of March 22, 1959, accused Khrushchev of indecision in Cairo's hour of crisis. Kassem was a colonel, not a general, in 1958 (p. 52).

The narrative is further weakened by serious omissions and highly questionable interpretations. In the first category, to list only the most blatant examples, the discussion of Khrushchev's diplomacy during the Lebanese crisis (pp. 53–54), of the implications of the 1959 Cairo-Moscow crisis (pp. 75–76), and of the Premier's 1964 visit to Egypt (p. 76), is, at best, scanty. The author also displays a lack of awareness of the connection between the deployment of Soviet naval vessels in the Mediterranean and the permanent stationing there of *Polaris* submarines.

Equally disturbing, in terms of interpretation, is the author's inability to get away from the stereotyped clichés about the nature of US-Soviet interaction in the Middle East. Thus, the Baghdad Pact is presented as a bulwark against possible Soviet aggression, while the serious threat to Soviet security posed by the US Sixth Fleet is disregarded. Similarly, the presence of US air and missile bases in the Mediterranean countries is treated in a cavalier fashion: only in one instance (p. 82) does the author admit that "the establishment of military bases by the West in . . . [Iran and Turkey—but not elsewhere] made Soviet 'southern belly' *somewhat* vulnerable" [Italics added]. Similarly incomprehensible are the references to Middle East oil as being "no longer . . . of crucial importance to Western security" (p. 123) or to the Soviet Mediterranean squadron acquiring "a tremendous position of power at both ends" of that sea (p. 116, n. 79).

In summary, the book shows no evidence of thoroughness in the study of available sources, and it contains no new insights or information. After a vain search for some redeeming features, I am driven to the inevitable conclusion that both the author and the publisher have contributed to further "pollution" of a market which abounds in second-rate studies of a complex and, in some respects, still relatively unexplored subject.

O. M. Smolansky
Lehigh University

The Retreat of American Power. By Henry Brandon. (New York: Doubleday & Co., 1973. Pp. 368. $6.95.)

This contribution to the nascent literature on Nixon foreign policy made by Henry Brandon, the chief American correspondent of the London *Times*, has the material strength of sound information; gains firmness from intelligent selection by a seasoned observer; and is characterized by the journalist's flair for reporting stories with an eye for personalities, detail, and color. The central theme of the nature of American involvement in the world and the secondary theme of the new character of American policy are allowed to emerge from the discussion rather than be formed through systematic, closely focused and sustained argument. Brandon forces attention to the vexing question of whether America has the capacity or will for leadership in a multipolar world. A serious thematic flaw is that since the author almost exclusively focuses attention on the difficulties created by the phenomenon of the diminution of American involvement, he does not adequately develop the Nixon global strategy's positive thrust toward a new structure of international politics.

Brandon describes and comments upon the personalities and understandings of Kissinger and Nixon; decision-making and crisis management processes; the episodes of the Cambodian incursion, the Hussein-Fedayeen conflict, the India-

Pakistan war, and negotiations over devaluing the dollar; the sequence of steps toward detente with Russia and China; and the broad areas of European, Middle-Eastern, Vietnamese, and defense policies. He concludes that the systematic Kissinger National Security Council analyses are superior to those which emerged from the lunch meetings of the Kennedy and Johnson years. His outline of official thinking on Vietnam is succinct and convincing. While his commentaries are not always complete, in all cases a journalist's method joins with care, accuracy, and solid data.

The thematic title, the retreat of American power, refers not only to modification of American international involvement, but also to diminished American power, daunted American will, and accompanying trauma. On the policy level, the word retreat indicates a shift to "safer and more solid positions" (p. 81) and a movement from dominance to mere leadership. The author believes that this transition and the new practice of unsentimental multipolar balance of power politics will be very difficult for the United States.

Brandon's information gives additional credence and specificity to certain conclusions or observations of the type which only can be deduced or inferred from public statements. For example, with respect to buying time as a policy goal, he reports that Kissinger "believed that an interval of about two or three years was necessary to shield the United States from events which could occur between the departure of the American troops and the collapse of the South Vietnamese regime" (p. 62). Another example, illustrating the inferrable Nixon-Kissinger sense of molding policy with attention to evolving conditions, is his report that Kissinger observed that if "the North Vietnamese would take the same long term view of their future relationship with Saigon as the Chinese do in regard to Taiwan, there would be no problem of finding a settlement" (p. 193).

The rationale of the Nixon foreign policy has been well articulated in the President's annual reports to Congress (which should remain the point of departure for the serious student). A "new era" presents opportunities as well as limitations. New power and interest configurations and the practice of Realist diplomacy and accommodation could constitute a stable structure of international relations. Brandon does not explore this argument.

Among the deficiencies which follow directly from not doing so is Brandon's inability quickly to see the place of the initiation of the dialogue with Peking. Brandon says that the Peking visit announcement "left one wondering how this latest move fit into a coherent picture of his world outlook" (p. 181). In fact, the place of accommodation with China, the importance of initiating a dialogue, and the nature of the steps to be taken had been indicated in the annual reports to Congress.

Another deficiency is that while Brandon is concerned about whether anyone else will be able to assume management of the "intricate weave of the Nixon-Kissinger diplomacy" (pp. 358–359), he does not focus on that which sustains it. He does not systematically articulate the base upon which successful management or manipulation, or the grander goal of restructuring international relations, ultimately rests: that understanding of the application of principles of international politics which, in fact, has given coherence and effectiveness to the Nixon-Kissinger foreign policy.

FRANK BURD
University of Maryland, Baltimore County

The Peace Conspiracy: Wang Ching-wei and the China War, 1937–1941. By Gerald E. Bunker. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 327. $13.50.)

In this important study, Gerald Bunker unravels the tangled attempts of a small number of Chinese and Japanese to bring the Sino-Japanese War to a negotiated end, and shows how this process involved the transformation of Wang Ching-wei from a nationalist leader to a despised traitor. Wang advocated peace from the very beginning of the war, but at first it was only one weapon in the Kuomintang's arsenal of defensive maneuvers, the counterpart of Chiang Kai-shek's advocacy of armed resistance; the Kuomintang used one or the other, depending upon the immediate situation. Only later, after the Japanese had declared their refusal to deal with Chiang under any circumstances, did Wang conceive the notion of realizing peace through the establishment of a rival Kuomintang government which would exact maximum concessions from Japan, retain maximum rights for the Chinese, and in that way avoid the utter destruction of Chinese civilization which Wang thought total Japanese military victory would certainly bring.

Wang failed because his assumptions were false. He believed that the Japanese really wanted to cooperate with a stable, independent China, a belief that completely misjudged the extent of Japanese appetites. In a sense, Wang was mistaken to think that he could deal effectively with the Japanese in any way; Bunker shows that the Japanese government and military were so divided by factionalism and rivalries that it was difficult for the Chinese to find anyone who could declare a policy that somebody else would not promptly repudiate. Wang's crucial assumption was that Japan would inevitably defeat China if the matter were left to the battlefield. That premise might have proved correct if events at Pearl Harbor had not suddenly altered the situation by

bringing the United States into the struggle. From that time, the ultimate defeat of Japan seemed certain, and resistance was the only feasible goal for nationalistic Chinese. Whatever logic Wang's actions originally may have had disappeared.

How could a man of Wang Ching-wei's experience think he could conciliate the Japanese by himself, and persuade them to make concessions sufficient to make their presence acceptable to Chinese nationalists? Bunker answers this question in an intelligent fashion, but he is not completely persuasive. He acknowledges that Wang's policy was one of "make-believe" (p. 205), but he emphasizes the "nobility of Wang's desire for peace and the sincerity of his devotion to Chinese interests" (p. 257). Generally, Bunker views the participants in the peace movement as men of good intentions who sought genuinely nationalistic goals in impossible circumstances; they failed, but their motives were pure. But can either nobility or sincerity explain Wang's attempting to persuade Chiang Kai-shek to sue for peace by lying to him about Japan's readiness to make concessions? Can they explain the persistence of Wang and his associates when repeated experiences proved that the Japanese would provide them with not a tittle of the substance of power, and little enough even of the trappings? At one point, Bunker himself is forced to explain their persistence in terms of their being "unemployed politicians" who wanted "to play the game of politics that they loved on the great stage" (p. 215).

Bunker provides much new detail about the people and events involved in the peace movement from its beginnings in vague speculation to its tragic end. It is a complicated story, and the general reader may not appreciate Bunker's extensive use of transliterated words and phrases and his inclusion of the names of many secondary characters who appear only fleetingly. Bunker's careful research, however, not only answers many questions about Wang Ching-wei, the nature of Japanese factions, and many other important aspects of Chinese and Japanese history, but also indirectly illuminates perenially significant questions about the ways in which politicians deceive themselves and ignore realities, and about the moral uncertainties which inevitably confront men as they try to cope with war and revolution.

JAMES E. SHERIDAN
Northwestern University

America and the World Political Economy. By David P. Calleo and Benjamin M. Rowland. (Bloomington: Indiana University Press, 1973. Pp. xii, 371. $12.50, cloth; $2.95, paper.)

America and the World Political Economy could not have been published at a more opportune time. Even a cursory glance at the front pages of any newspaper reveals that as some of the more pressing conflicts of the Cold War are receding from our attention, they are being replaced by new international problems that have an increasingly visible economic component. The symptoms of mounting difficulties are manifest: taxes imposed by Washington to discourage American investment abroad, repeated devaluations of the dollar, collapse of the Bretton Woods monetary system, worldwide inflation, "gold fever," questions about the domestic and international role of ITT and other multinational corporations, shortages of foods that were recently a glut on the market, the "energy crisis"—to say nothing of veiled and sometimes not-so-veiled threats and charges of economic warfare emanating from officials in Washington, Tokyo, Paris, and other major world capitals. The present international economic difficulties and their intellectual roots are the subject of this timely book.

In a brief introductory chapter, Professors Calleo and Rowland describe the American vision of a world economic order as an imperial one, but of a far different sort from that attributed to Washington by some other critics of American policy. The American vision is pernicious in ideology and rhetoric, but it has often been benign or even generous (e.g., the Marshall Plan) in practice. Part II is addressed to the intellectual roots of the Atlantic Community, with special attention to three contributing schools of theory: free trade (liberal economic theorists, Cordell Hull), geopolitics (Mahan, Acheson, Dulles), and federalism (Madison, Clarence Streit). The economic and political sources of contention within the Atlantic Community are discussed in Part III, with chapters devoted to the monetary system, trade, and international corporations. Although the main focus of the book is on the Atlantic Community, in Part IV Calleo and Rowland turn their attention to other areas, especially Japan and the Third World. In a brief conclusion the authors make clear that they are not opposed to a liberal world economic system, but only to the rigid version that has dominated official American thinking at least since the first FDR administration, if not that of Woodrow Wilson.

Underlying the analysis of the present economic crisis in the Atlantic Community are several themes:

(1) The difficulties that beset American policy can be traced back to failures of intellect and imagination—"superannuated and rotting idols that still enchant the American mind"—rather than to inherent flaws in liberal-democratic institutions. At the core of the problem is an ideology that fails to distinguish between liberalism and antinationalism, federalism and hegemony, and interdependence and dependence. The result is a

specious imperialism masquerading under the cloak of internationalism.

(2) The political economy is too important to be left to economic theorists, who often fail to appreciate Edmund Burke's distinction between a nation and a commercial contract. Political theory has tempered the classic laissez-faire system at home; it must, according to Calleo and Rowland, perform a similar role with the real or potential excesses of liberal free-trade doctrine.

(3) Above all, the integrity of the nation-state must be preserved. The liberal-democratic nation-state—the "highest form of rational organization man has yet developed, or will develop in the foreseeable future" (p. 253)—is the only alternative to chaos. The Calleo-Rowland critique of free trade ideology is not so much that it is bad economics, or that it doesn't "work." It is, rather, that free trade may threaten the ability of the nation-state to maintain rational control over the domestic economic environment—for example, when one nation uses trade as a means of exporting its domestic problems and inbalances to others. Calleo and Rowland accept neither the internationalist theory that the nation-state is an obstacle to peace, nor the functionalist thesis that economic interdependence might contribute to peace by diluting some of the effects of political rivalries. Indeed, they assert that the planned, democratic welfare state (which is inevitably mercantilist) is the greatest hope for peace in today's world.

The strengths of the book are many, not the least of which is its felicitous style. The work is well documented, although the placement of many tables at the end of chapters or in the back of the book does not add to their utility. By tracing the intellectual roots of present problems from the classical liberal economists through the British imperial experience, Wilson, Hull, and the Cold War era, Calleo and Rowland have achieved an impressive perspective on the problem.

The prescriptions set out in *America and the World Political Economy* will not please everyone. The authors assert that, just as the developed nations of the Atlantic Community must learn to absorb their own production, so must nations and regions of the Third World. This may be rather cold comfort for nations with one-crop or one-commodity economies. Aside from possible economic difficulties this solution might encounter, one might question whether its acceptance will be politically feasible for leaders of Third World nations. Second, Calleo and Rowland are unquestionably correct when they assert that many areas of American life (e.g., urban transportation, housing) have been neglected. It may be less evident that replacing market mechanisms with a political reordering of resource allocation will prove to be a wholly effective alternative. Finally, the authors end with a declaration that, "The age of the minimal state should now be over." The assertion may be true, but in the post-Watergate era perhaps one can look upon this tendency as a mixed blessing.

OLE R. HOLSTI

Duke University

Fundamental Guarantees of the Parties in Civil Litigation: Studies in National, International and Comparative Law. Edited by Mauro Cappelletti and Denis Tallon. (N.Y.: Oceana Publications, 1973. Pp. 821. $25.00.)

Increasingly, American political scientists have expressed interest in moving the research on public policy and decisional behavior beyond the parochialism of national study and into a comparative framework. But the obstacles facing any such project are great. With the way cleared years ago by such comparative institutionalists as Rudolph von Ihering and Edward McWhinney, forays into comparative judicial behavior have successfully been made lately by Theodore Becker, David Danelski, Glendon Schubert, and others. But comparative public policy largely remains *terra incognita*, and where guiding theory is concerned, we have neither chart nor compass. We stand in the prescientific stage of requiring case studies. There are not many, and those who produce them—legal scholars in the field of comparative law—are largely strangers to political scientists. Their categories of analysis are different from those used by social scientists and, as a literature search through the *Index of Foreign Legal Periodicials* readily shows, they often write in French, the language of international law.

To the specialist interested in the newly developing field of comparative public policy, procedure, and behavior, a very useful beginning with the staples of basic information can be made with the volume edited by Mauro Cappelletti and Denis Tallon. Prepared at the request of UNESCO, it reflects the papers, proceedings and discussion of the Florence Conference of the International Association of Legal Science (1971). The lengthy and awkward title of the book suits the two-inch thickness of the weighty volume. But it should not deflect from the vitality and sharp focus of the work. The constitutional rights of litigants in civil proceedings have never before been the subject of systematic study on a worldwide comparative level. National and regional reports are presented in practical and in some instances "realist" terms, not merely doctrinal terms. National reports were sought from quite varied political systems: economically developed liberal democracies, people's democracies, and third world systems. The descriptive quality of these reports is

generally high, though the surveys of Danish and Senegalese developments are sadly lacking in documentation. Fundamental guarantees for civil proceedings in most of the essays are separated into (a) guarantees concerning the judiciary, e.g., independence and impartiality; (b) guarantees concerning the parties proper, such as their right of action and defense, their right of effective access to courts, their right to counsel and legal aid; and finally (c) guarantees concerning the unfolding of the proceeding and proof-taking, e.g., adequate notice, admissibility or exclusion of illegally obtained evidence, publicity and orality of the hearing, and the written form of the decision including their grounds.

Sixteen reports are published, nine of which are in French. The balance are in English, as are the introductory comments, the editor's conclusions, and the very interesting and lively summaries of discussion. Twelve of the essays concern single national systems: Austria, Canada, Denmark; England, France, Germany, Italy, Japan, Senegal, Switzerland, the Soviet Union, and the United States. A particularly detailed analysis of rights associated with civil proceedings under the European Convention of Human Rights is offered by Jacques Velu of the University of Brussels. Zhivko Stalev of the University of Sofia has written a survey of the law of the European People's Democracies offering details of the East European judicial process nowhere else available in English and including some fragmentary serial data on the speediness of judicial proceedings. Two lengthy essays are concerned with Latin American systems. That by Héctor Fix-Zamudio is the more profound, and it is notable for achieving sophistication at the level of Latin American intersystem comparisons. Fix-Zamudio analyzes the gradual modernization of civil procedure transformed from one country to another in terms of the progress of traditional systems of formalistic justice into modernized, egalitarian, and responsive processes.

Several of the essays should be instructive to political scientists in emphasizing that judicial behavior is by no means the only relevant decisional behavior of importance in policy fields related to rights and liberties. If "justice," as Prof. Stalev of Yugoslavia commented, is "the solution of legal disputes with *res judicata* effect, using a procedure granting specific guarantees to the parties, then all bodies conducting such activities can be called 'justice-making bodies' " (page 783). Systematic comparative study of decisional behavior and policy among boards, commissions, tribunals and other parajudicial "justice-making bodies" has yet to be initiated by political scientists, though it is familiar territory for students of comparative law as illustrated by the Cappelletti and Tallon volume, especially the essay on Canada by Garry Watson of York University.

The concluding essay by Mauro Cappelletti, though by no means a summary of the various national reports, offers some effective comparative analysis of litigation processes among systems worldwide, and convincingly restates a theme problem which clearly absorbs the interest of all the contributors, viz.: how to make justice accessible to all. He writes: "As a consequence of profound societal changes, the principal issues of our day . . . reflect the problems of an industrial, dynamic, pluralistic society—including such matters as strike and labor conflicts, social and traffic insurance, pollution, consumer protection, and multinational corporations" (page 725). The conclusion, addressed from many points of view throughout the volume, is that law in rapidly changing societies must be open to reform and institutional change in keeping with its new "social role." Cappelletti describes that role as embracing the entire society: "One essential—possibly the essential feature of a really modern system of administration of justice must be its effective and not merely theoretical accessibility to all" (page 726). With such language permeating this interesting volume, it should be clear that it is about time for political science students of comparative development, behavior, and policy to get on speaking terms with students of comparative law.

RICHARD PIERRE CLAUDE
University of Maryland, College Park

A World Elsewhere: The New American Foreign Policy. By James Chace. (New York: Charles Scribner's Sons, 1973. Pp. 108. $5.95.)

"Postwar American foreign policy has been set on a new azimuth," says the managing editor of *Foreign Affairs* and author of this little volume; "there is no turning back" (p. 10). Of what does the new direction consist? It appears to be at once both Gaullist and Monnetist, Nixonist, and Kissengerist. It is rooted in awareness that the worldview formulated to contain Communist territorial expansion is obsolescent and that a more centralized international system, better able to cope with problems like monetary stability, trade barriers, pollution, and population control is preferable. It is intended to improve relations with our ideological rivals, to be compatible with changing relationships with old allies, and even afford a position within the global balance ultimately favorable to us. The specters of Metternich and Bismarck lurk in the background as if to give the process a measure of historicity and legitimacy. It is with these roots of the new foreign policy that the first chapter of Mr. Chace's book is concerned.

The second is devoted to the problem and process of balancing the powers, an admittedly difficult matter with nationalism on the rise in the world. At least one takeoff point is the Nixon Doctrine, in its starkest form averring that "we will help those who help themselves." Its ephemerality becomes evident, however, with the observation that "It's his doctrine, and he can damned well do what he wants with it," attributed to Professor Kissinger. That substantive change has occurred is undeniable in light of the re-establishment of relations with Communist China, an economic stand that withdraws the dollar from use as the currency for financing world trade, the conclusion of agreements with the Soviet Union to limit the deployment of weapons of mass destruction, the attempt to play the game of balancing the powers on a world scale, and the retreat from maintaining a global gendarmerie while pretending that we can maintain our commitments. The accords of 1972 tend to establish the new direction, of course, but it is well to remember that doctrine, like the actors, is manipulable. One need only advert to the Yom Kippur War of 1973 and the tension aroused, detente notwithstanding.

The third chapter is "On Navigation" insofar as it deals with piloting through the tides and currents, the shoals and "shelfy Coast" that threaten to make the course difficult. The current we must ride is the new attempt at equilibrium; a world wholly new to the postwar imagination, no longer bipolar, lies ahead. It will involve many overlapping coalitions and power relationships like the central strategic balance between America and Russia; the North-South axis, such as the European-African connection; and the movement of transnational forces, like economic agents and agencies, personnel and skills, even ideologies. America, it is concluded, in her search for new directions in her foreign policy, must pursue two lines simultaneously: she cannot disregard the realities of the nation-state and the "balance of power" politics that it implies; she must, concurrently, avoid the excesses of neonationalism—with its concomitant unilateralism —while finding ways of devising new relationships that will cut across existing alliances.

This is a slender volume, dispassionate, analytical, lucid, and logical in structure. From those associated with *Foreign Affairs* we expect solemnity and respectability, and this is what we find here; in a certain sense what is contained in Mr. Chace's book smacks of the prevailing apologia for current policy as proclaimed and practiced by the Nixon-Kissinger Administration. Conceivably more in the nature of criticism might have been forthcoming, obligatory as is such function on the part of academicians and theoreticians like Mr. Chace. He says little, for example, about the economic future, emphasizing throughout and perhaps alone the undeniably important political security area. He offers little in the way of explanation of why the Vietnam incursion was desirable or necessary as part of the total mosaic, and yet eschews criticism of it. He does not dare to venture courageously beyond the nation-state principle, as, indeed, man may yet have to, to overcome the insecurities of all the earlier ages. Nor does he underscore the power of both justice and morality in the resolution of international tension and conflict, wanting in no small measure as they are in the affairs of men, abstract as they frequently are in the minds of men. He does, nevertheless and notwithstanding, provide us with a set of valuable insights into the views that vivify the foreign relations sector of the United States government.

HILLIARD A. GARDINER
Queens College, CUNY

Soviet Policy toward Black Africa: The Focus on National Integration. By Helen D. Cohn. (New York: Praeger Publishers, 1972. Pp. 334. $17.50.)

This valuable volume presents a thorough analysis of Soviet theories on the nation-building process in black Africa. Professor Cohn makes excellent use of her training in both Soviet and African studies. Her book is one of three recent studies which have done much to deepen our knowledge of Soviet policies toward Africa, the other two being Robert Legvold's *Soviet Policy in West Africa* (Cambridge, Mass.: Harvard Univ. Press, 1970), and Edward T. Wilson's *Russia and Black Africa before World War II* (New York: Africana Publishing Corp., 1974). All three studies document the thesis that Russian national interests are paramount over Soviet ideology in shaping policy toward Africa.

In contrast to Legvold's general analysis of Soviet policy efforts in six West African countries and Wilson's detailed historical treatment, Professor Cohn has concentrated on one main topic—problems of national integration in Africa as viewed by Soviet academic and governmental theorists. She questions Western views which stress the lack of objectivity in Soviet writing and the subservience of African studies to government policy. She acknowledges the doctrinaire ideological approach of the Stalinist era when the "zigzag" course of the International Communist Movement resulted in Communist loss of "any real influence in the colonies" (p. 24). The Khrushchev regime, however, inaugurated a more pragmatic assessment of nationalism, a trend that flowered when Leonid Brezhnev and Aleksei Kosygin came to power. In place of the usual fostering of African nationalism as a weapon in the anti-imperialist struggle, nationalism came to be regarded as a "positive phenomenon" only if it

strengthened the Soviet position in international affairs (p. 35); otherwise it was opposed, the Biafran case being the prime example. Although the Eastern Nigerian leaders had earlier been praised as the progressive forces who were resisting "the feudalists and comprador bourgeoisie of the Northern and Western regions" (p. 169), in the Biafran rebellion the Soviet Union supported the stronger Federal Government against the weaker Ibo rebels in order to secure access to Nigerian markets, to displace British dominance, to impress the OAU and many black African governments, and for other pragmatic reasons.

The author points out that the death of I. I. Potekhin (the leading Soviet Africanist) in September 1964, and the deposition of Khrushchev in October made possible a flourishing debate on the applicability of the Soviet nation-building model to Africa. The 1966 declaration of the Brezhnev-Kosygin regime that the domestic development of the Soviet Union was its primary internationalist duty, was accompanied by a consequent waning of official interest in Africa, which freed the academic community from some of its earlier constraints.

The analysis of the ensuing debate on national integration in Africa is the heart of Professor Cohn's book. The Soviet model on nation building had emphasized a dominant nationality group, universal education, a single national language, and the eradication of religion and traditional cultural values. The inapplicability of this model to most African countries was already apparent to Soviet scholars who, toward the end of the Khrushchev era, had begun to urge a gradual approach toward social reform. Now the Soviet Africanists, somewhat like those in the United States, began to acknowledge the progressiveness of some of Africa's military leaders. Soviet scholars pointed out that the vanguard parties used in the integration of the U.S.S.R. could degenerate in Africa into institutions with little contact with the masses. The development of socialist economies and political parties would therefore be a slow process.

The right of self-determination continued to be stressed, but the Brezhnev-Kosygin regime allowed increased pragmatism and objectivity in the academic world because they were compatible with such official objectives as reducing the economic drain to Africa, promoting African stability so that official attention could concentrate on the higher priority Middle East and Southeast Asia, and establishing stronger government to government relations with as many African states as feasible. The author thus relates academic writings to government policies, giving her study a wider significance.

Although the task of organizing her material on Soviet nation-building theories was not easy, Professor Cohn has done it effectively in two thorough chapters (pp. 77–180) on short-term solutions to African centrifugalism in the social and political spheres, followed by two chapters on longer-term answers (pp. 181–255). The short-term solutions deal with educational, religious, cultural, and political measures suggested by Soviet writers to accelerate the nation-building process, while the long-term answers concentrate on economic development (preferably noncapitalist), and on the elevation of the progressive sectors (intelligentsia, bureaucracy, and military) to the dominant role in the political system.

These four chapters illustrate with abundant quotations how Soviet scholars moved away from their preconceptions toward a more realistic assessment of African complexities. The author also shows how Soviet rethinking paralleled in some respects the reconsideration by Americans of their own views on Africa.

The dominance of Soviet pragmatism over ideology is demonstrated not only in the Biafran case, but in the willingness to set aside the Stalinist definition of a nation when the African states were striving for independence, and to support unification of French and British Cameroons. This flexibility was also evident in the views of Soviet analysts on the role of religion in Africa, the question of a national language, the problem of cultural relativism, and the acknowledgment that the military and the intelligentsia play a more important role than the workers or peasants in nation building.

The author has well demonstrated her initial hypothesis that "the decade of the 1960s has witnessed an increasing impartiality in the works of Soviet Africanists" (p. 4). Moreover, she suggests that, aside from possible conflict over Somalia because of its strategic location vis-à-vis the Middle East, there are unlikely to be enough incidents in Africa to induce the Kremlin to refocus its attention on that continent in the next decade. This, she concludes, "may well have the fortunate consequence of allowing scholarly objectivity to flourish among Soviet Africanists" (p. 269).

VERNON MCKAY
School of Advanced International Studies
The Johns Hopkins University

The Anatomy of Influence: Decision-Making in International Organization. By Robert W. Cox, Harold K. Jacobson, Gerard and Victoria Curzon, Joseph S. Nye, Lawrence Scheinman, James P. Sewell, and Susan Strange. (New Haven: Yale University Press, 1973. Pp. 497. $15.00.)

It is difficult to know who has done greater damage to the study of international organizations—their euphoric friends or skeptical critics.

Thus it is especially useful in this area to have more dispassionate, scientific analyses such as the book reviewed here.

This work does not pretend to cover the whole waterfront. It concentrates on the decision-making process in eight important specialized agencies —more specifically, "to identify influence, to weigh and assess it, and ultimately to explain how influence is acquired and exercised in international organizations." This, in itself, is a major contribution to a more sophisticated analysis of international institutions and is part of the broader stream of comparable studies of all political organizations, both national and international. The authors and chapter subjects are: Robert Cox and Harold Jacobson, "The Framework for Inquiry" and "Power, Polities and Politics: The Environment"; Jacobson, "ITU: A Potpourri of Bureaucrats and Industrialists"; Cox, "ILO: Limited Monarchy"; James P. Sewell, "UNESCO: Pluralism Rampant"; Jacobson, "WHO: Medicine, Regionalism and Managed Politics"; Lawrence Scheinman, "IAEA: Atomic Condominium?"; Susan Strange, "IMF: Monetary Managers"; Gerard and Victoria Curzon, "GATT: Traders' Club"; Joseph S. Nye, "UNCTAD: Poor Nations' Pressure Group"; and Cox and Jacobson, "The Anatomy of Influence."

Unlike the usual panels, this team of eight authors, led by the chief participants, Cox and Jacobson, worked long and diligently to agree on a rigorous and integrating comparative methodology which has made this work more internally consistent and productive than most such enterprises. As each author has dissected his organization, he has concentrated on several major aspects: general historical evolution, current structure and functions, relative emphasis on different types of decisions (representational, symbolic, boundary, programmatic, rule-creating, rule-supervisory, and operational), types and roles of major actors (representatives of governments, agents of private organizations, international bureaucrats, etc.), and patterns of interaction with national states and the international environment.

As a result of this analysis, this volume penetrates more deeply and objectively into the boiler room of international organizational decision making than any other work available. The authors emphasize that, contrary to easy generalizations about the sources and exercise of influence by either major or minor powers, the situation is extremely complex, varying greatly from agency to agency, issue to issue, and stage to stage of the international environment. While the most powerful states generally dominate, especially in agencies and on questions considered vital to their interests, even their influence is often hobbled by numerous factors, including the inadequacy of their own capabilities—financial, human and substantive—and the opposition of other nations. Other states may wield greater influence than their material condition would suggest because of other sources of influence, including skilled representation and the assumption of key roles as initiators, controllers or brokers. The relatively new and growing cadres of international bureaucrats, especially those at the top of their hierarchies, are capable of exercising crucial leverage because of their strategic positions at the helms of these agencies, their intimate knowledge of their agencies' business, and their roles as servants, and therefore confidants, of all the members.

Other findings include the following: (1) a general shift among the eight organizations toward aiding the less developed countries, with a consequent increase in field operations and less emphasis on using such "functional" institutions to move toward general multilateral integration; (2) the creation of "shadow organizations," outside the agencies with more universal membership, to handle problems of direct and major concern to the more developed countries; (3) increasing "bureaucratization" of the eight organizations with more initiative and influence being exercised by those who have been long and intimately associated with the agencies and greater momentum toward self-motivated growth; (4) a difference between agencies with greater consensus on organizational ideology (e.g., IMF and GATT) and those with greater ideological conflict (e.g., UNESCO and IAEA); and (5) a difference between organizations which are considered to be of more vital importance to the major powers and are thus more closely controlled by such governments (e.g., ITU and IAEA) and those regarded to be less crucial and thus allowed greater internal autonomy (e.g., WHO and UNESCO).

Strong as this work is, it has certain weaknesses, some of which are inevitable in such an enterprise. The sample of agencies studied must be selective, and the selection necessarily affects the conclusions. In these, as in any institutions, the patterns of human interaction are tremendously complex and cannot be easily or precisely analyzed. The simple typology of national political systems used by the authors (competitive, mobilizing, and authoritarian) scarcely does justice to the variety of polities which populate the international landscape. While the authors rightly emphasize that international organizations are part of networks of relationships which intermesh with national societies, the authors give little attention to the intranational structures and processes which relate most directly to multilateral agencies. And they draw no conclusions regarding the linkage between their analysis and the larger body of related studies concerning intranational organiza-

tional behavior, although the two aspects of human interaction are closely connected and relevant to each other.

The authors also join the perennial debate over the "functionalist" approach to international integration. Unfortunately, they concentrate their fire primarily on the traditional concept of functionalism rather than the "neofunctionalist" approach. The classical version argues that the most promising path to international integration is via decentralized multilateral enterprises whose activities are technical, functionally specific, essential, and divorced from more general, political agencies—both national and international.

This volume criticizes this thesis by pointing out that such enterprises cannot escape politics, that what is considered "essential" varies according to different national concepts and conditions, that most governments—large and small—do not want international integration, that the trend is not toward integration but toward using international agencies to help the underdeveloped nations, and that the nation-state is still the most effective instrument to protect the interests of its citizens. There is much validity in these arguments. Politics must, and should, be part of any system dealing with governmental institutions and policies. Nationally oriented groups do generally resist merger with other nations. And the nation-state is still the strongest political agency—for good or ill. Nonetheless, the evidence in this volume, and in other sources, demonstrates that functional agencies, such as those studied here, have made substantial and measurable progress in dealing with some of mankind's most pressing problems. No one can predict whether this trend will predominate over increasing intranational cohesion, but there is, at least, absolute movement toward greater international communication and cooperation.

H. FIELD HAVILAND
The Fletcher School of Law and Diplomacy
Tufts University

A Socioeconomic Profile of South Africa. By William R. Duggan. (New York: Praeger Publishers, 1973. Pp. xviii, 181. $15.00.)

This book is of interest to the student of U.S. policy toward South Africa because of the experience of its author. William Duggan has had a distinguished career in the Department of State, serving as African affairs specialist in the Policy Planning Council during the presidencies of Kennedy and Johnson and for seven years in South Africa, where he was consul general in Durban before his retirement in 1971. He has written a largely factual handbook, covering familiar ground. (The title is misleading since the book is as much concerned with politics and international relations as with society and the economy.) Of special interest, however, are his evaluations and judgments and his proposals for change in U.S. policy. Regrettably, the proposals are sketchy, and their implications are not worked out.

Mr. Duggan has no illusions about the quarter-century since the Nationalist Party came to power in 1948. The regime has become "increasingly repressive" in maintaining "absolute White domination" (pp. xiv–xv). Political freedoms have been "ever more severely curtailed" (p. 27). Although *verligte* and *verkrampte* Afrikaner Nationalists do not disagree fundamentally on White domination, their dispute has forced the government to move further toward the right. Meanwhile, the liberal White opposition has become weaker, and the opposition United Party has become "increasingly conservative" (p. 38). Although White fear has "intensified," Whites make the "false and dangerous presumption" that the "deceptive calm" means "a 'happy' acceptance by non-Whites of the status quo" (pp. 50, 166). Indeed, while emphasizing the strength of the regime, Duggan compares "racial turmoil in southern Africa" to a "boiling cauldron" that "could boil over at any time" (p. 172).

Duggan welcomes the government's Bantustan program of separate development because of its unintended possibilities. Although it is thus far "only a temporary rural palliative" that is useful for international propaganda, it has reawakened African nationalism and provided an official platform for a new Black leadership (p. 62). The heroic figure who has emerged is Chief Gatsha Buthelezi of the Zulu "homeland," a "responsible" leader. What is curious in Duggan's treatment of African politics is his slighting of the national appeal of the older African leaders and his characterization of the non-White majority as apathetic in the years before the African National Congress and the Pan Africanist Congress were banned. Buthelezi himself has recently described such leaders as Nelson Mandela (imprisoned for life), Robert Sobukwe (under ban), and Oliver Tambo (in exile) as "heroes to Blacks", and men whose participation in dialogue is essential if there is to be any hope of peaceful change in South Africa.

Despite the apparently hopeless prospect of such participation and despite his own analysis of South Africa's backward trends, Duggan retains a liberal optimism that the South African situation can be defused. Because the regime is not only morally offensive, but also endangers international peace, he believes that the ambivalence of U.S. policy, which has "softened" under Nixon, should be replaced by an "orchestrated strategy . . . of carrots and sticks" conducted

jointly by the United States and some other Western countries (pp. 147, 172). Since Duggan considers South Africa a part of the "free world," however, his policy does not appear radically different from Nixon's (p. 71). "Dissociation" is "impractical" because the United States and Britain are "heavily involved with South Africa in financial, trade, and defense areas," for example, in jointly countering Russian naval influence in the Indian Ocean (p. 172). One carrot, designed to promote "more constructive association," would be the resumption of naval visits to South African ports (pp. 171, 174). At the same time, Duggan would press for social and economic amelioration (not political equality) and building up the Bantustans, which would improve South Africa's "international image" (p. 171). The strategy would include diplomacy, persuasion, and time for "Western business ethics" to exert influence (p. 168).

Duggan's reference to such sticks as visa retaliation and penalties on substandard business and his suggestion that two-year deadlines be linked to stronger pressures whet one's appetite for fuller analysis. One hopes that he will provide such analysis in the future as well as deal with some fundamental questions that the book does not, for one reader, satisfactorily answer: Does not his approach strengthen rather than weaken White domination? And are not the risks of identification with a racist regime too dangerous for the United States to accept in the last quarter of the twentieth century?

THOMAS KARIS

City College, CUNY

Creating a New World Politics: From Conflict to Co-operation. By David V. Edwards. (New York: David McKay Company, Inc., 1973. Pp. xvi, 191. $2.95.)

Whatever the final evaluation, one must welcome Professor Edwards's bold attempt to apply theories of international relations and social psychology to long-neglected normative issues. He poses as his central question: "What can be done to improve the quality of international relations?"—to reduce the high incidence of violence, restrictions on freedom of action and choice, and the continuing gross inequalities between rich and poor? At the level of basic values, he suggests, differences among international theorists are secondary, and none offer promising policies for the realisation of these values. The "idealists" offered fantasies, the "realists" see no remedy for continued violence and injustice, and the "neo-realists"—those who place their hopes in non-intervention and reduced American involvement in world affairs—offer no convincing prospect of improved global relationships but may inadvertently worsen them. If involvement is inescapable, how can it be made to serve constructive ends?

He finds his answer in the notion of the social construction of reality. Social relations are not immutable but are continuously created by the thoughts, choices and actions of individuals. Where this is not conscious, outcomes tend to be determined by rigid images and routine modes of action ("processes"). Greater consciousness leads to awareness of opportunities, in particular opportunities for fundamental change. Perceived social constraints can be overcome when their mechanisms are understood. For example, the action-reaction processes discussed by theorists such as Lewis Richardson and Kenneth Boulding do not inevitably lead to the escalation of hostility and arms races: if we understand the processes involved we can bring them under control. Understanding includes self-criticism: "realists" should long since have recognized that their arms policies are not only costly in terms of welfare goals but self-defeating in terms of security goals. The threat of annihilation is not an acceptable long-term basis for security.

The author's basic conclusion is that only a radical change in the international system can improve the quality of international relations, but that this is far more feasible than is commonly recognized, since the constraints generally believed to sustain the system as, in the last analysis, a threat system, are not inherent in international reality as such, but in orthodox images of reality. What is needed, therefore, is a new image of reality, in particular a new conception of world order transcending the usual power-oriented conceptions, and reflecting the cultural differences in contemporary international society. The development of the youth culture is a promising sign but is no guarantee that new values will prevail.

A summary cannot do justice to Edwards's closely reasoned argument, but part of the difficulty is in the argument itself, which remains at a highly abstract level. There are no more than faint hints at what the new reality image and the new conception of world order might be. Since specific policy proposals are omitted, even for illustrative purposes, Edwards fails to expose the problems of his general approach, in contrast to earlier authors whom he cites with approval such as Charles Osgood and Amitai Etzioni. The problems may not be insurmountable, but they certainly need to be surmounted. *Are* the constraints of power politics, and the conflict patterns studied by game theorists, solely in our images? There is no discussion of the reasons which have led most students of international relations to think that they are not.

A second set of questions concerns the book's preoccupation with super-power relations. The issue of international justice, raised at the outset, rapidly fades from view, and the new reality seems to promise much more domestic than international welfare. It is true that the goal of survival in the nuclear age should not be complacently taken for granted, but the modest efforts at reality creation on the part of Kissinger, Nixon, Brezhnev, Brandt, and Chou En-lai suggest that accommodation among adversaries is now becoming easier than agreements among allies or the resolution of the new issues which are now presenting themselves: issues concerning energy or the oceans, the stability of the international economy or the wider environmental and ecological issues. In all of these, the role of the material world and material interests are inescapable, the role of "reality creation" less than in the relations of nuclear adversaries. The one-dimensional quality of Edwards's analysis does not do justice to multi-dimensional, and changing, international reality. In the last analysis, his book is more important for the questions it raises than for its answers to them.

J. L. RICHARDSON
University of Sydney, Australia

The Fortunes of the West: The Future of the Atlantic Nations. By Theodore Geiger. (Bloomington: Indiana University Press, 1973. Pp. 304. $10.00)

Those who write of the future run certain risks. Some of these may be obviated by setting the "future" sufficiently ahead in time so that neither writer nor reader can verify the correctness of the predictions. By the time the work is published, however, the baseline for forecasting may already have shifted, if it was set in the "present." Such a problem can presumably be met by pushing the baseline far into the past. This solution was tried by Theodore Geiger, director of international studies for the National Planning Association. He has chosen to discuss "the future of the Atlantic nations" by stressing "the wider social-institutional and the deeper psychocultural aspects of human decision making and action." In doing so he admits that "the determinative relationships" will be "so diverse, complex and ambivalent" that the projections are bound to be "qualitative and generalized" (p. 5).

Through a survey covering 2,000 years, the author devotes about one-fifth of his study to the origins of "Western redemptive activism" and "Western rationalism and individualism," qualities which he finds characterize the behavior and outlook of those living in Western Europe and North America. After comparing the sociocultural sources of the Soviet Union and the United States and finding that the foreign policy of both countries tends to be messianic, in their competing drives to transform the world, he then seeks to discover whether or not another world war is in prospect and comes to the tentative conclusion that it is not. From that point he assesses the possibility and likelihood of European unification, which he distinguishes from "integration," defined as "the removal of barriers to trade and capital movements among a group of countries" (p. 130). Next he examines how the countries of Western Europe will be related to those in North America, with special attention to the economic aspects. Finally he takes a big jump into the 21st century to make some admittedly speculative projections of the possible transformations in Western society.

One of the underlying themes of the book is that changes will flow from the emergence of a new breed of "technocrats" just coming of managerial age in the United States and soon to emerge in Western Europe. He seems to expect that these professionals, well trained in the requisite skills to handle the complex problems of a complex society, represent an improvement on the political amateurs who preceded them. Nevertheless, he occasionally expresses some concern that their own sense of world mission may need to be disciplined. His observations on the technocrats are one of many signs that the book was written with a perspective from the Kennedy years. Since the revelations of Watergate, a decade later, the reader is not so certain that the skilled manager of public affairs may not sometimes be deficient in some other qualities than professional modesty. The old-fashioned politician is better than he appears in this book.

Thus an analysis which purports to look ahead to a time several decades from the present tends to be peculiarly unhelpful on the years just past or about to come up. Whether or not actually written before the 1970s, this book provides relatively little which could explain such developments as SALT, MBFR, and West Germany's *Ostpolitik*. There is only a side-glance at environmental issues, the questioning of economic growth, and the demands for population control. Although the author mentions the Vietnam War from time to time, and although he states that it was a prime cause of Western European dissatisfaction with American policy, he seems to underestimate its very deeply disruptive effects upon all aspects of society, economics, and politics in the United States. Nor does he allude to another form of violence which has increasingly afflicted advanced countries in a frightening form of transnationalism, the terrorist attacks on innocent people in the name of political causes espoused not by governments but by extremist

minorities. "Europe" means Western Europe, but even in Western Europe Scandinavia is scarcely mentioned, and the relationships of Western European countries to those in Eastern Europe are almost totally ignored.

The author is by profession an economist, which may explain why he overlooks the contributions of political scientists in many ways, (True, that in his 21st century projections he argues for a new kind of academic specialist who will in fact be a *generalist*, illustrating this need by referring to his own experience in finding his expertise inadequate for handling some technical questions.) It is not just that in listing the new breed of technocrats he includes all the social sciences except political science, nor is it the general absence of political scientists among the authorities he cites. His analysis ignores the recent literature on the subjects he treats, and he repeats old patterns of dealing with international relations. He makes use of the outmoded billiard-ball model of state behavior even if occasionally referring to elite groups within some states to distinguish them from the masses. In discussing the possibilities of a united Europe he speculates on whether, once formed, it might also try to expand its power. Thus he unconsciously adopts the "power politics" view of the state inevitably seeking to raise its position in one hierarchy of competing powers.

Theodore Geiger accepts unquestioningly the idea that to be a very great power (to distinguish such from the superpowers he coins the term "proto-superpower") the possession of nuclear weapons is necessary. This assumption leads him into somewhat absurd speculations, such as imagining Mexico or Argentina or "perhaps even Canada" as nuclear powers, thus completely ignoring the particular characteristics of each of these countries which make the notion grotesque. Rather than hard evidence Mr. Geiger often offers highly impressionistic statements; for example he writes that "the majority of Europeans are aware that . . . " and refers to "the American attitude toward Europe" (pages 172 and 72). His treatment of NATO concentrates on its military aspects, without attention to the role of the North Atlantic Council in promoting exchanges of foreign policy views and in seeking coordinated stands on such common concerns as arms control, a subject he omits.

Mr. Geiger explicitly denies any immediate policy implications for this work, although he was in the government service for many years. Whatever its shortcomings in the field of politics, however, this book could serve other policy analysts as it probably served the writer, by providing an historically based philosophical foundation for their choices. It should certainly make its readers more conscious of deeply rooted tendencies in Western society. They had better not rely on the index, which has many remarkable omissions.

ANNETTE BAKER FOX
Columbia University

Social Science Literature: A Bibliography for International Law. By Wesley L. Gould and Michael Barkun. (Princeton, N.J.: Princeton University Press, 1972. Pp. xiii, 641. Index of authors. $15.00 hardbound, photo-offset.)

The purpose of this annotated bibliography of close to 3,000 items is to make it easier for students of international law and practicing international lawyers to uncover and make use of social science writings that might be relevant to the international law field. A related and loftier goal is to help bridge the disciplinary gap between lawyers and social scientists.

The book is not organized along lines readily familiar to international lawyers, but rather the organization reflects social-science methodological concerns that, in the authors' view, would be useful to students of international law. The latter would have to be somewhat behaviorally sophisticated to look up entries under "system mapping" or "factor analysis," to mention two of them, but then perhaps the authors are wise in assuming that any international lawyer who buys this kind of book will already have some of this sophistication. Even so, to be on the safe side, the authors have included more of the traditional categories of international law (such as "Crimes," "Treaties," "Aliens," "Air Space") as subheads, and these categorizations are usually accompanied by very helpful cross-references to items listed in other categories.

Naturally any reader or reviewer could quarrel with the selection of entries, but in the main one must acknowledge that the authors have done a yeoman job and have produced a very valuable reference work. The selection of entries seems best in the area of political science, and perhaps poorest in social psychology; in the latter, entries tend to be chosen, it seems, on the basis of titles rather than substance, since a number of significant articles and books on areas that have nothing superficially to do with international law or politics are missing. But then, to include everything relevant would be to require theories of relevance that would probably engage the authors full-time and leave them no time to compile a bibliography such as this one.

This book and a companion volume by the same authors (*International Law and the Social Sciences*) grew out of panel discussions sponsored by the American Society of International Law, and that Society was helpful in getting the ven-

tures started. This leads me to an observation that may not accord with prevailing establishment views. I have found that international lawyers and students of international law have been trying very hard to "plug in" to behavioral sciences; they recognize as relevant and important the findings of social scientists and usually assume a continuing self-educational responsibility in reading and studying the behavioral contributions that might be relevant to international law. They initiate studies such as the book under review, and they always acknowledge the relevance and importance of behavioral science. But what about the other side of the coin? Do behavioral scientists bother to find out what international lawyers are talking about? Do they take the time and trouble to learn about the language of law? Do they care about the kinds of questions raised by international lawyers? Here, with few exceptions, I have found an amazing provincialism. Behavioral scientists seem to assume that there is absolutely nothing about "law" that they need to know about in order to plunge right in with their acquired methodologies and proceed to describe what is relevant and important for international law. (To be sure, matters were even worse in the past: Hans Morgenthau started everything by saying that international law did not exist, and it has taken political scientists quite a while to overcome that false start.) Maybe what we need is another couple of books by Gould and Barkun on international law for social scientists.

ANTHONY D'AMATO
Northwestern University School of Law

Japan and East Asia: The New International Order. By Donald C. Hellmann. (New York: Praeger Publishers, 1972. Pp. 243. $8.00.)

"Japan must play a central and vastly expanded role in East Asia." This opening sentence of Professor Hellmann's analytical study of Japanese foreign policy is currently an almost universally voiced theme. As Japan continues to build up her position as a global economic power, questions concerning political and strategic aspects of her foreign policy become more insistent. Japanese reluctance to play a more active international role has seemed increasingly out of keeping with the potential influence afforded by her economic strength. How does one account for "low posture" diplomacy? Is it a stance which can be continued? If change is to occur, whence comes the impetus for it? What form and direction is the change likely to take? These are some of the questions with which Professor Hellmann is concerned.

It is one of his major contentions that Japanese foreign policy is essentially reactive, the product of external forces rather than internal initiatives. There are both external and internal reasons for this pattern. Externally, the nature of the postwar international order and of Japan's particular relationship with the United States made virtually inevitable the foreign policy adopted. Internally, the nature of the decision-making process, the style of leadership, and the partisanship surrounding foreign policy questions have produced a tendency to "immobilism." Professor Hellmann sees little prospect of significant change internally (reviving nationalism is of secondary significance), but he perceives in global and regional forces the agents of change. As the international order shifts from bipolarity to multipolarity, as China's nuclear force increases, and particularly, as American policy moves toward reduced commitment in Asia, Japan is being brought face to face with critical policy decisions.

In East Asia, Professor Hellmann argues, shifts in the power positions of the superpowers and the increased power of China and Japan are producing changes in the international order. Since the outlook for peace and stability within the region is poor, Japanese policy will have to come out of its "pacifist cocoon" (p. 140). Japan will need to act on the principles of *realpolitik* rather than on those of a "trading company" (p. 62). An active and influential role in the region will demand a level of military power beyond the current level of rearmament, which is determined by budgetary rather than strategic considerations. Since nuclear power is essential to such a role, Japan's ultimate decision on nuclear weapons will be derived from her decision on whether or not to play an active role. The author rejects the view that the development of a Chinese nuclear force will have little effect on Japan's decision. International competition between China and Japan is one of the chief characteristics of the emerging East Asian "subsystem." Japan will, therefore, keep its options open and the definite implication of Hellmann's view would be that of "going nuclear." He warns, for example, that the American stance on the Non-Proliferation Treaty could produce an issue leading Japan to break with American policy.

The American security tie, says Hellmann, has been the single most important factor shaping Japanese policy and has made quiescent diplomacy possible. Now, however, the end of blanket containment and the search for détente have changed the nature and scope of the American commitment in Asia. This raises questions about the continued commitment to Japan, for points of divergent interest are likely to proliferate, abetted by Japanese nationalism, which has a distinctly anti-American flavor. The need for Japan to develop "clearly formulated strategic objectives" (p. 151) becomes more urgent.

It is Hellmann's contention that such clearly formulated objectives have been lacking in Japan's economic relations with East Asia (defined as China-Taiwan, Korea, Japan, and Southeast Asia). Her preeminent economic position in the region in which she is the first or second trading partner of virtually every other state, forms the major part of the "entangling web" between her and the rest of the region (cf. Chapter 5). Foreign aid and investment, while comparatively small in size, are region-centered. The critical point, however, is that all Japan's economic policies have been without comprehensive policy design. They reflect the "rationale of a trading house" (p. 99). In the author's view, politics and economics are not ultimately separable, and Japanese foreign policy must move beyond its narrow economic orientation. While economic factors will not dictate Japanese policy in the region, they will exert a powerful influence. The "dynamics of East Asian politics" will ultimately determine whether Japan will "join the dance" of international politics (pp. 81–82).

The argument of the book is persuasively presented; the style is attractive, and Hellmann has a fine gift for phrasemaking. It is a book bound to raise controversy. Critics of balance-of-power politics will reject the basic premise. Sympathizers with the political opposition in Japan will have their hackles raised by some critical comments. Many would challenge the assertion that China has replaced the Soviet Union as Japan's greatest security threat. Is the threat of a Chinese nuclear force and of Chinese competition in the region viewed as seriously in Tokyo as the book indicates? In spite of seeming inertia, are there not instances of initiative? Incrementalism, which the author correctly sees as one characteristic of Japanese policy, can involve originality as well as reactivism. I would question the Asia-centeredness implied in this analysis. Granted that Asia will grow in importance to Japan economically and otherwise, it is difficult to see her relations with the advanced nations becoming relatively less important. Already more than one-half of Japanese imported raw materials come from advanced countries; and the problems of oil and sea lanes can hardly be met on regional terms. Professor Hellmann admits that Southeast Asia does not occupy a high priority in Japanese policy or political consciousness. For Japan, the vital areas are China and Korea. Does a regional subsystem remain a very meainngful concept for Japanese foreign policy? The case for regional identity is, at best, thin, and whatever the "identity" which modern Japan finally achieves, it will inevitably transcend regional terms.

To pose these questions is to indicate that Professor Hellmann has produced a stimulating book which confronts the critical issues. For idealists and realists, for generalists and specialists, this is an important work worthy of careful reading.

WILLARD H. ELSBREE
Ohio University

Security in Europe. By Robert Hunter. (Bloomington: Indiana University Press, 1972. Pp. x, 281. $8.95.)

Dr. Robert Hunter's *Security in Europe* was first published in 1969. This second edition is based on the same chapter headings as the first, which is here updated and somewhat lengthened. Both editions are substantially padded out with appendices consisting of relevant documents, which take up almost a quarter of the book: as such, the present edition is not cheap. On the other hand, its subject, the problem of maintaining a feeling of confidence in the continuance of peace in Europe, is sufficiently weighty. Dr. Hunter begins his book, quite properly, with the question: What is security? (p. 1). But he does not answer it but instead goes on, in a curiously indirect manner, to say that (p. 3) "in post-war Europe the sense that somehow security was absent derived to a great extent from the lack of any clearly defined rules by which states could conduct their relations with one another, secure in the knowledge that everyone knew what he could and could not safely do." But this is putting the cart before the horse. Rules for conducting international relations and the assurance that they will be observed derive just as much, or probably more, from a feeling of security generally prevailing in the international system, as the other way about. If Dr. Hunter had stuck to his original question he might have told us more about the relations between rules and security, or rather security consciousness, for that is what we are really talking about when we discuss security as an international concept.

Instead we go on to "the beginning of the Cold War." Here it is stated that the Cold War was not inevitable and that is certainly a common opinion among contemporary writers "who"—Hunter says—"like me, will only know (the Cold War's) beginning from their elders." Then how did it start? The fact was, we are told, "that the onset of the Cold War really reflected the inability of both Russia and America to find ways either of overcoming their misunderstandings or of deciding where there was scope for bargaining." If Dr. Hunter learned this from his elders he could not have found them very helpful; one would have thought there was more to be said than that.

We are then taken on to consider the Threat (Chapter 2), to which the Western Powers' defensive movements, first decisively expressed in the Atlantic pact of 1949, were a reaction. Dr.

Hunter argues that in Western eyes the Soviet threat was not at first perceived as a military one and that it was only after "the context of military confrontation was established" (p. 39), that is, after the formation of the Brussels treaty in 1948, that Soviet forces in Eastern Europe were seen as a "positive threat" to the West. It is hard to see here how you can have a "context of conflict" between states unless each side, or at least one, perceives the other, whether rightly or not, as constituting a threat of some sort to itself. But it is true, of course, that once you have reason for perceiving the other side as threatful, it is difficult to interpret any of its actions as other than sinister.

Dr. Hunter is on safer ground when he goes on to write about "the role of institutions" in Chapter 3, in which he describes how the very institutional structure of the NATO and Warsaw pact systems contributed both to the rigidity of the Cold War and at the same time to its unloosening. But again Dr. Hunter's form of expression raises questions which plague the reader. "The format of unity in the North Atlantic Council," he contends, "inhibited the making of changes—as when the French, in 1958, proposed that responsibility for general Western interests, not just those within the NATO area, be divided among America, Britain and France" (p. 53). We can just imagine President de Gaulle's reaction if someone had proposed that "general Western interests" be divided among America, Britain and Germany, and if he was told that his reaction simply reflected a "format of unity" in the North Atlantic Council.

The main substance of the book, however, is contained in a long discussion of the East-West détente since 1962 in Chapter 6. It is a pity that there is not much analysis here of how the détente came about: light might then have been thrown on the probable viability and future of the détente, But there is quite a subtle and well-informed consideration of the many issues raised by détente. Some of Dr. Hunter's suggestions, as for instance for a European Security Commission (already proposed by the Russians), may strike some readers as evoking visions of one more lifeless East-West organisation. But the general tone is sensible and at times penetrating. Oddly enough, Dr. Hunter ends his book (p. 223) with a reassertion of the status-quo. "This system" (that is, the self-confronting system of NATO and the Warsaw pact) "has its tensions and problems, but to abandon it without finding something substantially better to put in its place might itself be the surest means of denying Europeans their present hopes." And it cannot be said that Dr. Hunter has invented anything substantially better.

This is a thoughtful book which raises questions as much about itself as about its subject. Because the standards of analysis it sets are high, it demands equally high standards of evaluation. Dr. Hunter has certainly shown that his subject, often used for the purpose of making glib or trendy judgments, demands a concentration of mental effort to master it. It is a pity that so often Dr. Hunter's own efforts are open to criticism.

F. S. NORTHEDGE
London School of Economics and Political Science

International Relations and World Images: A Study of Norwegian Foreign Policy Elites. By Helge Hveem. (Oslo: Universitetsforlaget, 1972. Pp. 336. $12.50.)

Is a nation's foreign policy based on elite perceptions of its role in the international system? This is the question which Helge Hveem raised in his thesis at the University of Oslo, further elaborated at the Oslo Peace Research Institute, and now presents in book form. His data are generated from a 125-item questionnaire presented to representatives of four elite groups in Norway—politicians, bureaucrats, interest groups, and people from the mass media. The purpose of the study is to see how the foreign-policy elite perceives the world and the nation's place in it. In a concluding chapter, he uses his interview data for observations and predictions about discrepancies between the perceived world of the elite and the "real world" and the probable consequences for future Norwegian foreign-policy making.

Considering that Hveem enters fresh into a very crowded field where foreign policy analysts for years have been chasing the key variables and arguing over their relevance, the study has many merits. It is technically well done and properly organized, most major propositions are illustrated by tables, there are some useful appendices, and the author shows a reasonable awareness of existing literature in the field. Apart from some occasional jabs at research activities that are not "peace-research oriented," the argumentation is on the whole sober and levelheaded. Hveem's distinctions among what he calls elite groups, opinion makers, and public opinion is done with understanding of the complexity of the problems of foreign-policy decision making. The book suffers from all-too-common tendency to draw too wide and sweeping conclusions from a single case. More serious doubts, however, appear when one looks more closely at the central issues in the book—the impact of elite images on foreign policy making, and the extent to which such perceptions can be substantiated by interviewing some one hundred members of a country's national elite.

To take the last question first, whose images

did the respondents actually reflect? Did they respond as independent individuals or do they reflect the national system? Hveem has clearly assumed that the interviewees would respond independently of the organizations to which they belonged. Therefore, he seems surprised to find a majority of the respondents reflecting the country's official foreign policy. He also makes a point of the very high degree of elite consensus on major policy issues. A further major observation is how the degree of consensus increases with increased social position. Far from being surprising, this reaction within the elite is what one would normally expect, particularly in a small countrry like Norway. Consensus in the author's terms really means conformity. The elite groups are relatively small and keep a close watch on each other. Most of them have arrived at elite positions by accepting the nation's official policy. The highest respondent scores appear on the questions where the interviewees have a neutral option, where they can avoid taking a clear stand or position on a certain issue, and there is less risk of sticking one's neck out. The respondents knew which were the "right" positions to take in terms of official Norwegian foreign policy, and most of them reacted correspondingly. One cannot avoid some doubts about conclusions drawn on the basis of the images which these answers reflect.

One should also be aware of another modifying factor: "interview fatigue." In some countries (Norway is one of them), social scientists have conducted survey research on elites for more than fifteen years. Because these groups are fairly small, the same persons have been exposed to questionnaires over and over again, in some cases several times a year. They know the interview techniques and also the attention that some of the resulting publications may get. This seems bound to affect their responses and "perceptions." Their reactions may vary. Some respondents welcome interviews as a break in an often drab bureaucratic routine and take great delight in adopting exotic and eccentric positions to test the reaction of the interviewer (often quoted among amused friends at subsequent parties). In such cases, which are no longer rare, one may be measuring moods and whims, rather than perceptions. Other respondents, probably a majority, are more concerned with possible consequeneces and take a vague, cautious approach, sticking close to the dotted line of official policy and qualifying their answers where there are no accepted, known guidelines. Given this background of consensus/conformity, one cannot really know whose images are being recorded in the survey.

Another, more fundamental question concerns the relevance of elite perceptions for the making of a nation's foreign policy. Hveem's major thesis is that the images which a nation's elite has formed of the international system will have a decisive influence on its foreign policy. It may even have a predictive dimension. If one knew the images of the relevant elites, one could then forecast the directions of its foreign policy. Suppose that it was possible to obtain a set of "correct" elite images, is really the relationship between what elite groups think of the world and what their nation will do sufficiently established to warrant predictions!

These two questions which actually form the core of the study are not satisfactorily resolved. Hveem's study implies that the elite national decision makers are mainly concerned about the place and ranking of their nation in the international system. He assumes that they will define their nation's role in systemic terms. Some of the one hundred respondents might do so, but it seems highly improbable that more than a scattered few of these hard-pressed politicians journalists and bureaucrats would give much thought to such abstractions as the international system. Further, it is even more improbable that such perceptions as might exist about Norway's place on the international ranking lists and its impact on the system should be a major factor in its day-to-day foreign policy decisions.

The world looks different when seen from a very small and a very large state. Small states do not generally see the working of the international system as their national responsibility. Their verbal activity on the global level is often more a reflection of domestic considerations than a preparation for national foreign policy actions. Instead of asking members of the elite what they *think* should be done on the world scene, Hveem might have asked them what their nation was prepared to do to make it happen.

Images versus reality is in fact another way of spelling out the old dichotomy between declaratory and action policies. If the goal of the exercise was to establish a nation's declaratory policy, one might suggest other and more direct approaches than by interviewing a sample of its foreign policy elite. A major goal in foreign policy analysis is to reduce the subjective element to a minimum and increase the amount of hard data, including such observable and measurable examples as treaties, agreements, organizational affiliations and behavior, public statements, and domestic political constellations. Until such sources are exhausted, it would seem a detour to use elite perceptions to establish what might be a nation's declaratory policy. Recording thoughts and beliefs on world affairs may be interesting and worthwhile as an intellectual exercise, but Hveem has not shown that there is a close relationship between confessed elite perceptions of international relations and the nation's foreign policy

as it actually is carried out. This might be kept in mind as a possible and more rewarding second stage of the study.

NILS ØRVIK
Queen's University, Ontario

China and the West: Mankind Evolving. By Robert Jungk, Ernan McMullin, Joseph Needham, Joan Robinson, Stuart Schram, William Sewell, and Bernard Towers. (New York: Humanities Press, Inc., 1971. Pp. xvii, 144. $5.50.)

China and the West: Mankind Evolving is the fifth volume in the Teilhard Study Library dedicated to the memory of the Jesuit paleontologist and thinker Pierre Teilhard de Chardin who died in 1955. Father Teilhard was in the same tradition as that group of worldly-wise Jesuits who went to China in the last decades of the Ming dynasty to proselytize but stayed on to write a splendid page in the history of cultural relations between East and West. Teilhard carried on much of his professional research in China, and the same breadth of vision that characterized his predecessors can readily be seen in his observations on communism in China. In 1947, at a time when the Vatican felt besieged and persecuted by what was then regarded as a monolithic anti-Christ, world communism, we find Teilhard speaking of communism in China as "that extraordinary and irresistible movement which, suddenly turning the old, traditional confucianist zest *for* man into a wholly modern faith *in* man, makes of the least Chinese peasant a proselyte zealous to the point of death" (p. xv). This faith in man, he wrote to a friend two years later, "could only be matched or surpassed by a Christianity where faith in God has incorporated the new human faith in the early future of Humanity. . . . " Regrettably, Teilhard observed, Rome did not yet see that " 'faith in man' is a distinctly new element in the human soul" (pp. xv–xvi).

Although only one of the seven contributors to *China and the West* (Ernan McMullin, "Teilhard, China and Neo-Marxism") focuses directly on Teilhard, all of the essays "explore the theory of convergence in human evolution" (p. xvii) which permeated the writings of Teilhard. Thus, the reader who imagines that the title of the book promises discussions of such topics as the Sino-Soviet conflict or the waxing and waning of Sino-American relations will be disappointed. The book is more about "mankind evolving" than it is about "China and the West" in the sense that that phrase ordinarily suggests. Indeed, two of the contributions and the appendix do not relate to China at all.

Joseph Needham, however, needs no introduction to China specialists. His massive multivolume study *Science and Civilization in China* is one of the most ambitious and valuable scholarly projects undertaken in the field of Sinology in this century. Presented in the book under review is a highly condensed (16 pages) summary of some of the themes which run through his *Science and Civilization in China*—for example that Chinese scientific accomplishments far outshone those of the West for at least a thousand years and that Europe was discovered by the Chinese rather than the other way around. But there are also some interesting reflections on the Cultural Revolution which Professor Needham believes is the "descendant of some of the ethical insights of Confucianism and Taoism" (p. 26). He traces, for example, the Maoist command to "fight self-interest and struggle against privilege" to the Taoist insistence that the Sage must make himself lower than the people so that, whether he leads or follows the people, the people will feel no offense.

Stuart Schram in his "Social Revolution and Cultural Revolution in China" presents here little that is not more fully set forth in his *Mao Tse-tung* (1966) and *The Political Thought of Mao Tse-tung* (1969). It is nonetheless a thought-provoking essay in which Schram gets to the heart of Mao's contribution to Marxism—the notion, born in the grim siege years of Chingkangshan in the late 1920s, that "by changing people's mentality through indoctrination and study, one could also change objective social reality" (p. 66). The test of the axiom came in the early land reform attempts of the 1930s in Kiangsi when Mao discovered that mere changes in the formal structure of property control in the countryside would not bring about lasting change. Power inevitably gravitated back into the hands of the old privileged classes. The only way that this reversion could be prevented was to direct efforts not so much at the social structure but at the minds of the poor peasants in order, as Schram writes, "to develop their self-confidence and allow the conviction to take root that henceforth they were the masters of society" (p. 67).

This goes far towards explaining the meaning of the Cultural Revolution which, as Schram views it, was brought about to insure that positions of authority are to be allocated to individuals delegated by the masses. "[T]he whole point of the Cultural Revolution," he writes, "is that neither the technical skills of the engineers, nor the political skills of the Party and government cadres, shall ever give them a permanently privileged status in society" (p. 77).

Essays on "Society and Economics in China Today" by Joan Robinson and "Religion in China Today" by William Sewell round out the contributions directly related to China. Also included in this volume is "Evolution and Revolu-

tion in the West", by Robert Jungk, and "Mankind Evolving" by Bernard Towers, the General Editor of the Teilhard Study Library. While I have concentrated on only two of the essays for reasons of space, all of the essays in this book are of uniformly high quality and make a distinguished addition to the Teilhard Study Library.

JOHN H. BOYLE
California State University, Chico

Soviet Relations with India and Pakistan. By Davendra Kaushik. (New York: Barnes and Noble, Inc., 1972. Pp. 119. $5.00.)

In 1920 a member of the Soviet Commissariat of Nationalities suggested that a quick assault upon India by Soviet forces might Bolshevize that country. Had Davendra Kaushik dealt with that incident he would have relegated it to the collection of "misunderstandings" which litters his history of Soviet relations with the Indian subcontinent. For Dr. Kaushik, of the Department of History, Kurukshetra University, has attempted to prove that "right from the very beginning, Soviet policy towards India and Pakistan has been consistently aimed at strengthening friendship with both, while trying to bring them closer together" (p. 111). The value of this book lies in the argument it presents to uphold such an erroneous thesis: an argument which has gained currency among both Soviet and Indian defenders of the increasingly close relations between the two states.

The problem for Kaushik is to align the Indian version of Indo-Soviet relations with current official Soviet historiography. The "ultra-leftism" of the 1928–34 and 1948–50 periods has to be explained and criticized without damaging the official understanding of revolutionary Leninism. Similarly, the overtures towards Nehru's government made by Khrushchev and his successors must be justified according to the Leninist principles. Kaushik jumps in at the deep end. The change of line at the Sixth Congress of the Comintern is considered to be "unfortunate," its overthrow in 1935 was "not effected by a self-critical evaluation of the negative course . . . which allowed an easy relapse into the anti-Leninist ideological posture after the end of the World War II . . . . It was only much later, in 1956 at the Twentieth Congress of the CPSU . . . that the correct Leninist ideological stand towards national revolutions was restored" (pp. 17–18). The "correct Leninist" stand is here seen as a simple distinction between "the socialist movement of capitalist countries and the national movement of the colonial countries" (p. 14), a distinction, of course, which should be more justly attributed to Stalin.

Having constructed the outline of his mythology, Kaushik conceals its weaknesses with a gallery of characters who have either promoted or jeopardized Indo-Soviet relations. Lenin, Gandhi, Nehru, Khrushchev, Brezhnev and Kosygin belong to the first category; Roy, the early Dyakov and Balabushevich, the Chinese leadership, and all Pakistani governments are relegated to the latter. The only contention with the official Soviet history is over precisely what opened the way to mutual understanding: Nehru's nonalignment policy or the adoption of the "correct Leninist" line by Khrushchev in the 1950's. Kaushik tends to the view that with India's demonstration of nonalignment the "earlier Soviet mistrust of its intentions began to give way" (p. 54). The ensuing friendship survived despite the "severe strain" (p. 63) imposed by the Soviet intervention in Hungary and the "near storm" (p. 73) caused by Pravda's famous editorial during the 1962 Sino-Indian border conflict.

Kaushik echoes the diplomatic justifications for the relationship, quoting Nehru: "Let our coming together be because we like each other and we wish to cooperate and not because we dislike others or wish to do them injury" (p. 60). Great stress, however, is laid upon Soviet support for the liberation of Goa and for India's case against the Kashmir plebiscite. The MiGs delivered in 1963 were, according to Kaushik, "to be used for India's defence against China" (p. 74). Indeed, the focus of the book revolves around the 1965 war and consequent Tashkent peace, in which the author appears to relish the contradiction between descriptions of Soviet support for the case against the plebiscite, Soviet military supplies to India, combined with Kosygin's diplomacy at Tashkent, and yet asserting that "it was not its [the Soviet Union's] fault if Pakistan did not clasp the proffered hand of friendship, which India not only grasped but sought" (p. 83). Pages are devoted to Indian and Soviet statements celebrating the wisdom of the Tashkent Declaration, while it is noted that "a similar response to the declaration from the Pakistani leaders has unfortunately been wanting" (p. 99).

One purpose of the book is to combat the assertion by some Indian scholars (such as Harish Kapur) that the Soviet policy of scrupulously building equidistant relations with India and Pakistan by the Brezhnev and Kosygin team must imply a partial withdrawal from India, and from India's conception of South Asian security. Kaushik argues that the Soviet Union has always attempted to have good relations with both India and Pakistan and that only the reluctance of the latter has caused the consequent imbalance. Today, with the benefit of hindsight, Indian fears in this respect can be seen to have been effectively eradicated. "Good neighbourly relations between

India and Pakistan," concludes Kaushik, "are viewed by the Soviet Union as the sheet anchor of Asian security" (p. 111). The changed situation, however, in which Moscow's new "sheet anchor" is a militarily assisted and strategically powerful India, is unlikely to disturb Kaushik's mythologically conceived history of their relations. Indian scholarship on this question is rapidly becoming a mirror of its Soviet counterpart; a reflection of contemporary political and historical requirements rather than an objective inquiry into the purpose of such a relationship. But, as with Soviet work, it is no less valuable for the light it sheds upon the current justifications, motives, and effects of the relationship between the Soviet Union and its largest non-Communist arms recipient.

PAUL GERHARDT

Oxford University

The United States, Communism, and the Emergent World. By Bernard P. Kiernan. (Bloomington: Indiana University Press, 1972. Pp. 248. $7.95.)

In a perceptive and cogently argued essay Professor Bernard Kiernan outlines what he considers to be the major errors underlying U.S. policy in the emergent world of Africa, Asia, and Latin America. Most important, according to Keirnan, is the tendency of Americans, including those in decision-making positions, to view revolutionary political activity in these areas as part of a worldwide Communist conspiracy directed by Moscow or Peking or both. Rather, according to Kiernan, Communism and other forms of revolutionary political activity should be seen as national responses to the myriad problems of economic and social development in backward societies.

The United States' effort to suppress this revolutionary process is, according to Kiernan, doomed to failure. In fact, the U.S. policy of containment which has resulted in various types of American intervention in the emergent world has actually pushed national leaders desirous of creating truly independent political-economic systems in the direction of revolutionary solutions and contact with the Communist world. The only alternative, Kiernan argues, to a continuation of a U.S. policy of intervention based upon the assumptions of containing communism can be found in an effort on the part of Americans to learn to live with the revolutionary politics of the emergent world. Such a revolution is contrary to U.S. national interests only when they are defined at the expense of other peoples.

Most of Kiernan's study is concerned with supporting his major argument. Although he does not provide major new insights in the chapters containing the supporting evidence and interpretations, Kiernan has produced a concise overview of the literature on the attraction of "communism" as a development strategy, the problems facing those countries that are developing in the late twentieth century, and the failure of the U.S. policy of containment of communism in the nonindustrialized world.

Kiernan begins his analysis with a survey of the development process in the West during the past several centuries. The most important part of his discussion of this subject, at least from the perspective of his major argument, concerns the undemocratic nature of the modernization process. Fortunately for the Western countries, however, several of the major tasks of modernization—in particular, state building—had already been accomplished before the process of industrialization was initiated. One of the major results of socio-economic-political modernization, as Kiernan argues, has been the major expansion of the role of the state in all aspects of human existence—in other words, a reduction of the independence of the individual vis-à-vis the polity at the very time that new organizational and technological developments have decreased man's dependence on the vagaries of nature.

The major goal of nationalist leaders in the emergent world is the acquisistion of the power to determine their own existence independent of of outside control. This requires the rapid creation of "modern" societies by telescoping the political, economic, and cultural changes that occurred over centuries in the West into a generation. It is here, according to Kiernan, that the Communist model of modernization asserts its attraction on these nationalist leaders. In addition, he maintains, since the United States has defined its policies toward the emergent world in terms of the containment of Communism and has viewed virtually any revolutionary movement as inimical to American interests, U.S. policy assists in pushing nationalists toward Communist alternatives.

On the whole, Kiernan's argument is well developed. His essay does suffer, however, from several rather serious flaws. Most important, to my mind, is his effort to categorize all revolutionary movements in the developing world as "communist," defined as an authoritarian movement, committed to economic development and social revolution pushed through from above, and inherently anti-Western (pp. 14–15). This definition of "communism," as Kiernan himself notes, can be applied both to formal Communist parties and to a wide range of other revolutionary movements. I do not believe that this conscious effort on the part of Kiernan to include all revolutionary groups in a single category facilitates our understanding of the revolutionary process. For example, he ignores the difference between those

"revolutionaries" who have allied themselves with the Soviet Union and, in effect, taken orders from the Soviets, and others who have taken an anti-Soviet as well as anti-Western position. He includes Latin American Communists, who have played down revolution, with revolutionaries like Colonel Khaddafi of Libya who have opposed Communist solutions to socioeconomic modernization. Kiernan's approach makes it extremely difficult to understand the reasons for the attempted coup by the Communist Party in Sudan against the government of Numiery which fits Kiernan's definition of "communism." Kiernan himself appears to be aware of this problem, for on several occasions he does distinguish between "left-wing radicalism" and "Communism" (pp. 150, 151).

In addition, Kiernan's interpretation of future developments in the emerging world is too deterministic. For him the possibility of any but an authoritarian, "communist" approach appears to be ruled out and, in order to maintain this argument, he is required to include all countries in the same basic category of authoritarian/totalitarian without taking into consideration their major differences. The possibility that some countries (especially those that are blessed with more abundant resources) may not select the revolutionary approach to modernization is also downplayed.

Finally, the evidence is not clear that U.S. foreign aid has been used, as Kiernan argues, "primarily as a means of maintaining America's informal overseas economic empire" (p. 204). A recent study of U.S. economic aid by Eugene Wittkopf, for example, indicated that U.S. economic aid correlated highly with geographical proximity of the recipient to the Soviet Union and China and was thus directly related to a policy of containment rather than to U.S. business interests. This conclusion fits Kiernan's basic view more closely than does the claim which he actually makes.

These weaknesses in Kiernan's essay appear to be related to Kiernan's effort to present a comprehensive picture of the emerging world in which there are no "loose ends." Still, *The United States, Communism, and the Emergent World* does present an analysis of developments in the Third World and an assessment of U.S. policy there that is generally valid, even though a few countries do not fit neatly into the general model which is presented.

ROGER E. KANET
University of Illinois, Urbana

Europe in the Twentieth Century. By George Lichtheim. (New York, Praeger Publishers, 1972, Pp. 372. $12.95.)

The late George Lichtheim's book is concerned with the transformation of European society in our century. The text attempts to summarize political, social, and economic history: the first part deals with the years 1900 to 1914, as the background of what is to come; the second discusses the First World War and the interwar years, while the last section of the book takes the story from the Second World War to the sixties. For Lichtheim, Europe is a unity which excludes both the USSR and the United States. This is no properly balanced text, however, but a *tour d'horizon* with one of the most powerful minds of our time, whose untimely death has robbed us of the kind of stimulation which this book so amply provides.

The last chapter of the book is called the "post European age," but it is typical of Lichtheim's turn of mind that he ends this discussion and the book by asserting that European influence might remain powerful in the realm of thought—provided it was based on the principles of theoretical reason as exemplified by the tradition of Kant, Hegel and Marx. This was the tradition within which Lichtheim moved, it set the standard for his judgments; "western standards" as he calls them here (p. 35). Compared with such standards, philosophies which differ such as those of Soviet Marxism or fascism are designated as "philosophical nullities" (p. 213), and the book refuses to linger long over such nonsense. The right kind of philosophy counts, for modern society is dependent upon critical assessment and this in turn must rely upon the human mind.

The dynamic of the twentieth century is provided by Marxism, a very proper viewpoint for the leading Marxist scholar of his generation. As a result, however, movements like fascism are defined in opposition to the left, they emerge as "negative" factors, though Lichtheim recognizes the contribution which fascism made to modernization. Still, this movement was retrograde, "drawn from the sweepings of the gutter" (p. 61), and therefore could not be understood on its own terms. All the fascists did was to take over and caricature the Communist example. Lichtheim was well aware that modern scholarship would dispute such statements, but fascism departed furthest from theoretical reason, and that was enough to characterize it. His judgments upon leading Marxist theoreticians also applied such standards. It seems bizarre to give pride of place to Antonio Gramsci and hardly any consideration to George Lukacs because he "became mired in the Stalinist bog" (p. 144). But for Lichtheim Lukacs abandoned the proper standards, while Gramsci in theory at least, returned to the original Marxist and Hegelian inspiration (whatever mistakes he made in practice).

George Lichtheim had little use for the argument that Lukacs went along with the Com-

munist party line so as not to lose contact with the working classes. The people do not enter this book, nor does popular art or literature. Ideas interested him, and when the people do appear they are motivated by pressure rather then by ideology. Religion, myth and symbol, the motivating factors of twentieth century popular movements are outside Lichtheim's concern, and he seems to despair when he has to explain their triumph (as in fascism). This book is a history of intellectual movements and ideas. Thus; the stress on philosophies like logical positivism and, after 1950, upon a revival of Vico's philosophy of history which, I fear, never really took place at all.

But such weaknesses are also real strengths. At a time when so many scholars migrate into the market place Lichtheim refused to follow. What he himself found interesting became determinant in his historical analysis, if always within a Hegelian, dialectical framework, It does not really matter that at times some judgments are impossible to accept (thus Nietzsche is, above all, the greatest anti-Semite of all time), they are always relevant and thought-provoking. This is true even for his passionate admiration of de Gaulle who was "anachronistic" (as he writes here p. 35) but who seemed to him nevertheless the last world historical figure. His Hegelianism may have gotten the better of him there, but at the price of ignoring the General's philosophy. George Lichtheim loved, however, to provoke, to enter into an intellectual clinch with his opponents; there was nothing flabby about the man or his mind. He writes in this book about "superannuated liberals" who fought a rear guard action against irrationalism: but he himself typified the best of liberalism without its prevarications.

Lichtheim reminds us that intellectuals are not necessarily defined through opposition to all establishments, but rather through the clarity of their arguments and by the standards they are able to set. These certainly depend upon that theoretical reason which informs this book.

GEORGE L. MOSSE
University of Wisconsin, Madison

International Economic Reform: Collected Papers of Emile Despres. Edited by Gerald M. Meier. (New York: Oxford University Press, 1973. Pp. xvi, 293. $7.50.)

Some twenty years ago, I was telling a friend about a long conference at which Emile Despres had consistently exhibited his mastery of the entire field of economics, his imaginative and constructive substitutes for much of the conventional wisdom, his sensitivity to the political implications of different economic policies, his gift for lucid exposition, and his fine sense of humor. "Yes," said my friend, "Since Emile won't write, he ought to be wired for sound."

This collection of his papers, skillfully selected and grouped by Gerald Meier, is the nearest thing to wiring for sound that we shall ever have. Despres died in the spring of 1973, having made almost no concessions to what he called "my lifelong aversion to writing for print." Nevertheless, his influence—as Professor of Economics at Williams College and Stanford University, as an employee of or consultant to various U.S. Government agencies, as first director of the Pakistan Development Institute, and in other capacities—was great. It is easy to see why.

The papers span three decades, 1938–1968. They consist of memoranda prepared for policy makers here and abroad, reports and recommendations, Congressional testimony, corrected transcripts of lectures, and two or three co-authored pieces written for publication. They deal with fundamental questions in three areas: international trade, international development, and international monetary reform. Remarkably free of jargon, only two out of more than twenty will give any trouble at all to political scientists untrained in economics; indeed Despres' style may come as a welcome relief to the tortured and turgid English of many of their colleagues. See, for example, Despres' 1959 testimony on "The Significance of the European Common Market to the American Economy," or pages 136–38 on import controls and other direct controls in less developed countries, or "Determining the Size of a Development Plan: Pakistan."

Most impressive perhaps are Despres' prescience and originality. He was integrating the problems of the less developed countries into his analyses long before it became fashionable to do so. He was prompt to call attention to the importance of developing agriculture in low-income countries and to the political strains inherent in the development process, including the strains caused by unemployment and by growing income disparities among economic classes and regions within poor countries.

He foresaw accurately the difficulties the European Economic Community would cause and the benefits it would bring. He was not the first to advocate the demonetization of gold, but he was one of its most effective advocates and was recommending the establishment of a two-tier market for gold five years before that policy was adopted.

Monetary economists could illustrate his unconventionality by numerous examples from the papers on monetary reform; I shall use for that purpose two papers on development. In a memorandum to Malaysia's Economic Planning Unit, Despres made what seems to me a convincing

case for ignoring the cautious advice to Malaysia's leaders in a World Bank report and pursuing instead "an aggressive policy . . . with respect to obtaining and utilizing external loans" (p. 165). "Vigorous growth," he said, "attracts its own financing" (p. 169). In a brilliant paper on "Inflation and Development: Brazil," he explains why he believes foreign loan capital is normally a much better buy, from the point of view of a developing country, than foreign direct investment. The paper on Brazil, however, is most important for its analysis of why stopping inflation is the wrong immediate goal in the Brazil-type situation (of 1967). The preferable immediate goal is to make the inflation more neutral in its effects by eliminating the major distortions to which it gives rise.

If the book has a theme, it is the need for the world to free itself from the restrictive shackles of some of the most hallowed beliefs of economists so that it can, in good conscience and good cheer, equip itself for renewed, more rapid headway on international trade, international development, and international monetary reform.

ROBERT E. ASHER

Washington, D.C.

The Study of International Affairs: Essays in Honour of Kenneth Younger. Edited by Roger Morgan. (London: Oxford University Press, 1972. Pp. x, 309. $17.00.)

How does one review books "edited by . . . "? Such books have become a problem. In a recent issue of *APSR* one could find, within the confines of a few book review pages, complaints about "unevenness in the quality of contributions," "discontinuity in the themes pursued," "total failure of contributors to respond to one another," "lack of a unifying theme," "no apparent relationship among the articles"; it was "jarring to move from a critique almost directly to a prime example of what had just been debunked"; and so on. The problem is that, wherever some workers in some subvinyard of our profession congregate for some symposium, lecture series, or panel discussion, a volume of "collected essays" is due, conferring immortality in library catalogues upon the ed(s), but not otherwise appreciably promoting that knowledge which anyway tends to become less and less cumulative in our field. Are we then, increasingly, engaging in *Dichtung* rather than in *Wahrheit*?

Noting that the editor of the present volume describes it as a mixture of *Festschrift* (for the former director of Chatham House as well as the Royal Institute's own 50th anniversary) and *Mélanges* (in the French sense), I expected the worst, i.e., a hodgepodge of unintegrated articles. I was agreeably surprised: Most of the contributors (almost all connected with the Institute) had made an effort to adhere to the objective laid down by the editor: to "survey some of the major aspects of international studies at the present time," to deal with "the methods used to study international affairs," and to "place the relevant research activities of Chatham House in the perspective of international studies as a whole" (p. ix).

About half of the articles are area studies. Not surprisingly, they deal with "substantive" questions of the respective regions and countries rather than with the study of their international and foreign affairs. In most, there is at least a critique of the literature. Thus we have an essay on the Commonwealth (Bruce Miller), on Africa (Dennis Austin), on South Asia, primarily India (Hugh Tinker), and on Western Europe (Roy Price; with a brief survey of new methods to study "integration"). Michael Kaser, in a contribution on the Soviet Union and Eastern Europe, offers a mere survey of literature, mostly on domestic affairs. Would that at least bloc relations and bloc "integration" had been dealt with, not to mention the specifically Communist approach to studying international affairs. The one outstanding article in this group is John Gittings's on "Western Scholarship and the Cold War in Asia." Frankly revisionist, it undertakes to refute the "reactive myth" of a purely defensive U.S. policy in postwar Asia. I would say that, because of their mutual "security dilemma," both sides (as is generally the case in a cold war constellation) were acting in an "aggressively defensive" way, that is, subjectively, defending themselves against an opponent *viewed* as aggressive. But whether cold-war traditionalist, revisionist, or what not, the historian of the cold war will have to take notice of Gittings's arguments and the sources he meticulously assembles. Even this splendid piece, however, pays little attention to *how* to study a foreign policy (e.g., the new approaches by Allison et al. to foreign-policy decision making).

In-between the area studies and the methodological pieces there are some that straddle the approaches. D. C. Watt, writing about one of the chief glories among the Institute's products, the *Survey of International Affairs*, probes problems and pitfalls in the study of contemporary history, as illustrated by the variegated history of the *Survey* itself, with an outlook on how the "generalist" historian may cope with the problem of covering an ever more extensive and complex flow of data. Two articles by economists—Susan Strange and Caroline Miles—fall likewise in the in-between category. Strange issues a clarion-call for overcoming disciplinary divisions, calling specifically for a study of the "politics of inter-

national economics" and of the "economics of international politics." Miles, among others, calls for a new approach to the study of multinational corporations. One misses something on the neo-Marxist approach and theories of "economic imperialism." Two more articles deal with human rights and international law, respectively. J. E. S. Fawcett's on "Human Rights in International Relations" is a hodgepodge of random issues (dealing, in addition to human rights, with "aggression in Vietnam," "self-determination" in Rhodesia, etc.). Rosalyn Higgins's on "International Law and the UN System," on the other hand, is an admirable, concise yet profound probing of the place, role, and problems of law in present international relations and organization. Even this author touches the problems of study and method only incidentally. Yet this piece alone is (in Justice Holmes's words) "worth the price of admission."

Finally, the introductory and concluding methodological articles, by Andrew Shonfield and Roger Morgan, respectively: Shonfield emphasizes the importance of overcoming preconceived "explanatory models" in favor of agreed-upon methods and images. It is the business of scholars "to provide models of the international system which can be used by practical men of affairs to interpret new events in a way which assists them to make rational decisions" (p. 12). But how to arrive at such uncontested and undistorted "models"? Morgan finally arrives at what one had been waiting for all along: What about the new methods developed by the (primarily American) "quantifiers" and "behavioralists," the systems analysts, the students of decision making, etc.? Morgan perceives that there has already been a recession of the "new scientism," and a recent modesty in the claims of its protagonists. He stresses the importance of strategic studies for students of an international system vitally affected by the nuclear factor. He comes out for the development of "futurological" methods and models if the study of international politics is to remain "relevant to the real world" (p. 290). To this the reviewer can only say "Amen," but the question is, Can we agree on what is "relevant"? I have had my say on this (in *Polity*, Autumn 1971). Nothing, in my opinion, is truly relevant in international studies any more unless it is related to the questions of physical survival of mankind, i.e., the dangers of over-population, exhaustion of resources, and destruction of the global environment. Richard Falk has remarked: "Henry Kissinger, Hans Morgenthau, or George Ball will write at length about alliance patterns, bipolar relations, the strategic arms race, but will not devote a single word to the ecological fragility of the world" (*This Endangered Planet*, p. 181). Here is the challenge, to Chatham House as to all of us.

JOHN H. HERZ

City College, CUNY

Strategic Forces: Issues for the Mid-Seventies. By Alton H. Quanbeck and Barry M. Blechman. (Washington, D.C.: The Brookings Institution, 1973. Pp. 94. $1.95, paper.)

U.S. strategic policy has been the subject of more extensive analysis and debate than all other aspects of our national security policy combined. For more than ten years, deterrence of nuclear attack through assured destruction has been the overriding strategic policy goal, but now this relatively well-defined objective may be fading as the administration and its military planners seek to acquire an increased flexibility to fight nuclear wars. Like a kaleidoscope, flexibility can mean many things to many people; it can be an open-ended invitation to procure all manner of new weapons and devise ingenious methods for their employment.

Quanbeck and Blechman, in their booklet published in the spring of 1973 by the Brookings Institution, have made an important contribution to the study of nuclear strategy at a time when a policy shift is raising many questions. They have not only pulled together in concise and clear form authoritative basic information on the strategic nuclear forces of the U.S. the U.S.S.R. and China, but, more important, they have put in perspective the political factors involved in force planning. In their analyses they add to the expenditures necessary for procuring the weapons themselves the prorated cost of operation and maintenance and extrapolate these costs to the end of the decade, thus providing a realistic picture of that fraction of our total military effort being devoted to strategic forces.

The book presents a series of alternative "higher and lower cost" aggregate strategic force postures, the "higher cost" options being only slightly above the FY74 program. This is not surprising since the administration was already pushing in 1973 for vastly increased strategic weapons procurement, and now the FY 75 Defense Department proposals apparently even surpass this "higher cost" option. Even the "lower cost" options put forth by Quanbeck and Blechman do not involve radical reductions in our strategic programs. The most extreme option, but one that is probably not politically viable, calls for phasing out land-based ICBMs because they may become vulnerable in the 1980s and for relying entirely on submarine missiles and bombers for deterrence.

After developing the factual base, the authors analyze the military, political, and economic

issues presented by alternative programs for individual weapons systems. The potential vulnerability of the Minuteman ICBM force is the most critical problem facing strategic planners and affects decisions not only on ICBMs but on other strategic systems as well. Even the expensive and questionable Trident submarine missile program is related to increased concerns for survivability of the Minuteman ICBMs in the late 1970s. The book discusses the rationale for the acceleration of the Trident, but backs away from drawing any conclusions on whether it is needed or whether another smaller submarine, which is now in the new Defense program, would be better.

This refusal to take positions is an unfortunate characteristic of this Brookings staff paper. The authors, seeking the presentation of a balanced point of view, explore the pros and cons for each program without, regrettably, drawing any net conclusions. The reader is therefore left to evaluate for himself the soundness of the stated military requirements. More judgments from the well-qualified Brookings experts would increase the value of this book. Only in the final few pages do the authors advance any opinions, and these are made in such general terms as to reduce the usefulness of the study in guiding the less expert reader toward a reasonable strategic force policy. Therefore, the pamphlet is more valuable as a source of information and basic data than as a prescription for a sound future nuclear strategy.

HERBERT SCOVILLE, JR.
McLean, Virginia

Hitler's War Aims: Ideology, the Nazi State, and the Course of Expansion. By Norman Rich. (New York: W. W. Norton & Co., 1973. Pp. xliii, 352. $10.00.)

This is a strange book. Its title bears almost no relation to the text. To be exact, the Ideology of Expansion, as laid down by Hitler in *Mein Kampf*, is summarized in the first seven pages. Thereafter it hardly raises its head again except for occasional remarks that Hitler did not conform to his own rules. Another extraordinary feature is that there is no discussion of Germany's economic problems on a practical basis: not a word about the Great Depression or the system of autarchy with which Germany overcame it. Hence there is no attempt to discuss how far German foreign policy was economically motivated, how far, that is, Germany, a great industrial power inadequately endowed with raw materials, was seeking to break out of this straitjacket. The discussion of the Nazi state is also pretty summary, all done with in 65 pages.

The rest of the book is devoted to the course of German expansion. The treatment of this, too, is uneven. Rearmament, for instance, is dealt with in the simple statement that Hitler rearmed Germany. There is no explanation of the particular character of German rearmament—all for the front line, nothing in reserve, preparation for a series of small wars, not for a great one. Surely this is fundamental to an understanding of German policy before the war and of German strategy during it. Lacking this key, Rich falls back on the banal statement that "Hitler was a pathological egotist whose lust for power and dominion expanded with the prospects that opened before him" (p. xiii). Later we are told that he was "above all a prophet of the cult of nationalism," whereas his real characteristic was a determination to make Germany a World Power. Hitler recognized that Germany was not strong enough to achieve this by a direct assault on the existing order and hoped to tiptoe into world powerhood unperceived.

The course of the war from the German point of view is, on the whole, described correctly. The element of forethought in the final occupation and dismemberment of Czechoslovakia is perhaps exaggerated, and Rich fails to appreciate how deeply Hitler was committed by German sentiment, particularly among his generals, to his demands on Poland. Thereafter Rich judges with a clear grasp. He shows that Hitler was driven against his will to overrun Denmark and Norway by the British plans to cut off Germany's supply of iron ore from Sweden. He also shows that the conquest of France and the Low Countries, as distinct from their defeat, was an aberration. As Seyss-Inquart remarked, when taking up his post in the Netherlands: "In the east we have a National Socialist mission; over there in the west we have a function. Therein lies something of a difference!" Probably Rich exaggerates also the importance to Hitler of the projected invasion of England and its failure. Sea Lion was a try-on by the German army and airforce which Hitler tolerated without showing much interest in it.

It is often said that Hitler made a great mistake by not moving through Spain to North Africa in the autumn of 1940. Rich ascribes this failure almost entirely to Spanish reluctance. But Hitler believed that Great Britain was already defeated, and he feared that a German penetration into North Africa, far from bringing about a British collapse, would provoke the United States into entering the war. The only way out, to Hitler's mind, was an invasion of Russia. In Rich's words, "Hitler's arguments in arriving at that decision made it seem not only the best of the various courses open to him, but less hazardous than many of the gambles he had risked during his previous seven years of power" (p. 26). Finally, Rich tells us, Hitler encouraged Japan to move southward in order to distract the United States

from the Atlantic. When the Japanese actually went to war, he even threw in a declaration of war against the United States in order to encourage them further. This, too, was probably his motive, though Hitler's declaration of war against the United States is one of his few acts that cannot be rationally explained according to his own terms.

Rich's general approach is to explain German policy in terms of war. Hitler attacked Poland in order to secure his rear while he attacked in the west. He attacked France to secure his rear while he attacked in the east. He attacked Russia in order to secure his rear while he made a final assault on Great Britain. Only then would Germany become a World Power capable of fighting a great war against the United States. Sometimes Hitler said this would happen a hundred years later; sometimes he proposed to conduct it in his own lifetime. Perhaps therefore Hitler's war aim was to dominate the world—an ambition attributed to other Great Powers, including at the present time Soviet Russia and the United States. But to judge from events and Rich's analysis, Hitler's sole aim in the wars he fought between 1939 and 1941 was to win. As some chess grand master once said: "When I look at the board, I ask—does this move improve my position? If it does, I make it." Hitler's great negative aim was to avoid a World War. In this he failed.

A. J. P. TAYLOR

Beaverbrook Library, London

Planning, Prediction, and Policymaking in Foreign Affairs. By Robert L. Rothstein. (Boston: Little, Brown & Company, 1972. Pp. 215. $6.95.)

Professor Rothstein of Johns Hopkins University has written a complex, delightful, essay on the interplay of theory and practice in foreign policy making. He is not a disinterested party; his is an unabashedly intellectual point of view. Rothstein writes with candor and subtle humor, as he dethrones ruling ideas and prominent persons. His approach to the problem is in the end too intellectual, I think, but my disagreements with him do not diminish the fun and the profit in the reading.

Rothstein's thesis may be summarized as follows—though no brief account can do justice to the detailed web of his argument or to all of the insights in these pages: The international system, he claims, has changed fundamentally, so that the traditional principles and doctrines (which he describes as Political Realism, rather in the Morgenthau mold) are no longer relevant or applicable. International interdependence is the name of the new game, and developments within it can be influenced not by incrementalism but only by long-range planning. Such planning calls for an intimate relationship between theorists (planners) and practitioners (policy makers)—a relationship which will be possible only after major changes take place both in the way each of these sets of specialists views his proper professional behavior, and in the education of practitioners so that they know how to make use of the (more relevant) work of theorists.

The manifest purpose of the book is to get "true" policy planning enthroned in foreign policy making (though not in the State Department); but I found Rothstein's discussion of the problems of foreign policy making more perceptive than his proposed solutions. Rothstein has done a superb job in confronting the intellective, practical, and affective configurations of the "two cultures" of foreign policy. But while the world of the practitioner of Realism (e.g., pp. 76–77) is as good a capsule explanation of the White House of Watergate as we are likely to find anywhere, I am doubtful that practical remedies are going to be found—or should even be sought—through the work of theorists. Rothstein criticizes Political Realism for being, among other things, elitist and antidemocratic. It strikes me as professional myopia to believe that one can find a democratic alternative to these particular sins among academic foreign policy theorists! A generous definition of "theorist" might turn up a non-elitist (but not a non-elite) group, but Rothstein isn't even inclined to be very generous: Those in the academy who offer advice and opinion on policy are playing the practitioner's role (p. 15) and should not be regarded as theorists. The few who are left, however, can hardly be counted on to fuel a democratic counter-revolution.

This is not a petty point. Rothstein conceives of planning as "an effort to control or limit uncertainty," and it "clearly implies some active intervention into ongoing affairs" (p. 89). It is questionable whether any distinction between theorist and practitioner is viable in the light of this conception of planning. In any event, I would argue that this kind of planning goes to the very heart of political choice (if not of divine intercession) and is therefore far too important to be left to the theorists. If I understand Rothstein correctly, he admits at a later point that what he regards as an inescapable need to rely on expert judgment "surely does some damage to traditional principles of representative government" (p. 197). Had he confronted that problem at the beginning of his essay rather than at the end, the pages in between might have read very differently.

Rothstein has gunned down the foreign affairs bureaucracy with a practiced eye, as if the signal had at long last been given for the revolt of the intellectuals. And we can murmur "Write on!"

because his aim is so true. But I remain puzzled about why intellectuals, so addicted to canons of evidence on other propositions, are so readily persuaded by logic alone of their own unique and indispensable contribution to the political salvation of mankind. Where, indeed, is the evidence that the theorist *qua* theorist can help the practitioner to do better—to "improve his judgment" (p. 147)? One might argue that he can scarcely do worse—but self-serving thoughts of this kind are insufficient basis for the reorganization of national policy making. This is only one of the unexplored normative judgments that inform Rothstein's analysis; he aims for "true policy planning," which could never lead us to disaster, presumably, else it wouldn't be "true." His aspiration is that practitioners will "ask the right questions," and "make better decisions." All of which leads me to the conclusion that it is not policy making but policy itself that is up against the wall, albeit in disguise. (What then of Rothstein's credentials as theorist?)

Let me make the point another way: I do not find it so clearly established, as Rothstein seems to, that interdependence has replaced security as the dominant consideration in world politics. It is one thing to believe that the Cold War is over—and even that one should ferret out the remnants of the Cold-War mentality in the policy folkways of a generation, both in government service and out of it. But it is quite another to argue that the situation calls for an "alternative interpretation of world politics," for a new style of decision making, for changes in basic thought patterns (pp. 78–79). Overkill, perhaps, in the interest of an eventual new post-Vietnam policy doctrine, the likes of which are not revealed to us. If so, we might do better to confront that issue directly, so that we know what we are arguing about. But in the meantime, if we have to move the question "Where do we go from here?" to a higher, more generalized, plane, Rothstein has at least picked one with some substance, and treated it with intelligence and style.

BERNARD C. COHEN
University of Wisconsin, Madison

The Dimensions of Nations. By R. J. Rummel. (Beverly Hills: Sage Press, 1972. Pp. 512. $20.00.)

The *Dimensions of Nations* book has been built around, but is not a collection of, many of the earlier research reports and publications from the Dimensionality of Nations project (DON). This project, started in 1962 at Northwestern, has moved through several stages of development. The initial part of the project was begun to replicate the work of Cattell, who had previously located nations on various attribute dimensions using factor analysis. His study, however, was rather truncated and the DON project aimed at increasing vastly the number of nation and attribute variables processed. The *Dimensions of Nations* book covers this initial phase thoroughly providing a complete description of how the data were collected, processed and evaluated to produce massive evidence for empirical dimensions of nations, for a topology of nations, and for many political, social and economic indicators.

After this initial period (1962–66), attention was turned toward behavioral relationships between nations, a study which finally culminated in Rummel's Social Field Theory and Status Field Theory, which are briefly described in the book.

Since nearly half of the book has already been published in the prolific DON research reports, one may ask why publish a book? I feel that the book is an improvement upon the research reports. First, presumably having had to confront an armada of readers and editors all suggesting revisions and improvements, the book is a tighter product. Second, some of the unorthodox positions taken in the earlier research reports and published papers have been modified. A good example of this is Rummel's earlier view on explanation and prediction and the more acceptable one he puts forward in the book. Third, it is far more convenient to have one comprehensive summary volume covering the whole atheoretical period of the project than having the piecemeal research reports cluttering up one's shelves. The book exhibits mature consideration rather than the unfocused style so evident in the research reports.

And finally, since DON is one of the superprojects in international relations, having spent almost a million dollars, it is essential that there be some permanent and parsimonious record of its research. This volume, along with the others intended by Rummel, will form a series which will trace the development of the project from its atheoretical beginnings through its more theoretical research thrust.

The book contains a monumental amount of data on the attributes of eighty two nations. The attributes of these nations are spread among 230 variables. This whole data matrix is factor analysed to produce the more distilled dimensions of nations in attribute space. In this way nations can be located in attribute space and those clustered together are scrutinized for possible links producing such similarity. The research thus delineates nation types. Foreign conflict behavior is also processed along with the attributes of nations. It thus became possible to determine whether nations with any particular attribute pattern evidenced any foreign conflict behavior pattern.

Although the book is a wealth of data and data-

related techniques and will be very useful to the international relations empiricist, I suspect that the volume (and the series) will also be of use to the scholar interested in the development and history of knowledge in political science. The project spanned a period from 1962 to 1973 and at one time had thirty staff members. It moved from Northwestern to Indiana to Yale and finally ended up in Hawaii. Much of the research has attracted fierce criticism, far more than is normal.

All of this evidence can be used to contrast the various ideas of knowledge growth put forward by Kuhn, Lakatos, and Popper. (Lakatos and Musgrave, *Criticism and the Growth of Knowledge* [Cambridge: Cambridge University Press, 1970.]). Without doubt a Kuhnian "normal science" perspective is exhibited in the approach of Rummel, and so too are the "naif falsificationism" of Popper and the "sophisticated falsificationism" of Lakatos. DON provides an excellent case study by which to compare and contrast these various ideas of how political science knowledge is accepted, rejected and accumulated. The book might have paid more attention to why the project began. Presumably no one initiates such a large project without some goal. Harold Guetzkow (one of the early project leaders) has mentioned that his INS and IPS simulations required validating data. If this was the original aim of the project (and it has been used in this way by Chadwick) what produced the swerve toward social field theory? Was it the rampant criticism of the inductive and atheoretical aspect of the earlier work of DON?

In general the book is well written and produced. One is allowed an awe-inspiring glimpse into the detail of the project, from the collection, coding and scaling of the data to the subtle analyses of the effects of data omission and error. As I think later volumes in the series will show, Rummel's incremental research strategy generates much admiration.

For the international relations scholar and the comparative politics specialist, there is a wealth of "clean" data readily available here, with which to test a variety of hypotheses. It is thus as useful a shelf addition as the World Political Indicators book (Russet, Deutsch, Alker, Lasswell. *World Handbook of Social and Political Indicators* [Yale, Yale University Press, 1964]), although it has less general relevance.

Rummel's early atheoretical style is still reflected in the book. For example, the first part of the book is devoted to just listing the findings of the project without any interpretation or fitting into extant knowledge. This is remedied to some extent later in the book, but not nearly as much as I would like. Nevertheless, his attempt at embedding, particularly in the later parts of the book, is some improvement upon the more factual and less interpretative research reports.

A significant part of the book is concerned with the methodology employed—factor analysis. A chapter or so attempts to enlighten the reader as to the various methodological maneuvers that follow. Factor analysis, however, is only one technique that might have been used. There are others, such as multidimensional scaling, all of which are equally credible. Nowhere in this volume does Rummel consider the alternatives, although in fairness to him they are considered in his Applied Factor Analysis book (Rummel, *Applied Factor Analysis* [Evanston: Northwestern University Press, 1970]). Nor is there much discussion of the underlying theoretical assumptions of factor analysis—assumptions such as linearity and additivity. Since the initial aim of the project was to replicate the work of Cattell, who used factor analysis, it may be that DON also had to use factor analysis. If this was the case, why not admit this instead of giving the impression that the use of the techniques was decided purely on research criteria.

Rummel is winding down the project now and is writing up the subsequent volumes in the series. I think that this first volume will be remembered for its methods rather than its theoretical import; for its data rather than its substantive depth. But it has value added because it traces the development of a large political science research project.

GORDON HILTON
Northwestern University

Republic Or Empire: American Resistance to the Philippine War. By Daniel B. Schirmer. (Cambridge, Mass.: Schenkman Publishing Co., Inc., 1972. Pp. 298. $9.50 cloth; $4.50, paper.)

Daniel B. Schirmer feels that the questionable American role in Vietnam has focused attention on past overseas adventures undertaken in the name of humanitarianism. In the 1890s, there developed one of the most fascinating such examples culminating in the acquisition of noncontiguous possessions. Cleveland's Venezuela Message (1895) asserted the force of the Monroe Doctrine in defiance of Great Britian and her South American policy. Latter-day Jeffersonians eschewed an activist, expansionist role for the United States, preferring instead to practice the time-honored policy of twisting John Bull's tail. But the McKinley Administration was more receptive to outward thrusts. The Hawaiian Annexation Treaty and the acquisition of empire that resulted from a war fought to liberate Cuba from Spain are cases in point. A "Great Debate" ensued in the Senate about the retention of over-

seas territory and especially about what to do with the Philippines. An independence movement thirty years old, and concern over the economic, humanitarian, and constitutional propriety of acquiring remote land made little impression in the thinking of American expansionists who equated taking territory five thousand miles from our shore with the growth of American power and status. Anti-imperialists opposed what they called an unrighteous war with the insurrectionists that lasted until 1902. Their organization, the Anti-Imperialist League, created in 1898, centered in Boston, and dominated by a New England intellectual elite, was the force around which "Little Americans" grouped. In the election of 1900, the League was stymied by a candidate with whom it had little sympathy except for his non-expansionist ideas, and by a cause that ran counter to the *Zeitgeist* at the end of the century. Expansion was the order of the day, the break with tradition was made, and the twentieth century seemed to portend a new vigorous policy for the Republic, one which nevertheless had many serious implications.

Revisionist historians have been criticized for polemical statements and strained analogies. The relation between the Filipino episode and Vietnam, and the American role in the Philippines as inspiration for Vietnam is too similar not to make the comparison. Relating earlier episodes to contemporary events is fraught with danger, but such parallels often show the relevance and provocativeness of historical study. While Schirmer acknowledges that the Vietnamese imbroglio and the Filipino insurrection were both imperialistic, his treatment of the latter involvement and of the anti-imperialist resistance is judicious, restrained, and well documented. Perhaps more controversial is Howard Zinn's introductory observation that because our historical education was so imperfect, we did not immediately associate the Filipino affair with the Vietnam war as the latter became "obviously ugly."

Not surprisingly, Schirmer feels that governmental imperialism was fostered by a complex of industrial, financial, and racist forces. America's economic position, this argument held, necessitated foreign markets, and the Philippine Islands were a convenient base for the illusory Chinese trade. As has been pointed out so often, however, not all business men were of a single mind; prestige and grandeur were strong motivations, and the anti-imperialists were also divided on the subject of economic determination.

Schirmer points out the intresting relationship between anti-imperialism and the antislavery movement of four decades earlier, demonstrating clearly the link between southern white supremacy and the policy of dominating the Filipino natives. Indeed, Schirmer's heroes—the anti-imperialists—were not only opposed to military-economic exploitation but were also the first to show the connection between the black civil rights struggles at home and overseas imperialism.

Despite the initial failure of the Anti-Imperialist League, Schirmer aptly notes that it was instrumental in tempering future policy on colonialism. Reforms did come within the military as a result of the publicity given over the "genocidal" subjugation of the Filipino rebels.

Schirmer's emphasis is on economic and racist motivation. The former is too simplisitic an explanation and the latter is a real contribution. This work is based on solid research in manuscripts, government documents, and periodicals, and the photographs and drawings are a real asset. Unfortunately, it relies too heavily on two leading Massachusetts newspapers, the bibliographical note is too short and hardly reflects this topic as an historiographic battleground, and the sources are listed as backnotes and not footnotes.

Despite these flaws, the author has focused our attention on an important historical episode which he feels has contemporary relevance. Unlike other analyses that attempt to draw parallels, this is basically a balanced and relatively unemotional treatment of an emotionally loaded incident.

MILTON PLESUR
State University of New York at Buffalo

The Berlin Crisis of 1961: Soviet-American Relations and the Struggle for Power in the Kremlin, June–November 1961. By Robert M. Slusser. (Baltimore: The Johns Hopkins University Press, 1973. Pp. 509. $17.50.)

Professor Slusser's book offers a painstakingly researched chronological account of six critical months in the relations between the United States and the Soviet Union beginning with the Soviet government's insistence in June of 1961 that a peace treaty be signed with Germany by the end of that year. Amplifying this demand, Nikita Khrushchev declared West Berlin should be given the status of a "free city," and, following the conclusion of the peace treaty with the German Democratic Republic (GDR), control over the access routes to West Berlin would be turned over to the GDR authorities because "the sovereign rights of the GDR, through whose territory the communications linking West Berlin with the outside world pass, must be respected" (pp. 15–16).

Concurrently the Soviet government was intent on resuming atmospheric tests of nuclear weapons in the multimegaton class in violation of the 1958 de facto test ban. At the same time

the United States was seeking to work out a formal treaty banning such tests. During this period certain other Soviet foreign policy objectives had a bearing on Soviet-American relations; they included the introduction of the troika system for the post of Secretary General of the United Nations, improvement in the relations with the Peoples' Republic of China and Albania, and Khrushchev's desire for the Soviet Union to surpass the United States economically.

Despite various Soviet threats, which prompted the Kennedy Administration to increase defense expenditures substantially and call reserves to active duty, and alternatively confidential personal communications from Khrushchev to Kennedy through letters and other means urging negotiations on the peace treaty, the deadline was abandoned in October, and the troika initiative for the UN given up. However, the Berlin Wall, designed to stop the flood of refugees to West Germany, was built during that period, and the Soviet Union proceeded with atmospheric tests of nuclear weapons up to 58 megatons.

In his analysis and evaluation of the crisis-filled six months Slusser examines in detail the factors responsible for the rise of tension between the superpowers and the pressures building up on Kennedy and Khrushchev to adopt tough positions toward each other. The author probes deeply into the factionalism and ongoing power struggle in the Presidium of the CPSU's Central Committee as it relates to Khrushchev's ability to formulate and carry out policies. His main antagonist and leader of the hardliners in foreign policy was Frol Kozlov, whose strategies for placing constraints on Khrushchev's actions and for imposing his own foreign policy concepts toward the United States and China are carefully analyzed. Clearly, major decisions about the policies to be pursued before and during the crisis were not those of Khrushchev alone but of the collective leadership in the Presidium. In certain instances, however, Khrushchev sought to circumvent the Presidium's collective decision making or to influence it through devious means.

Although in Kozlov's view good Soviet relations with China should receive highest priority, for Khrushchev it was only the Soviet Union and the United States which counted in world politics. If these two countries unite for peace, there can be no war, he declared in his confidential letter to Kennedy (p. 255). Khrushchev's idea of a Soviet-U.S. joint rule over the world has continued to attract subsequent Soviet leaders but has upset many of NATO's European members who fear that such a close U.S.-Soviet relationship would diminish their power.

In his evaluation of the interrelationship between the peace treaty deadline and the nuclear tests Slusser implies that the tests constituted a smokescreen to distract attention from the Soviet abandonment of its much heralded plan to sign a peace treaty with East Germany before the end of the year (p. 168). Could the reverse not be true, however? The tests may well have been the primary objective for 1961 and the peace treaty secondary. The threat to sign the treaty with the GDR may have been designed to raise the level of tension in East-West relations in order to absorb the attention of NATO leaders and conceal the chief intention of Soviet decision makers to test new powerful weapons, with the increased tension serving as a convenient pretext for the tests.

Without doubt Slusser's "microscopic" study (p. ix) of the power struggle within the Presidium based on minute details observed during the 22nd CPSU Congress has yielded significant inferences about the activities in the Presidium regarding foreign and internal policies. But the long quotes from the more important speakers during the Congress and the summaries of the speeches of others are somewhat tiresome, and the chapter on the Congress—nearly 200 pages long—could have been shortened appreciably without depriving the reader of the necessary insight into the factional struggle. Despite this minor criticism, Slusser's book makes a major contribution to better understanding the Kremlin's decision-making process, which is far from the monolithic procedure that existed under Stalin. Just as in the United States, there appears to be a definite interplay between domestic and foreign policies. It would have been interesting to learn about the influences of the Soviet bureaucracy upon foreign policy formulation, and compare them with studies made by American scholars on the influence of U.S. bureaucracy on U.S. foreign formulation.

WERNER J. FELD
University of New Orleans

Barriers to Progress in South Vietnam: The United States Experience. By Edward Reynolds Wright. (Seoul, Korea: Pomso Publishers, Ltd., 1973. Pp. 174. No price indicated.)

So many books have been published about the civil war in Vietnam, and particularly concerning American intervention, one might wonder what more could be added. In *Barriers to Progress in South Vietnam: The United States Experience*, Edward Wright, long-time executive director of the U.S. Fulbright Program in Korea, has produced an analysis, although short in length, which is uniquely penetrating and revealing.

Having served as a visiting professor in Saigon during the fateful 1963–1964 period that witnessed the fall of Diem and incipient American belliger-

ency, and again in 1966 during massive Americanization and escalation of the war, Dr. Wright has shown himself a consummate student of the history, culture, economics, and politics of this ill-fated country.

This study is neither a prescriptive policy paper nor a detailed analysis of the traumatic developments since 1963. Nor is any attempt made to treat U.S. efforts to pacify the countryside or to assess the peace movement within U.S. society—the substance of most recent books on Vietnam.

Rather, it is the author's effort "to clarify the underlying issues which should continue to be basic in any policy formation with regard to Vietnam." Accordingly, considerable attention is devoted to developmental programs and progress for the decade 1954–1964, analysis of which ends in 1964 when the conflagration of war enveloped the Vietnamese. The author suggests that this is the critical period in the recent history of Vietnam, for this decade embraced "the barriers to developmental progress" that explain the foreground to war and will persist for years to come. The merit of this study, then, is that it identifies and clarifies the problems that confront strong nations which try to guide economic, social, and political development in weak nations.

The massive United States aid program in South Vietnam entailed the expenditure of more than $2,100,000,000 dollars in economic assistance alone between 1955 and 1964. In assessing the administration of U.S. aid, Dr. Wright poses this question: "To what extent does the dispenser of aid attempt to impose its values on the administration and implementation of the aid program?" This is a problem of "cultural transference"—of transferring money, materials, and skills from the U.S. whose social strata and political culture are radically different from Vietnam.

The author identifies three major categories of "barriers to progress" in utilizing U.S. aid: (1) political instability and military insecurity, (2) widespread waste and corruption in the use of U.S. aid funds, and (3) a drastic shortage of trained technicians and professionally qualified personnel. South Korea, he asserts, is a success story because it has "strikingly different potentialities" for managing its own affairs.

Although discussion of these barriers represents the thrust of this book, it appears to this reviewer that the author makes another singular contribution to the burgeoning literature on Vietnam through imparting his understanding of Vietnamese society and his grasp of its historical background. His second chapter on the French War in Indochina is quite instructive, particularly about political cleavages within France which affected French policy in Vietnam.

It was Georges Bidault's short-sighted anti-Communist policy, and his refusal to consider the basic issues in Indochina, which brought an end to the Laniel Government. The final decisions reached at the Geneva Conference of 1954 were instigated by the Pierre Mendès-France Government in which Bidault and the MRP refused to participate. Dr. Wright's fluency in French enhances his treatment of these developments, as he exhibits equal facility in the use of French- and English-language sources.

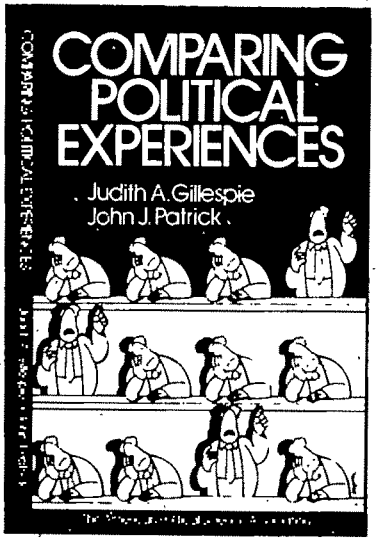
If this book has any flaw, it is the brevity of the author's treatment of the Viet Minh and North Vietnam's political culture, especially its ideology. At least equal weight should be accorded to the ideological and nationalistic perseverance and will of the opponents of the French, the Americans, and the Saigon governments.

But Dr. Wright has produced a brilliant book, well documented, attractively designed, and clearly written. Added features include a useful guide summarizing major periods in Vietnamese history, ten tables concerning economic factors, an appendix comprising the full text of the Vietnam Peace Agreement of January 27, 1973, and a complete index. A "postscript" on Vietnamization consists of the author's devastating review of the book "No Exit From Vietnam" by Sir Robert Thompson—reputed creator of President Nixon's Vietnamization strategy.

The depth of Edward Wright's analysis is indicated by enlargement of his main theme, in his conclusion: "In the final analysis, the relatively developed nation can only advise, help and protect. By no means should they attempt to impose a unilaterally derived alien system upon a traditional culture and expect it to 'work'." Had such advice been followed, perhaps the tragedy of Vietnam could have been avoided. Here, then, is a lesson that should be heeded by other nations which are recipients of U.S. aid.

WILLIAM W. BOYER
University of Delaware

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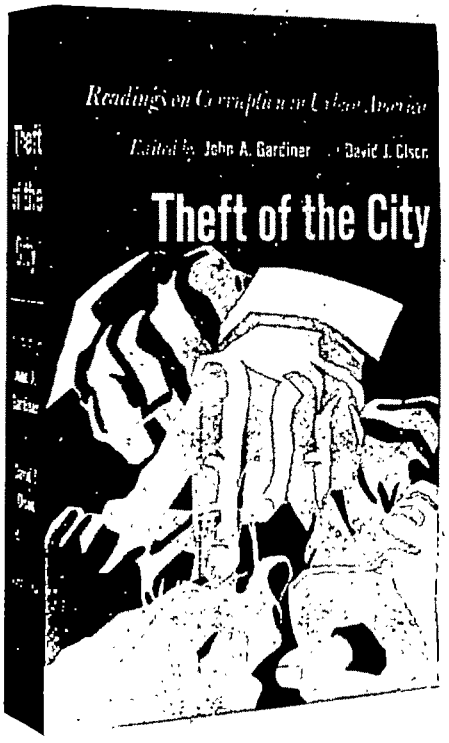
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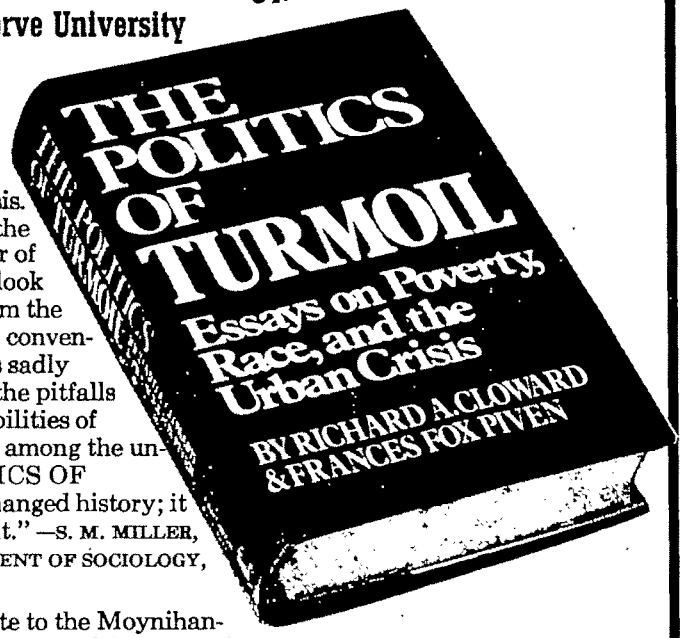
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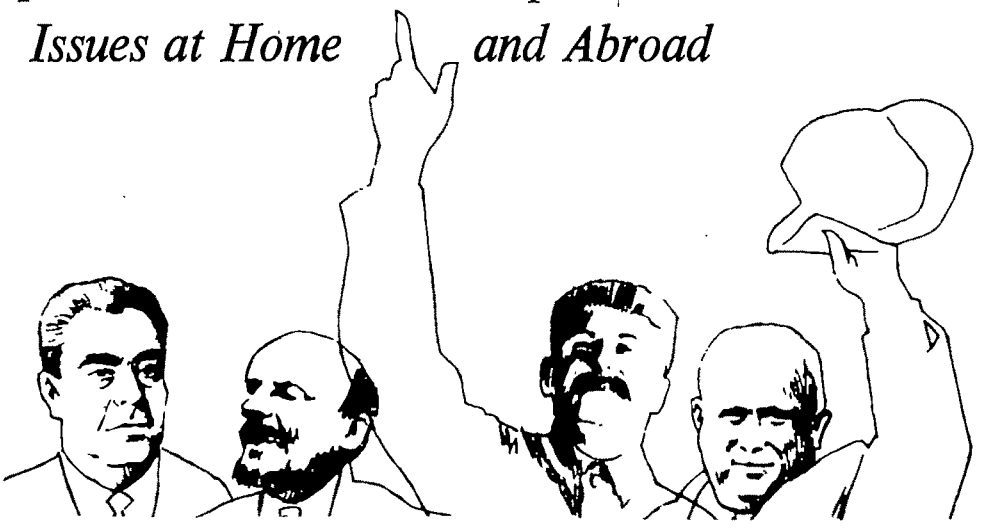
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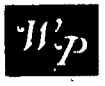
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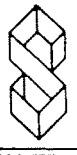
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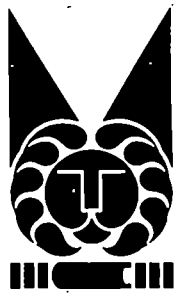
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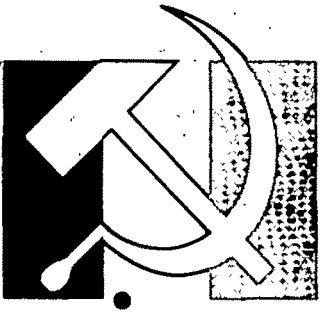
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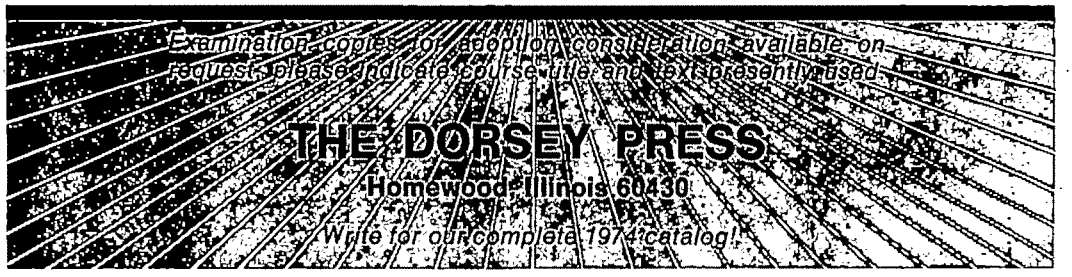
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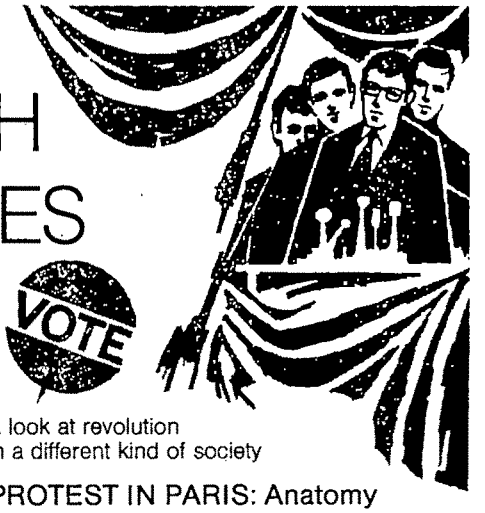
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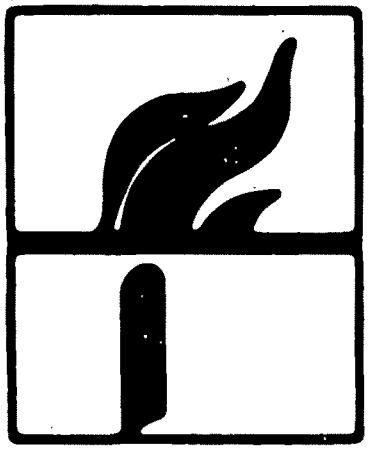


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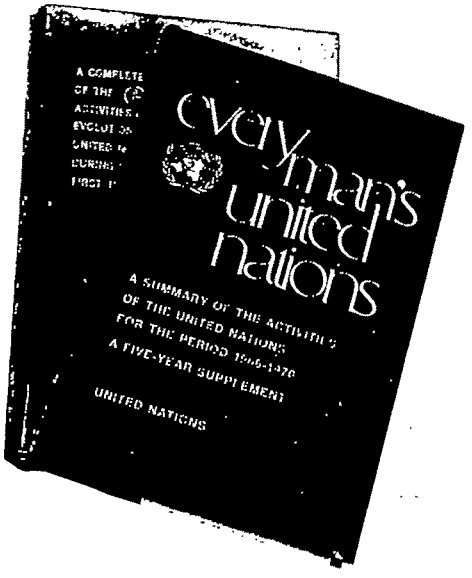
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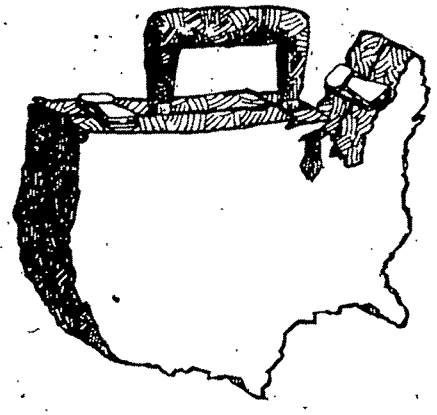
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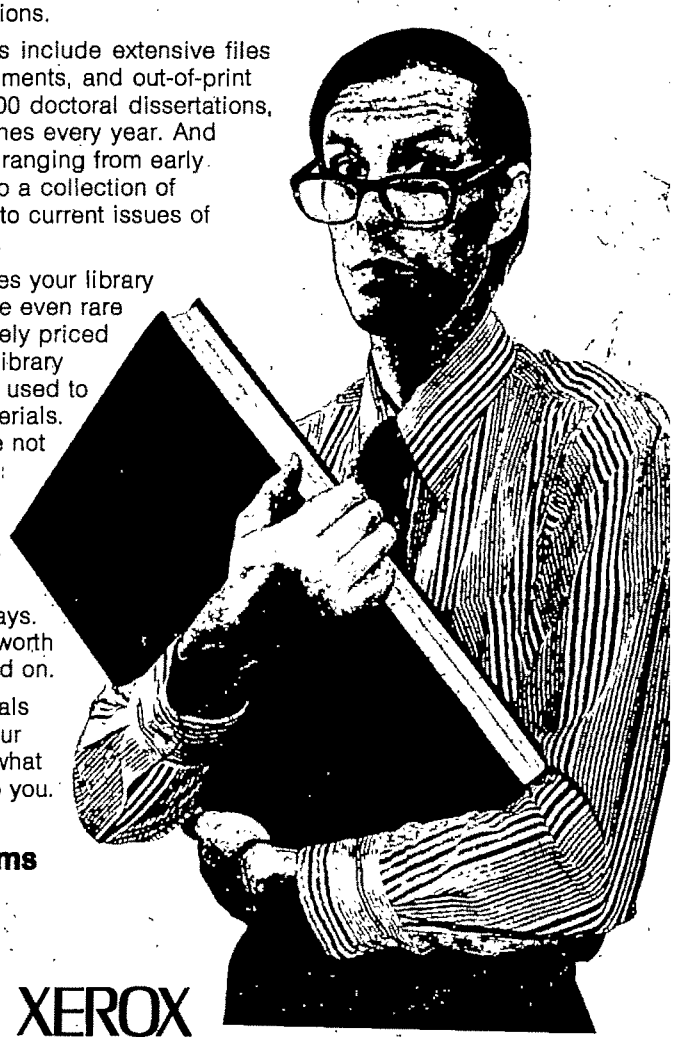
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Designed for the one-quarter or one-semester course, this extensively revised and shortened version of the eminent authors' AN INTRODUCTION TO AMERICAN GOVERNMENT (described below) provides a totally unbiased working knowledge of the American political system. Lighter in approach and 40% shorter than the parent text, this core text has been completely rewritten. It now includes new data throughout, new controversies (each chapter concludes with a controversial question designed to help the student understand underlying issues), more photographs, and an expanded treatment of Watergate. Examples apply everyday experiences to American politics. Tentative: 384 pages; $6.95; paper. February 1975. *Instructor's Manual.*

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CONTENTS

1478 Editorial Comment

1482 Persistence and Change in Political Systems, 1800–1971 — Ted Robert Gurr

1505 On the Origins of Interest-Group Theory: A Critique of a Process — G. David Garson

1520 Ethnic Voting and Political Change in South Africa — Stanton Peele and Stanley J. Morse

1542 Hobbes and His Audience: The Dynamics of Theorizing — Mark Gavre

1557 Separate and Unequal: Governmental Inequality in the Metropolis — Richard Child Hill

1569 Conflict, Power and Relative Deprivation — Walter Korpi

1579 The Influence of Parents on the Political Attitudes of Adolescents — Kent L. Tedin

1593 Majority Party Leadership and the Committee System in the House of Representatives — Louis P. Westefield

1605 Ethnic Politics, Representative Bureaucracy and Development Administration: The Zambian Case — Dennis L. Dresang

1618 Levels of Sociocultural Development Among Soviet Nationalities: A Partial Test of the Equalization Hypothesis — Brian Silver

1638 Models of Statistical Relationship — Herbert F. Weisberg

1656 Individual and Contextual Variables in British Voting Behavior: An Exploratory Note — C. Neal Tate

1663 Community and Control: Critical Democratic Theory in the Progressive Period — David E. Price

1679 Communications

1696 Book Reviews and Essays
Political Dictionaries: A Bibliographical Essay — Clement E. Vose
Political Implications of Gender Roles: A Review of the Literature — Wilma Rule Krauss

1885 Index to Volume LXVIII

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ARTICLES

1482 **Persistence and Change in Political Systems, 1800–1971.** This study reports the codification of basic authority characteristics of 336 national political systems (polities) that functioned in 91 nation-states between 1800 and 1971. In form the typical 19th-century polity was an autocracy with minimal functions. Its 20th-century counterpart was either an activist plural democracy or an activist autocracy. The incidence of system-transforming political change has been equally high and pervasive in both European and Third-world polities, but greater in the 20th century than the 19th. The data are used to test three hypotheses that attribute the persistence and adaptability of political systems to their authority characteristics. "Institutionalization" arguments about the stability-enhancing effects of complexity and directiveness receive no consistent support. Conventional beliefs about the greater durability of democracies vs. autocracies vs. anocracies (uninstitutionalized polities) are confirmed only in Europe in the 20th century. The most durable historical and Afro-Asian polities have been either autocratic or anocratic. The data generally support the hypothesis that "pure" political systems—consistently democratic *or* consistently autocratic—are more durable than systems of mixed authority characteristics. Long-term trends in political "development" and their determinants are discussed in the light of the findings.

By TED ROBERT GURR, Professor of Political Science, Northwestern University.

1505 **On the Origins of Interest-Group Theory: A Critique of a Process.** Starting with nineteenth-century jurisprudence, this essay traces the changing perceptions of the nature and role of groups in politics as affected in turn by scientific reformism, classic pluralism, and descriptive science orientations to the study of politics. The work of Laski and Bentley is highlighted.

The essay contends that political science has not grown by scientific study laid upon scientific study, but rather by a process of gradual encroachment of ideas in which basic premises, questions, and terms shifted without adequate examination. It is argued that this change is not properly portrayed as a trend from abstract legal metaphysics to ever more sophisticated approximations of reality. The forgotten contributions of past orientations are stressed, and the reasons that Bentley was ignored in his day discussed.

More specifically, the study concludes that the process of accretion of ideas was of some benefit to political science as a discipline, but the heritage left by this process was negative in two important respects: the neglect of a dynamic in favor of a static perspective, and the divorce of normative theory from empirical research on American politics.

By G. DAVID GARSON, Assistant Professor, Tufts University.

1520 **Ethnic Voting and Political Change in South Africa.** Immediately prior to the 1970 parliamentary election in the Republic of South Africa, 462 white voters in Cape Town were questioned about their demographic backgrounds, voting intentions, and political attitudes. The study showed that ethnicity is the major determinant of party vote: Afrikaners vote for the National Party, the English-speaking for the United Party. SES-related factors predict party identification only insofar as they covary with ethnicity. While a liberalization of political attitudes with rising SES can be observed, this has no bearing on electoral behavior. Party vote is not related to ideological or issue orientations, but is related to the intensity of the voter's identification with his own ethnic group and with white South Africans in general. Voters tend to react positively or negatively to the NP, with the UP serving chiefly as a vehicle for protest votes against the government. The slight drop in NP support in 1970 was due to a key group of abstainers who—while basically Nat supporters— were more liberal than those who said they would vote for the NP. It is "Ambiguous Afrikaners" (those who are changing to an "English" identity), and only some of those, who are defecting completely from their traditional political allegiance. They represent the one sign of potential change in South Africa's uniquely stable political system.

By STANTON PEELE, Assistant Professor of Organizational Behavior, Harvard Business School, and STANLEY J. MORSE, Fulbright Professor of Social Psychology, Pontifícia Universidade Católica de São Paulo, Brazil.

1542 **Hobbes and His Audience: The Dynamics of Theorizing.** This paper argues for a conceptualization of political theory based upon a jurisprudential model of reasoning, rather than the more commonly accepted model of logical demonstration. The purpose of political theorizing is to persuade and convince, not to prove; consequently it is a form of argument necessarily directed at a particular audience or audiences. Hobbes is examined as a theorist who directed his argument at those audiences which were politically most significant. This paper explores that aspect of Hobbes's theory which was intended to persuade his Puritan audience. Hobbes attempted to persuade the Puritans by presenting his argument in a manner subtly similar to the style of reasoning they were familiar with in religious terms. This interpretation is supported by a comparison of the arguments of Hobbes and Calvin. This analysis of Hobbes is intended to illustrate both a general view of the nature of political theorizing, and the advantages of adopting an historical method in examining past political theorists.

By MARK GAVRE, Ph.D. Candidate in Political Science, University of California, Los Angeles.

1557 **Separate and Unequal: Governmental Inequality in the Metropolis.** The political incorporation and municipal segregation of classes and status groups in the metropolis tend to divorce fiscal resources from public needs and to create and perpetuate inequality among urban residents in the United States. An investigation of data collected for a large number of metropolitan areas in 1960 reveals a number of variables associated with inequality in the distribution of fiscal resources among municipalities in metropolitan areas. The level of income inequality among municipal governments in metropolitan areas varies directly with: location in the South; age, size and density of the metropolis; nonwhite concentration; family income inequality; residential segregation among social classes; housing segregation by quality; and governmental fragmentation. The data provide support for the argument that governmental inequality occupies a central position in the urban stratification system.

By RICHARD CHILD HILL, Assistant Professor of Sociology, Michigan State University

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1569 **Conflict, Power and Relative Deprivation.** The widely accepted expectation achievement approach to conflict, which views conflict primarily as a response to relative deprivation, has recently been challenged by proponents of a political process approach, the central features of which are mobilization of power resources and the struggle for power. Here a power balance model of conflict is developed which incorporates the core concepts from both approaches. In this model the difference in power resources between the contending parties is used as the central independent variable. Relative deprivation, utility of reaching the goal and expectancy of success are introduced as intervening variables to relate the effects from changes in the balance of power between the parties to the probability of manifest conflict between them.

According to the power balance model of conflict different types of relative deprivation (aspirational, decremental and progressive) will be differently correlated with the probability of conflict. The overall correlation between relative deprivation and conflict is expected to be insignificant. Situations where the difference in power resources between two parties is decreasing are seen as most conducive to conflict. When the power resources of an already weaker party are decreasing, the probability of conflict is assumed to be lower than when the weaker party is gaining power resources.

by WALTER KORPI, Professor of Social Policy, Swedish Institute for Social Research and Department of Sociology, University of Stockholm.

1579 **The Influence of Parents on the Political Attitudes of Adolescents.** The purpose of this paper is to specify the conditions in which parents influence the party identification and certain issue attitudes of their adolescent children (recent high school graduates). The nature and extent of the parent-adolescent attitude correspondence is first established. Next, parental as opposed to environmental explanations for this correspondence are considered. Finally, the effects on parental influence of family interaction, political interest, issue salience to the parent, and accuracy of the adolescent's perception of the parental attitude are analyzed. Issue salience and perceptual accuracy are found to have strong effects; the other variables have lesser or no effect. When issue salience and perceptual accuracy are held constant in a multivariate equation, the beta weights indicating the influence of the parent attitude on the attitude of the adolescent are very similar for all issues and party identification. It is concluded that idiosyncratic variations in successful parent-child attitude transmission can be explained by a general equation.

By KENT L. TEDIN, Assistant Professor of Political Science, University of Houston.

1593 **Majority Party Leadership and the Committee System in the House of Representatives.** In this paper one party leadership strategy with respect to the committee system of the House is examined. Building on several relatively clear concepts such as compliance, quality of assignment, expectation, scarcity, and exchange, a very elementary, yet explicit, theory is constructed. It is shown that the leaders pursue a strategy of accommodation. The leaders increase the number of positions on those committees prized by the members in order to guarantee a steady supply of resources to gain leverage with the members. But a steady increase in the supply of positions reduces the scarcity of positions and hence their value to the leaders. Thus, a consequence of the strategy is the need periodically to reorganize or make adjustments in the committee system.

By LOUIS P. WESTEFIELD, Assistant Professor of Government and Public Affairs, Southern Illinois University, Edwardsville.

1605 **Ethnic Politics, Representative Bureaucracy and Development Administration: The Zambian Case.** Ethnic groups argue that a certain proportion of administrative positions should be filled by their members. This concern assumes that a bureaucrat applies to his tasks the values and interests of his social background. Evidence from Zambia, a country in which ethnic and regional differences have been highly politicized, suggests that this assumption lacks empirical support. The fundamental guidelines that appear to shape administrative behavior in Zambia are the calculations of what must be done to achieve individual career advancement. Administrators are, in addition, constrained from pursuing parochial interests by the range of authority and discretion attached to their particular positions.

Although bureaucrats do not appear to provide ideal representatives for ethnic interests, the ethnic

composition of the most visible echelons of the government is of critical political significance. This is particularly evident in a country like Zambia where ethnically defined groups are contending for positions in the new postcolonial society. The symbols of power can be as important as power itself.

By DENNIS L. DRESANG, Associate Professor of Political Science, University of Wisconsin, Madison.

1618 **Levels of Sociocultural Development Among Soviet Nationalities: A Partial Test of the Equalization Hypothesis.** Recent studies of regional economic development of the Soviet Union have suggested that since 1958, Soviet union republics have become less equal in their levels of industrialization, share of investments, and living standards. This paper assesses the degree to which equality of social and cultural development characterizes Soviet ethnic groups and union republics. Drawing aggregate data chiefly from the 1926, 1959, and 1970 Soviet censuses, the analysis shows that: (1) relative variation (inequality) in the sociocultural development of Soviet nationalities has declined continuously and markedly over time on measures of education and occupational structure, but that large inter-nationality disparities remain in the development of skilled manpower; (2) inter-nationality variation in family role structures has been slower to decline than differences in education and appears to follow a curvilinear pattern: for a time during the Soviet era, inter-nationality differences *increased* but have been followed recently by significant declines. Furthermore, it is shown that (3) on the average the sociocultural development of union republic populations well exceeds the development of the indigenous nationalities in these republics.

By BRIAN SILVER, Assistant Professor of Government, Florida State University.

1638 **Models of Statistical Relationship.** The choice among measures of relationship has increasingly become a matter of the interpretation of their intermediate values. Interpretations are important, but a prior question is the statistic's implicit model of a relationship—what it considers a perfect relationship, and what it considers a null relationship. A family of models based on combinations of certain maximum- and null-value conditions is analyzed in this paper. The distinction between the models can be used to shed light on the stakes involved in the choice among dichotomous variable measures as well as that among familiar ordinal statistics.

The models are ordered in terms of their leniency, and the coefficients based on each model are specified. An empirical analysis shows that the different measures are positively correlated, but those measures based on different models can differ sharply from one another. Statistics based on the same model covary regardless of differences in their interpretations. Since different models are intended to measure different concepts, multiple coefficients can allow investigators to examine their data in greater detail. Several political examples of the use of multiple models are provided.

By HERBERT F. WEISBERG, Associate Professor of Political Science, Ohio State University.

1656 **Individual and Contextual Variables in British Voting Behavior: An Exploratory Note.** This note reports the results of an initial exploration into the significance of the social environments ("contexts") in which people live in the shaping of their individual political behavior. Many scholars have argued that social scientists should pay more serious attention to contextual variables when they go about constructing social theories. But there have been few systematic efforts to demonstrate empirically the overall importance of contextual variables as predictors of individual behaviors, especially relative to the importance of personal ("individual") predictors. Here the relative potency of two sets of predictors—one individual and one contextual—is investigated for a sample of British voters by means of a well-known multivariate search strategy, "tree analysis." The results suggest that contextual variables have little to add to explanations of voting behavior based on individual variables—at least for these data.

By C. NEAL TATE, Assistant Professor of Political Science, North Texas State University.

1663 **Community and Control: Critical Democratic Theory in the Progressive Period.** Current political debate over the flaws and the fate of the American liberal tradition mirrors closely the critiques of the Progressive period. A number of those thinkers—theorists of "social control," exemplified by Ward and Ross—took aim not at the liberal-utilitarian conception of the ends of politics but rather at the notion that these goods would be produced by the politics of *laissez-faire*. A second body of critics—here termed "communitarians" and including Cooley, Royce, Croly, DuBois, and Dewey—leveled a more fundamental indictment: liberal individualism left men's needs for human sympathy, shared meanings and loyalties, and common effort unfulfilled. But the difficulties of the communitarian theorists, the range of associations on which they pinned their hopes and their fundamental ambivalence concerning politics demonstrate how little in the way of ideological or social-structural reinforcement the environment provided for one who would apply the insights of Burke or Hegel or Tönnies in America. Modern political criticism has resurrected the themes of the Progressive period, but even such bridge-builders as Dahl and McWilliams have not been sufficiently attentive to interrelationships among the persisting critiques of liberalism and to the shape ameliorative communities might assume.

By DAVID E. PRICE, Associate Professor of Political Science and Policy Sciences, Duke University.

1679 COMMUNICATIONS

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By CLEMENT E. VOSE, John E. Andrus Professor of Government, Wesleyan University.

1706 **Political Implications of Gender Roles: A Review of the Literature.** This essay introduces the reader to the contemporary literature on gender roles and feminine behavior, including the major concepts, empirical findings, and social thought which have implications for political behavior and research. Gender roles as they relate to the psychology and activity of men and women, and their systemic cultural, economic, and legal ramifications provide an explanation and a basis for understanding political behavior, including recurrent women's protest movements. Contemporary writing contributes to building a non-androcentric and accurate body of knowledge regarding political woman, and it calls into question the ideology of the biological determinism of political activity. The literature surveyed has potential usefulness for public policy: an expansion of democracy is viable with the discernment and removal of barriers which hinder substantial proportions of women from achieving political leadership and hence participating in authoritative decision making and value allocation. A bibliography of major references is appended.

By WILMA RULE KRAUSS, Assistant Professor of Political Science, Northern Illinois University.

1724 **Political Theory, History of Political Thought and Methodology**

Gail Belaief, *Spinoza's Philosophy of Law.* Douglas Sturm, p. 1724
Norman Birnbaum, *Toward A Critical Sociology.* Guenther Roth, p. 1724
Marlen Blissett, *Politics in Science.* Gene M. Lyons, p. 1726
H. H. Bruun, *Science, Values and Politics in Max Weber's Methodology.* Jerome Gittleman, p. 1727
Werner J. Cahnmann and Rudolf Heberle, eds., *Ferdinand Toennies on Sociology: Pure, Applied and Empirical. Selected Writings.* Donald A. Nielsen, p. 1728
Maurice Cranston and Richard S. Peters, eds., *Hobbes and Rousseau: A Collection of Critical Essays.* C. B. Macpherson, p. 1730
Bernard Crick, *In Defence of Politics*, second edition. James H. Nichols, Jr., p. 1731
Léon Dion, *Société et Politique: La Vie des Groupes. Tome Premier: Fondements de la Société Libérale; Tome second: Dynamique de la Société Libérale;* Rainer Eisfeld, *Pluralismus zwischen Liberalismus und Sozialismus;* and Franz Nuscheler and Winfried Steffani, eds., *Pluralismus: Konzeptionen und Kontroversen.* Henry W. Ehrmann, p. 1733
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James Gouinlock, *John Dewey's Philosophy of Value.* Edward B. Portis, p. 1735
Alfred Grosser, *L'Explication Politique: Une Introduction à l'Analyse Comparative.* Roy Macridis, p. 736
Robert L. Hoffman, *Revolutionary Justice: The Social and Political Theory of P.-J. Proudhon;* and P.-J. Proudhon, *General Idea of the Revolution in the 19th century.* Trans. by John Beverly Robinson. Charles E. Butterworth, p. 1737
Nellie D. Kendall, ed., *Willmoore Kendall Contra Mundum.* Joseph C. Mouledoux, p. 1738
Marion J. Levy, Jr., *Modernization: Latecomers and Survivors.* Lucian W. Pye, p. 1740
John Maguire, *Marx's Paris Writings: An Analysis.* Alfred G. Meyer, p. 1741
Holton P. Odegard, *The Politics of Truth.* Victor A. Thompson, p. 1742
Saul K. Padover, ed., *Karl Marx on Revolution.* Z. A. Jordan, p. 1743
Raymond Plant, *Hegel.* Judith N. Shklar, p. 1744
James L. Price, *Handbook of Organizational Measurement.* Lawrence B. Mohr, p. 1746
Ellis Sandoz, *Political Apocalypse: A Study of Dostoevsky's Grand Inquisitor.* Joseph Frank, p. 1747
William R. Shaffer, *Computer Simulations of Voting Behavior.* Samuel L. Popkin, p. 1747
John A. Sohnquist, *Multivariate Model Building: The Validation of a Search Strategy.* Bernard Grofman, p. 1749
Peter W. Sperlich, *Conflict and Harmony in Human Affairs: A Study of Cross-Pressures and Political Behavior.* Ada W. Finifter, p. 1749
Jack Woddis, *New Theories of Revolution.* Bernard S. Morris, p. 1751

1752 American Government and Politics

Stanley V. Anderson and John E. Moore, eds., *Establishing Ombudsman Offices: Recent Experience in the United States.* William B. Gwyn, p. 1752

John L. Andriot, *Guide to U.S. Publications;* Alexander C. Body, *Annotated Bibliography of Bibliographies on Selected Government Publications and Supplementary Guides to the Superintendent of Documents Classification System;* and Laurence F. Schmeckebier and Roy B. Eastin, *Government Publications and Their Use.* John Brown Mason, p. 1753

James Alden Barber, Jr., *Social Mobility and Voting Behavior.* Jerrold G. Rusk, p. 1754

Sidney Bell, *Righteous Conquest: Woodrow Wilson and the Evolution of the New Diplomacy.* Milton Plesur, p. 1755

Alexander M. Bickel, *Reform and Continuity: The Electoral College, the Convention, and the Party System;* Judith H. Parris, *The Convention Problem: Issues in Reform of Presidential Nominating Procedures;* and Neal R. Peirce, *The People's President: The Electoral College in American History and the Direct-Vote Alternative.* Charles Press, p. 1756

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Jules Cohn, *The Conscience of the Corporations: Business and Urban Affairs, 1967–1970.* Joseph Smucker, p. 1760

Daniel J. Elazar, *The Politics of Belleville: A Profile of the Civil Community.* Phillip Monypenny, p. 1761

James M. Fallows, *The Water Lords.* Dean E. Mann, p. 1762

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H. Paul Friesema, *Metropolitan Political Structure: Intergovernmental Relations and Political Integration in the Quad-Cities.* Francis M. Wilhoit, p. 1765

David R. Godschalk, *Participation, Planning, and Exchange in Old and New Communities: A Collaborative Paradigm.* Judith V. May, p. 1766

Virginia Van Der Veer Hamilton, *Hugo Black: The Alabama Years.* R. H. Clark, p. 1767

Jerome Johnston and Jerald G. Backman, *Young Men Look At Military Service: A Preliminary Report;* and Charles C. Moskos, Jr., ed., *Public Opinion and the Military Establishment.* James M. Gerhardt, p. 1768

John C. Leggett, *Race, Class, and Political Consciousness.* Michael Cohen, p. 1770

Beverly Bowen Moeller, *Phil Swing and Boulder Dam.* Daniel M. Ogden, Jr., p. 1771

Russell D. Murphy, *Political Entrepreneurs and Urban Poverty.* Louis F. Weschler, p. 1771

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Jerome Mushkat, *Tammany: Evolution of a Political Machine, 1789–1865.* Joel A. Tarr, p. 1773

Stuart S. Nagel, ed., *The Rights of the Accused.* David Fellman, p. 1774

William Niskanen, Jr., *Bureaucracy and Representative Government.* William C. Mitchell, p. 1775

Wallace E. Oates, *Fiscal Federalism.* Deil S. Wright, p. 1777

Robert D. Reischauer and Robert W. Hartman, with Daniel J. Sullivan, *Reforming School Finance.* David Stern, p. 1778

Bruce M. Stave, *The New Deal and the Last Hurrah: Pittsburgh Machine Politics.* William C. Seyler, p. 1779

Otis H. Stephens, Jr., *The Supreme Court and Confessions of Guilt.* David Chadwick-Brown, p. 1780

Anselm L. Strauss, *The Contexts of Social Mobility: Ideology and Theory.* Edward O. Laumnan, p. 1781

1783 Comparative Government and Politics

Simon Abbott, ed., *The Prevention of Racial Discrimination in Britain:* Benjamin W. Heineman, Jr., *The Politics of the Powerless: A Study of the Campaign Against Discrimination;* and Anthony Lester and Geoffrey Bindman, *Race and Law in Great Britain.* Richard P. Longaker, p. 1783

Heribert Adam, ed., *South Africa: Sociological Perspectives.* Edwin S. Munger, p. 1785

Don Aitkin, *The Country Party in New South Wales: A Study of Organisation and Survival.* D. W. Rawson, p. 1786

Klaus R. Allerbeck and Leopold Rosenmayr, *Aufstand der Jugend? Neue Aspekte der Jugendsoziologie;* Werner Klose, *Lebensformen Deutscher Jugend, vom Wandervogel zur Popgeneration;* and Peter Menacher, *Jugendliche und Parteien.* Peter H. Merkl, p. 1788

Miguel Arraes, *Brazil: The People and the Power.* Trans. by Lancelot Sheppard. Ronald H. Chilcote, p. 1789

Michael Banton, *Racial Minorities;* and Ernest Q. Campbell, ed., *Racial Tensions and National Identity. Proceedings of the Second Annual Vanderbilt Sociology Conference, November 4–6, 1970.* Jonathan Pool, p. 1791

Ian Bellany, *Australia in the Nuclear Age: National Defence and National Development.* Harry G. Gelber, p. 1792

James Alban Bill, *The Politics of Iran: Groups, Classes and Modernization.* Sepehr Zabih, p. 1793

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Leon Goure, *The Military Indoctrination of Soviet Youth.* Jaan Pennar, p. 1808
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Paul Hollander, *Soviet and American Society: A Comparison.* Zygmunt Bauman, p. 1811
Gerrit Huizer, *The Revolutionary Potential of Peasants in Latin America.* Enrique Baloyra, p. 1812
Barbara Wolfe Jancar, *Czechoslovakia and the Absolute Monopoly of Power: A Study of Political Power in a Communist System.* Galia Golan, p. 1813
Raouf Kahil, *Inflation and Economic Development in Brazil, 1946–1963.* Margaret Daly Hayes, p. 1815
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Abraham Katz, *The Politics of Economic Reform in the Soviet Union.* Frederic L. Pryor, p. 1817
Majid Khadduri, *Arab Contemporaries: The Role of Personalities in Politics;* and Majid Khadduri, *Political Trends in the Arab World: The Role of Ideas and Ideals in Politics.* Iliya Harik, p. 1818
R. S. Khare, *The Changing Brahmans: Associations and Elites among the Kanya-Kubjas of North India.* Roderick Church, p. 1819
S. Clement Leslie, *The Rift in Israel: Religious Authority and Secular Democracy.* Leonard J. Fein, p. 1820
Sven Lindqvist, *The Shadow: Latin America Faces the Seventies.* Trans. by Keith Bradfield. W. Raymond Duncan, p. 1821
Robin Luckham, *The Nigerian Military: A Sociological Analysis of Authority and Revolt, 1960–67.* Grady H. Nunn, p. 1822
Ronald I. McKinnon, *Money and Capital in Economic Development.* W. W. Rostow, p. 1822
Ian Mackler, *Pattern for Profit in Southern Africa.* Julian R. Friedman, p. 1824
Yogendra K. Malik, *East Indians in Trinidad: A Study in Minority Politics.* H. Hoetink, p. 1825
James M. Malloy, *Bolivia: The Uncompleted Revolution.* Robert J. Alexander, p. 1825
Richard H. Marshall, Jr., ed.; Thomas E. Bird and Andrew Q. Blane, assoc. eds., *Aspects of Religion in the Soviet Union, 1917–1967.* John A. Armstrong, p. 1827
Mya Maung, *Burma and Pakistan: A Comparative Study of Development.* Frank N. Trager, p. 1828
Hla Myint, *Southeast Asia's Economy: Development Policies in the 1970's.* H. W. Singer, p. 1830
Ikenna Nzimiro, *Studies in Ibo Political Systems: Chieftaincy and Politics in Four Niger States.* Alvin Magid, p. 1831
James Petras and Hugo Zemelman Merino, *Peasants in Revolt: A Chilean Case Study, 1965–1971.* Alan Angell, p. 1832
David H. Pinkney, *The French Revolution of 1830.* John Dunn, p. 1833
Christian P. Potholm, *Swaziland: The Dynamics of Political Modernization.* Sheridan W. Johns, III, p. 1833
Gustav Ranis, ed., *Government and Economic Development.* Bela Balassa, p. 1834
Charles Price Ridley, Paul H. B. Godwin, and Dennis J. Doolin, *The Making of a Model Citizen in Communist China.* Franklin W. Houn, p. 1835
Richard A. H. Robinson, *Origins of Franco's Spain: The Right, The Republic and Revolution, 1931–1936.* Glen A. Waggoner, p. 1836
Ronald M. Schneider, *The Political System of Brazil: Emergence of a "Modernizing" Authoritarian Regime, 1964–1970.* Riordan Roett, p. 1837
J. F. Short, Jr. and M. E. Wolfgang, eds., *Collective Violence.* N. Lucas, p. 1839
Milton Singer, *When a Great Tradition Modernizes: An Anthropological Approach to Indian Civilization.* Richard L. Park, p. 1840

Stockholm International Peace Research Institute, *Resources Devoted to Military Research and Development: An International Comparison.* Barry M. Blechman, p. 1841
Andrew Strathern, *The Rope of Moka: Big-Men and Ceremonial Exchange in Mount Hagen, New Guinea.* Ralph R. Premdas, p. 1842
Virginia Thompson and Richard Adloff, *The French Pacific Islands: French Polynesia and New Caledonia.* Francis West, p. 1843
Ger van Roon, *German Resistance to Hitler: Count von Moltke and the Kreisau Circle.* Trans. by Peter Ludlow. Walter Struve, p. 1844
Benoit Verhaegen, *Rébellions au Congo.* Rene Lemarchand, p. 1845
Malcolm T. Walker, *Politics and the Power Structure: A Rural Community in the Dominican Republic* G. Pope Atkins, p. 1846
Theodore Zeldin, *France, 1848–1945, Vol. I: Ambition, Love and Politics.* Eugen Weber, p. 1847

1848 International Politics, Law and Organization

Hadley Arkes, *Bureaucracy, the Marshall Plan, and the National Interest.* Thomas R. Wolanin, p. 1848
Peter Baehr, *The Role of a National Delegation in the General Assembly.* John O. Lindell, p. 1849
Bryan Bean, trans., *Problems of War and Peace: A Critical Analysis of Bourgeois Theories.* Hedley Bull, p. 1850
W. R. Böhning, *The Migration of Workers in the United Kingdom and the European Community.* Dennis Brooks, p. 1851
Alastair Buchan, *Power and Equilibrium in the 1970s.* Robert L. Rothstein. p. 1852
Oliver J. Caldwell, *A Secret War: Americans in China, 1944–1945;* John Paton Davies, Jr., *Dragon by the Tail;* Chin-tung Liang, *General Stilwell in China, 1942–1944: The Full Story;* Roderick MacFarquhar, ed., *Sino-American Relations, 1949–1971;* and Kenneth E. Shewmaker, *Americans and Chinese Communists, 1927–1945: A Persuading Encounter.* Allen S. Whiting, p. 1853
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David Davies Memorial Institute of International Studies, *International Disputes: The Legal Aspects.* A. J. R. Groom, p. 1857
Robert A. Divine, *Roosevelt and World War II.* Arnold A. Offner, p. 1858
Glen H. Fisher, *Public Diplomacy and the Behavioral Sciences.* Richard A. Rettig, p. 1859
James W. Gould, *The United States and Malaysia.* Robert O. Tilman, p. 1860
Charles F. Hermann, *International Crises: Insights from Behavioral Research.* P. Terrence Hopmann, p. 1861
Alfred O. Hero, Jr., *American Religious Groups View Foreign Policy: Trends in Rank-and-File Opinion, 1937–1969.* Kenneth Thompson, p. 1863
Aaron S. Klieman, *Soviet Russia and the Middle East.* Amos Perlmutter, p. 1864
E. Lauterpacht, ed., *International Law: Collected Papers of Hersch Lauterpacht; Vol. I: General Works.* Louis B. Sohn, p. 1865
Leon N. Lindberg and Stuart A. Scheingold, eds., *Regional Integration: Theory and Research.* David P. Calleo, p. 1867
Hsu Long-hsuen and Chang Ming-kai, compilers, *History of the Sino-Japanese War, 1937–1945.* John H. Boyle, p. 1869
Margery Perham, *Colonial Sequence, 1930 to 1949: A Chronological Commentary upon British Colonial Policy, Especially in Africa;* and *Colonial Sequence, 1949 to 1969: A Chronological Commentary upon British Colonial Policy in Africa.* L. H. Gann, p. 1870
Stefan T. Possony and J. E. Pournelle, *The Strategy of Technology: Winning the Decisive War.* George H. Quester, p. 1872
Rouhollah K. Ramazani, *The Persian Gulf: Iran's Role.* R. M. Burrell, p. 1873
W. W. Rostow, *The Diffusion of Power, 1957–1972: An Essay in Recent History.* Lincoln P. Bloomfield, p. 1874
August Schou and Arne O. Brundtland, eds., *Small States in International Relations, Nobel Symposium 17.* Nils Ørvik, p. 1875
Harvey Sicherman, *Aden and British Strategy, 1839–1968.* Aaron S. Klieman, p. 1877
Marshall R. Singer, *Weak States in a World of Powers.* Annette Baker Fox, p. 1877
Joseph M. Siracusa, *New Left Diplomatic Histories and Historians: The American Revisionists.* Ole R. Holsti, p. 1878
Harvey Starr, *War Coalitions: The Distributions of Payoffs and Losses.* James Lee Ray, p.1880
William C. Stinchcombe, *The American Revolution and the French Alliance.* Alexander DeConde, p. 1881
Alexander Szalai with Margaret Croke and Associates, *The United Nations and the News Media.* Leon Gordenker, p. 1881
Christos Theodoulou, *Greece and the Entente: August 1, 1914–September 25, 1916.* D. George Kousoulas, p. 1883
Richard L. Walker, ed., *Prospects in the Pacific.* Willard H. Elsbree, p. 1883
Janet Zeigler, compiler, *World War II: Books in English, 1945–65.* Barton Whaley, p. 1884

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EDITORIAL COMMENT

Happy Anniversary. Professor Herbert Weisberg sentimentally passed up the chance to get "Models of Statistical Relationship" published earlier so that it could appear in this issue, twenty years to the month after the classic article by Leo Goodman and William Kruskal on which it is based.

A pang of envy frequently attacks us when we contemplate the greener and better tended forage available to "harder" disciplines (like statistics), where scholars can actually build upon one another's work, where agreement can be reached about what the problems are, and where consequently there can be such things as "classic" articles, statements which productively focus the attention of many minds and which may be superseded, but are rarely misunderstood.

As little as this thumbnail sketch describes political science, it did occur to us to cast a backward glance at the *Review* of 20 years ago. Would we find anything at all in Volume 48 worth noting today? Any memorable articles, never mind classics? Somehow, we doubted it. Political science lurches from fad to fad, we thought, ignores its roots, cultivates illiteracy. There would be nothing of contemporary interest, only evidence of false starts, blind alleys, brave futile beginnings. We were wrong! Managing Editor Hugh L. Elsbree presided over what even the most jaded graduate student can recognize as a cornucopia. Not the least interesting was Ralph J. Bunche's Presidential Address, marking the 50th anniversary of the Association. But there was much, much more.

In those days, it appears, political scientists had not yet lost their taste for politics and government. Bertram Gross and John Lewis discussed the beginnings of the Council of Economic Advisors. Richard Neustadt contributed his notable and enduring "Presidency and Legislation: The Growth of Central Clearance." Ralph Huitt's "The Congressional Committee: A Case Study" appeared. Happy Anniversary, one and all. In comparative government Volume 48 features (among others) a bibliography on comparative administration by Fred Riggs, an article on Asian neutralism by Robert Scalapino, and a discussion of Great Britain by Leon Epstein.

In party politics there were articles by V. O. Key, Duane Lockard and that odd couple, Ranney and Kendall. David Spitz contributed "Democracy and the Problem of Civil Disobedience"; Sheldon Wolin, "Hume and Conservatism" and Emmette Redford "Administrative Regulation: Protection of the Public Interest."

Henry Kissinger shed his warm-up jacket with "The Conservative Dilemma: Reflections on the Political Thought of Metternich." And there was even something for premature formal theorists: Shapley and Shubik's "A Method for Evaluating the Distribution of Power in a Committee System."

Most of us have seen citations to one or more of these articles within living memory; whether "classics" or not, evidently they have had something to say to political scientists that has been more than ephemeral. And this is why a subscriber would have been ill-advised to wrap fish with old copies of volume 48 of the *Review*.

Can we say as much of more recent volumes? Readers had better decide for themselves. For ourselves, we are reminded of the time we served on a committee to hire a sociology department. One committee member, a very dubious historian, said: "Let's get somebody like Robert MacIver." We said, "O.K., let's." The historian replied: "They don't make them like that anymore."

Emily Post Says. Although norms concerning multiple submissions and appropriation of the work of others are well settled in our profession—and indeed in the scholarly community generally—an occasional slipup comes to our attention, suggesting that it may be worthwhile to describe some of the underlying rationale of *Review* policy for people who may have tuned in late.

Each year, scholarly journals ask scores of scholars to set aside their own work in order to evaluate the merit of articles proffered for publication. An editor of the *Review* has reason to know how many scholars—they are legion—do these unpaid, anonymous chores willingly, graciously, even promptly. This creates an obligation on our part, at the least to avoid bothering our referees frivolously or in vain.

When an author submits the same manuscript simultaneously to two or more unwitting editors, he is asking them to become his accomplices in wasting the time of referees. For only one journal can print the manuscript. Consequently the courteous thing to do is to submit articles to journals one at a time, and hope for the best.

In these harsh days of tenure squeezes and publish or perish, there must be a strong temptation for authors to cut down on the agony of waiting by trying a buckshot approach to all journals at once. That constitutes an imposition on a lot of colleagues, however, and journal editors, fortified by an advisory opinion of the APSA ethics committee, take a dim view of multiple submissions.

An even more straightforward rationale underlies the APSA policy concerning the reprinting of articles. Our position for years has been that persons wanting to reprint an article must seek and receive the permission of the scholar who did the work, before the Association, as copyright holder, will grant its permission to reprint. The technicalities of copyright law may or may not support the Association's scrupulousness on this issue; nevertheless to us it seems manifestly reasonable that authors retain control over the reprinting of their work, and the Association acts accordingly. In this, we consider that we are following a well-understood, if unwritten rule of the profession, and cannot imagine, in any case, that a political scientist seeking to reprint the work of a colleague would dream of neglecting to ask the author's permission.

Readers will also be gratified to learn that we oppose plagiarism (as well as piracy) and think well of springtime. But enough of controversy.

On Scholarly Infallibility. We see by the papers that Washington columnist Joseph Kraft says political science is in big trouble. He doesn't know the half of it, we thought, until we read a little farther and discovered he didn't know the other half of it, either. His argument goes this way: because the state of our knowledge about voting behavior is unsettled, and because the nature of voter commitment to political parties in America is changing, political science has "lost its way."

Many political scientists will testify that they weren't heading in that direction anyhow, that some folks in Our Nation's Capital must have a mighty narrow idea of what political science is. What puzzles us even more is this cultivated and well-informed layman's peculiar conception of the calling of the scholar. And if Joseph Kraft has such a misconception, heaven knows how many others do as well.

It is our duty, it seems to us, less to celebrate past victories over ignorance than continually to confront what we don't know, to grapple with the anomalous, the imperfectly understood. The existence of perplexity in a scholar is, we believe, an unmistakable sign of life (maybe the only sign of life) and in a discipline, likewise.

This leaves open—as it had better—the question of our competence, individually or collectively, to focus our inquiries, to organize our assaults upon our ignorance with skill and sensitivity. On this score people are entitled to have their doubts. But not on the question Mr. Kraft raises. Scholars are not oracles. Our commitment is to inquiry, not to final solutions, and it is amazing that a Washington pundit would want us crowding his territory.

Articles Accepted for Future Publication

Christopher H. Achen, Yale University, "Political Belief Systems in Mass Publics: The Problem of Inconsistent Opinion Survey Responses"

Francisco Arcelus and Allan H. Meltzer, Carnegie-Mellon University, "The Effect of Aggregate Economic Variables on Congressional Elections"

C. Arnold Anderson, University of Chicago, "Conceptual Framework for Political Socialization in Developing Societies"

Neal Andrews, Wayne State University, "Integration and Community in Communist Theory"

Robert L. Ayres, University of California, Berkeley, "Development Policy and the Possibility of A 'Liveable' Future for Latin America"

Richard Allen Chapman, University of Montana, "*Leviathan* Writ Small: Thomas Hobbes on the Family"

John P. Clark, III, City College, Loyola University, "On Anarchism in an Unreal World: Kramnick's View of Godwin and the Anarchists"

Claude S. Colantoni, Terrence J. Levesque and Peter C. Ordeshook, Carnegie-Mellon University, "Campaign Resource Allocations Under The Electoral College"

David Collier, Indiana University, and Richard E. Messick, Office of U.S. Senate, "Functional Prerequisites Versus Diffusion: Testing Alternative Explanations of Social Security Adoption"

Andrew T. Cowart, University of Iowa, Tore Hansen and Karl-Erik Brofoss, University of Oslo, "Budgetary Strategies and Success at Multiple Decision Levels in the Norwegian Urban Setting"

Geoffrey Debnam, University of Otago, "Nondecisions and Power: The Two Faces of Bachrach and Baratz"

Douglas Dobson, Northern Illinois University and Douglas St. Angelo, Florida State University, "Party Identification and the Floating Vote: Some Dynamics"

Claude S. Fischer, University of California, Berkeley, "The City and Political Psychology"

Robert C. Fried, University of California, Los Angeles, "Party and Policy in West German Cities"

Richard Funston, San Diego State University, "The Supreme Court and Critical Elections"

Sheldon Goldman, University of Massachusetts, Amherst, "Voting Behavior on the U.S. Courts of Appeals Revisited"

Fred I. Greenstein, Princeton University, "The Benevolent Leader Revisited: Children's Images of Political Leaders in Three Democracies"

Fred W. Grupp, Jr., University of Connecticut and Allan R. Richards, Louisiana State University, "Variations in Elite Perceptions of American States as Referents for Public Policy Making"

Susan Blackall Hansen, University of Illinois, Urbana, "Participation, Political Structure, and Concurrence"

K. J. Holsti, University of British Columbia, "Underdevelopment and the 'Gap' Theory of International Conflict"

Robert T. Holt and John E. Turner, University of Minnesota, "Crises and Sequences in Collective Theory Development"

M. Kent Jennings, University of Michigan and Richard G. Niemi, University of Rochester, "Continuity and Change in Political Orientations: A Longitudinal Study of Two Generations"

Jae-on Kim, University of Chicago, John R. Petrocik, University of Chicago and Stephen N. Enokson, University of Iowa, "Voter Turnout Among the American States: Systemic and Individualistic Components"

David Koehler, American University, "Vote Trading and the Voting Paradox: A Proof of Logical Equivalence"

J. A. Laponce, University of British Columbia, "Spatial Archetypes and Political Perceptions"

David G. Lawrence, Fordham University, "Procedural Norms and Tolerance: A Reassessment"

Peter M. Leslie, Queen's University, "Interest Groups and Political Integration: The 1972 EEC Decisions in Norway and Denmark"

Patrick J. McGowan, Syracuse University and Robert M. Rood, University of South Carolina, "Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to 19th-Century Europe"

Alan Marsh, University of Michigan, "The 'Silent Revolution,' Value Priorities, and the Quality of Life in Britain"

Joseph A. Massey, Dartmouth College, "The Missing Leader: Japanese Youths' View of Political Authority"

Kenneth John Meier, Syracuse University, "Representative Bureaucracy: An Empirical Analysis"

Fritz Nova, Villanova University, "Political Innovation of the West German Federal Constitutional Court: The State of Discussion on Judicial Review"

N. Patrick Peritore, University of Missouri, Columbia, "Some Problems in Alfred Schutz's Phenomenological Methodology"

Adam Przeworski, University of Chicago, "Institutionalization of Voting Patterns or Is Mobilization the Source of Decay?"

Douglas Rae, Yale University, "The Limits of Consensual Decision"

Austin Sarat, Yale Law School, and Joel B. Grossman, University of Wisconsin, Madison, "Courts and Conflict Resolution: Problems in the Mobilization of Adjudication"

Joseph A. Schlesinger, Michigan State University, "The Primary Goals of Political Parties: A Clarification of Positive Theory"

Paul R. Schulman, University of Tennessee, "Non-Incremental Policy Making: Notes Toward an Alternate Paradigm"

J. S. Sorzano, Georgetown University, "David Easton and the Invisible Hand"

Peter G. Stillman, Vassar College, "The Limits of Behaviorism: A Critique of B. F. Skinner's Social and Political Thought"

Gerald S. Strom, University of Illinois, Chicago, "On the Apparent Paradox of Participation"

Edward R. Tufte, Princeton University, "Determinants of the Outcome of Midterm Congressional Elections"

Eric M. Uslaner and J. Ronnie Davis, University of Florida, "The Paradox of Vote Trading: Effects of Decision Rules and Voting Strategies on Externalities"

Meredith W. Watts, University of Wisconsin, Milwaukee, "B. F. Skinner and the Language of Technological Control"

J. Weinberger, Michigan State University, "Hobbes's Doctrine of Method"

Mary B. Welfling, Virginia Polytechnic Institute and State University, "Models, Measurement and Sources of Error: Civil Conflict in Black Africa"

Persistence and Change in Political Systems, 1800–1971*

TED ROBERT GURR
Northwestern University

The "stability" of political systems has been attributed to factors as diverse as modal personalities, popular well-being, and a chief executive's rights with respect to a hostile assembly. The "constitutional engineering" approach which prevailed from World War I through the 1940s was largely discredited by the failure of engineered democratic experiments in many of the "new nations," and in some old ones like Weimar Germany. In the 1950s, the search for political explanations of political stability lost favor to socioeconomic and environmental explanations. The effects on stability of productivity, mass education and communication, cultural homogeneity, "cross-cutting cleavages," and, most recently, international inequalities and dependencies have now been studied, with results less than wholly satisfying. "Political" explanations again are being adduced and tested, but two of the major approaches, those of "political culture" and "political performance," have the faint but distinct odor of tautology about them because the antecedent conditions of the "good polity" are often indistinguishable, definitionally or operationally, from their claimed effects.

This paper does not take direct issue with other approaches, but it does attempt to deal afresh with a perennial question of political analysis: whether the durability (persistence and adaptability) of political systems depends at all upon the nature of their structures of political authority. One of the questions raised, and tested, is whether typological traits like "democracy" and "autocracy" predict consistently to durability. A second question is whether such universal traits as the "institutionalization" or internal coherence of authority structures affect durability, and if so, whether the universal traits are more or less important than typological ones. This study's answers to these questions also suggest answers to some hoary debates, for example whether active or passive polities fare better, and whether systems with "mixed" authority characteristics have better survival prospects than "pure types."

One distinctive feature of this study is the uniquely broad empirical base used to assess such questions. National systems of all types are included: emirates, parliamentary democracies, peoples' democracies, and all the rest. Moreover the universe of analysis encompasses most national states of the 19th and 20th centuries, including many which no longer exist. The only significant exclusions are the "new nations" established since World War II. Thus the authority characteristics of national political systems, and their durability, can be examined separately by world region and in different historical eras.[1] And the incidence and broad patterns of structural change in those systems can be mapped on a worldwide basis, during the seven or eight generations since the American and French Revolutions.

The first part, below, discusses some basic concepts of the study. Part II surveys the procedures used to measure political authority characteristics and summarizes the coded data. The next part similarly reports on the "durability" data and includes "life expectancy" tables for polities. The final section presents and tests three hypotheses which link authority characteristics to durability. Numerous other hypotheses about the determinants of durability, which can be tested against the same data, will be evaluated in future papers.

Assumptions and Concepts

The study has no implicit ideological axes to grind. I do not assume that political systems which are durable are necessarily the better for

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[1] The new nations are very numerous and too short-lived to warrant definitive judgments about the durability of their political systems. Analysis of polities in older nations should provide a baseline of generalizations which can be used later to assess the prospects of the new nations. The numbers of states included, by world region, are:

Europe	49[a]
Central and South America	23
Africa	4
Asia	15

[a] Including Australia, Canada, New Zealand, the Orange Free State, South Africa, and the United States.

being so, though large portions of mankind have evidently valued political continuity over discontinuity. Nor do I hope to demonstrate that "democratic," or "autocratic," or highly competitive political systems are more or less good than others. Rather, my central concern is whether any of the prevailing scholarly and popular beliefs about the viability of particular kinds of national political arrangements have historical or cross-cultural validity.

Authority and Authority Patterns. The meaning of "authority" in this study requires clarification. I am not concerned with the *extent* of political systems' "authority," either in the sense of legitimacy as understood by Weber or in the sense of the amount of control or coercion they exercise. The object of this inquiry is polities' patterns of authority as defined by Eckstein: an authority pattern is a "set of asymmetric interactions among hierarchically ordered members of social units that involve the direction of the unit."[2] More loosely, the state's authority pattern is the set of structures and processes through which directives applicable to members of the state are made, issued, and enforced.

We have specified elsewhere a set of 12 basic dimensions which can be used to characterize and compare the authority patterns of any social units.[3] The political systems studied here are characterized on five of these dimensions:

(1) The openness of *Recruitment* to superordinate positions. Recruitment is "open" to the extent that prevailing modes of advancement give subordinates equal opportunities to become superordinates. This study focuses on the modes by which executives are recruited.

(2) The extent of *Decision Constraints* on superordinates, indexed in this study by the extent of limitations on the decision-making powers of the chief executive.[4]

(3) The amount of *Participation*, which is high to the extent that there is a large volume of influence attempts by subordinates via open channels. The operational focus is on the extent and openness of political competition and opposition.

(4) The *Directiveness* of the social unit, which is high to the extent that a wide scope of members' activities is closely regulated and sanctions frequently imposed for violations. The Directiveness of polities is evaluated in this study by reference to the scope of governmental regulation of societal functions.

(5) The *Complexity* of decision structures, indexed here by the plurality of top decision-making structures and the degree of governmental decentralization.

Polities. The unit of analysis in this study is not the nation-state or national political community as such but rather the *polity* or political system: that is, the basic political arrangements by which national political communities govern their affairs. These systems are described in terms of the above five dimensions of authority patterns. When a system changes abruptly and substantially on one or more of these dimensions the change is treated as the end of one polity and the beginning of another. A very few nations, including Britain and Costa Rica, had the "same" gradually-changing polity throughout the era in which they are studied here. In contrast, some nations experimented with more than 10 successive political systems; Greece and Guatemala are examples. This study is based on data for 336 polities which functioned in 91 nominally-different states at some time during the 19th or 20th centuries or both.

Political Stability and Durability. Political stability is ordinarily discussed in empirical political science as a valued condition. The same is true of the now- more-fashionable term which has partly replaced it, "political performance." We speak of the "stability" and "performance" of political systems in ways analogous to physicians' concern with the health of a patient and economists' evaluations of the efficiency of an enterprise. But, whereas physicians are largely agreed about the characteristics of healthy people, there is much inconsistency and disagreement about what constitute the "vital signs" of political systems. With reference to "performance," Eckstein has argued for four all-but-axiomatic dimensions: the political system which performs well must have some degree of efficacy, legitimacy, civil order, and durability.[5] The empirical literature on political

[2] Harry Eckstein, "Authority Patterns: A Structural Basis for Political Inquiry," *American Political Science Review*, 47 (December, 1973), pp. 1153. For a survey of conventional academic definitions of "authority" see Robert L. Peabody, *Organizational Authority* (New York: Atherton Press, 1964), chap. 2.

[3] In Harry Eckstein and Ted Rober Gurr, *Patterns of Authority: A Structural Basis For Political Inquiry* (New York: Wiley-Interscience, forthcoming), Part II.

[4] At the time this study was begun we treated Decision Constraints as a simple and distinct dimension. We later came to regard it as a synthetic concept, i.e., a multiple function of a number of other dimensions. In either conceptualization it is a dimensional concept and can be used, as it is here, to characterize and distinguish among political entities.

[5] Harry Eckstein, *The Evaluation of Political Performance: Problems and Dimensions* (Beverly Hills: Sage Professional Papers in Comparative Politics, 1971), Vol. 2, No. 01–017. Gabriel Almond has identified five categories of system performance, which

stability exhibits an even wider range of relevant phenomena. Hurwitz has recently pointed to five different views of political stability, in terms of (1) the existence of a legitimate constitutional regime; (2) the relative absence of violent civil conflict; (3) the absence of structural change in the political system; (4) the relative longevity of "governments," especially executives and cabinets; and (5) the presence of multiple societal attributes, often including economic and social well-being as well as more narrowly political variables.[6]

Some of the conceptual and operational diversity with which stability is treated arises from unresolved questions about what are the "most important" facets of political systems. The lack of agreement on what is "most important" reflects, first, a normative condition: Western political analysts value different characteristics of political systems. Second, it reflects a lack of paradigmatic agreement about what conditions are essential to the proper functioning of political systems. The common emphasis on civil strife as the opposite of political stability, for example, reflects a premise that civil order is a paramount political variable—a view that can be rationalized either on the evaluative grounds that civil order is an ultimate good, or by the functional argument that a substantial degree of civil order is a requisite for the effective operation of a polity.

This study rests on the premise that the most important feature of any political system, for purposes of description, is its pattern of authority relations. A "stable" political system, on this premise, is one whose authority patterns remain similar over a long period of time. The most enduring systems, though, are not necessarily those which are rigidly resistant to change. Three of the most long-lived and change-resistant polities identified in this study are Czarist Russia, the Ottoman Empire, and China under the Manchu Dynasty, each of which collapsed utterly under the impact of internal and external crises, to be replaced by wholly new political systems. The most durable political systems, in my view (and also from the perspective of the "general systems" literature[7]) are not merely persistent but also demonstrate a capacity to adapt more or less gradually in response to internal and environmental stress. English parliamentary democracy is the most long-lived "adaptable" polity among the 336 cases, and at its present age of 280+ (dating from ca. 1689) it is younger than were either Czarist Russia or the Ottoman Empire when they collapsed. The most salient feature of the English system, however, is that it has undergone a fundamental change *of* system during the last two centuries nearly as great as the system change from pre- to post-revolutionary Russia, and it has done so through an incremental series of changes *in* its system, i.e., changes that at no point were large and abrupt enough to warrant the conclusion that a "new" polity had come into being.

Durability is the term used here for the distinguishing property of systems which both persist and adapt. It is a variable property, of course, and is judged according to the historical experience of a polity. Note that durability at time x conveys no necessary implications about future durability. The future prospects of any contemporary polity, evaluated now, are presumably a function of a number of traits and conditions of the sort which this study attempts to identify. Conceptually durability is a single property. It has two separable aspects, however, which are sufficiently different, operationally and empirically, that they are treated separately throughout this study.

(1) *Persistence* is defined simply as longevity: it is the length of time a political system endures without major, abrupt change in its authority pattern. In a previous empirical study of political performance I also dealt with polities' persistence, reporting evidence from a small set of cases that persistence is closely correlated with, and substantially determined by, other aspects of performance, notably decisional efficacy, legitimacy, and civil peace.[8] The present study differs in these important respects: it makes conceptual and operational distinctions between persistence and adaptability; it uses more comprehensive data on authority for a much larger universe of cases; and it attempts to relate authority characteristics directly to durability.

(2) *Adaptability* refers here to the extent of a political system's demonstrated capacity for undergoing incremental change in its authority pattern. This definition requires a brief justification. In a general sense, adaptability signifies a capacity of people and institutions to cope with various

have to do with how well such functions as regulation, extraction, and distribution are performed; see his "Political Development: Analytical and Normative Perspectives," *Comparative Political Studies,* 1 (January, 1969), pp. 460–467.

[6] See Leon Hurwitz, "Contemporary Approaches to Political Stability: A Review Essay," *Comparative Politics,* 5 (April, 1973), pp. 449–463.

[7] See, for example, Walter Buckley, *Sociology and Modern Systems Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1967) and W. Ross Ashby, *Introduction to Cybernetics* (New York: Wiley, 1956).

[8] Ted Robert Gurr and Muriel McClelland, *Political Performance: A Twelve-Nation Study* (Beverly Hills: Sage Professional Papers in Comparative Politics, 1971, No. 018), pp. 72–85. The dependent variable of this earlier study was labeled Durability but was defined as Persistence is in this study.

kinds of stresses. Scholars might reasonably define the adaptability of political systems in such terms. But there is an operational argument against doing so. In some of our previous work on political performance Harry Eckstein and I encountered prohibitive operational difficulties when attempting to evaluate political systems' responses to stress: stresses are numerous and incommensurate, and viable responses to them vary with situations.[9] There is also a theoretical argument for focusing on changes *in* the system. A polity which gradually changes itself in response to stress demonstrates a higher order of what we mean by adaptability than one which suppresses or survives the effects of stress without change. This is analogous to the argument in the general systems literature that an adapting system is one whose parameters change in response to environmental change. It is a considerable accomplishment for political systems to deal with, say, economic malaise or civil war, or crises of integration or of distribution, without themselves changing. But a higher order of adaptability is evidenced by polities which cope with change partly by changing what they do *and* partly by changing aspects of themselves.

Measuring Characteristics of Political Authority

Categories and Measures. The literature of comparative politics offers many conventional categories and variables for the description of authority patterns, few of which correspond precisely to the 12 dimensions of authority specified in the analytic scheme Eckstein and I developed.[10] What we conceptualize as the "openness of Recruitment," for example, is more commonly described by reference to the prevalence of competitive election, appointment, cooptation, ascription, seizure of power, and so forth. For this study our first step was to identify and define briefly some of these conventionally-categorized properties of political systems, concentrating on those which (a) were regularly referred to in scholarly analyses of historical and contemporary polities, and (b) could be related, at least loosely, to some of the dimensions of the analytic scheme. The properties on which we finally settled are listed in Table 1.

A coder tested the workability of operational definitions for an initial set of such categories by applying them to source materials on a few countries, including some which were intrinsically difficult to analyze. The categories were successively redefined on the basis of a series of trial runs. Some were eliminated, others added, operational definitions were revised, concrete examples

Table 1. Operational Definitions of Authority Dimensions

1.1 *Openness of Executive Recruitment*

Type of recruitment	*Openness score*
Competitive	5
Caesaristic	4
Dual executive	3
Designation	2
Ascription	1

1.2 *Decision Constraints on the Chief Executive*

Extent of constraints	*Constraints score*
Coequal with or subordinate to legislature	4
Substantial limits	3
Some limits	2
Unlimited	1

1.3 *Extent of Political Participation*

Pattern of participation	*Participation score*
Institutionalized	5
Factional/restricted	4
Uninstitutionalized	3
Restricted	2
Suppressed or nonexistent	1

1.4 *Directiveness: Scope of Governmental Control*

Scope of control	*Directiveness score*
Minimal	5
Segmental minus	4
Segmental	3
Segmental plus	2
Totalitarian	1

1.5 *Complexity of Governmental Structures*

a) *Executive characteristics*	*Weight*
Collective	4
Qualified individual	3
Pure individual	1
b) *Decision constraints*	*Weight*
Coequal	4
Substantial limits	3
Some limits	2
Unlimited	1
c) *Centralization*	*Weight*
Decentralized	4
Centralized	1

Complexity score = a + b + c (range 3 to 12)

[9] Some of these problems are discussed by Ted Robert Gurr, "Vergleichende Analyse von Krisen und Rebellionen," in Martin Janicke, ed., *Herrschaft und Krise* (Opladen: Westdeutscher Verlag, 1973), pp. 68–69, and by Eckstein, *The Evaluation of Political Performance*, pp. 69–73. Other approaches to crises and crisis-response are to be found in Leonard Binder et al., *Crises and Sequences in Political Development* (Princeton: Princeton University Press, 1971).

[10] These have been used in collateral research. See Eckstein and Gurr, Parts II and IV, and the studies cited there for use of these dimensions in collateral research.

were chosen. In the process of revision we sought a closer "fit" between the categories and the concepts of the analytic scheme, but the final translation of the coded categories in terms of authority dimensions was made only after the data on 336 polities were coded and ready for analysis. The advantage of this procedure was to keep the coding close to the kinds of information given in the sources, thereby maximizing both the completeness and reliability of coded information.

Indicators of Authority Dimensions: The information coded in these categories is in nominal form, but by making certain assumptions, the categories were used to make two alternative kinds of indicators. First, five indicators of authority dimensions were constructed by ordering the relevant coded categories along each dimension, then assigning rank scores to the categories. The results of this exercise are on display in Table 1. Note that the dimensions are defined *a priori*, and that these specific indicators reflect both the exigencies of imprecise or limited information and the characteristics of the kind of units being surveyed; many quite different operationalizations of the dimensions are equally or more appropriate.

Two of the dimensions are represented by categories which are self-evidently scaleable. The categories of *Decision Constraints*, for example, refer directly to different degrees of limitation on the decision-making powers of chief executives by the legislature (and similar accountability groups). The categories of governmental *Directiveness* are defined so as to refer to apparently-equal increments in the scope of control and regulation. The other dimensional indicators require more explanation.

Recruitment to any position or set of positions is "open" to the extent that all lower-ranked individuals have equal opportunity to attain it. Recruitment by lottery is the theoretical extreme of openness, but no chief executives in recent history have been so chosen. Of the more common modes of Recruitment to the position of chief executive, competitive elections are judged to be more "open," while succession to a royal parent (ascription) is the most "closed." The "dual executive" combination of a monarch who shares power with an elected premier makes a natural midpoint to the scale. The "Caesaristic" pattern of executive self-selection by seizure of power is toward the open end of the scale, but less open than competitive elections because a military base is usually necessary and attempts are very risky (both of which constrain opportunity). "Designation" is the choice of chief executives within the elite without formal competition; opportunity is restricted to a very small, pre-determined group of "eligibles," most of whom have little influence over the outcome. The point is that this ordering of categories is based on informed judgments of what each mode of Recruitment implies about the opportunities inherent in the process.

Participation is indexed by reference to prevailing patterns of political competition among all members of the polity. It is assumed that the volume of Participation is greatest when there are relatively stable and enduring political groups (not necessarily parties) which regularly compete for national political influence ("institutionalized" competition). Participation tends to be least when no open, organized competition is allowed. (There is some participatory activity in even the most closed and repressive polity, just as there is much apathy in multi-party systems; the point is that the volume of participation *tends* to be much greater and influential in the latter than in the former.) The difference between the "factional/restricted" (4) and "restricted" (2) categories is substantial. The former label refers to a pattern of intense, often violent competition between "in" and "out" factions, who hold power *seriatim* and repress their opponents: participatory activity is high, even though some groups are temporarily excluded. The purely "restricted" category is characterized by persisting, institutionalized restriction on Participation by some large classes of people; contemporary South Africa and pre-revolutionary Russia are examples.

Complexity of governmental structures is simplest in a unitary state with one-man dictatorship. Conditions which increase complexity include (a) decision making by collectivities rather than individuals; (b) the presence of several overlapping decision-structures at the same level; and (c) the vertical differentiation of a unit (here the polity) into distinct sub-units on the same level. The three sets of ordered categories shown in Table 1 correspond closely to these three facets of complexity.

The ranked categories in Table 1 are scored so that high scores represent relatively high levels of subordinate (citizen) participation and low levels of superordinate (elite) control, while low scores signify the opposite conditions. For purposes of some analyses the categories are assumed to represent approximately equal units along the underlying dimensions, hence the indicators are treated as having interval properties.

These five indicators are referred to below as *authority indicators* and are capitalized to distinguish them from more colloquial uses of the same terms.

Indicators of System Type: The second set of indicators constructed from the coded data con-

Table 2. Operational Definitions of Scales of Autocracy, Democracy, Anocracy

Authority variable	Categories and Weights by System Type		
	Autocracy	Democracy	Anarchy
Executive recruitment	Ascription =1 Designation =1 Dual =1	Competitive elections =2	Caesaristic =1
Decision constraints	Unlimited authority =2 Slightly limited =1	Legislative parity =2 Substantial limits =1	None[a]
Participation	Suppressed =2 Restricted =1	Institutionalized =2	Uninstitutionalized =2 Factional/restricted =1
Directiveness	Totalitarian =2 Segmental plus =1	None[c]	Minimal =2
Centralization	None[b]	Decentralized =1	Decentralized =2
Maximum score	7	7	7

[a] None of the categories of decision constraints seems uniquely or predominantly associated with the anocratic polity. Whatever the relative independence of their executives, the more salient characteristic of such polities is their lack of directiveness.

[b] Centralization is not a distinctive feature of autocracies, in view of the large portion of competitive democracies that share that characteristic.

[c] No particular level of directiveness is distinctively characteristic of democracies; empirically the range is from "minimal" to "segmental +."

sists of three measures of system type. That is, some of the categories are distinctively associated with polities of "democratic" or "autocratic" type, while others are typical of the so-called "anocratic" or uninstitutionalized state.[11] Still other categories signify intermediate conditions. The indicators rest on these difinitional assumptions:

(1) The essential quality of the "autocratic polity" is the institutionalized monopolization of power and political activity by the state.

(2) The essential quality of the "democratic polity" is the presence of multiple institutionalized centers of power, some of which are open to widespread citizen participation.

(3) The essential quality of the "anocratic polity" is its relative lack of political power and institutionalization.

The indexes constructed on these assumptions are shown in Table 2. The principle is that each polity is given a weighted score depending on the nature and number of autocratic, democratic, and anocratic properties it has. The categories of executive Recruitment and Participation are all distinctively associated with, and distinguish among, the three system types. The categories of the remaining three authority indicators provide the basis for further distinctions between two of the three types.

No attempt is made to label a polity as a "democracy" or an "autocracy." Rather, the indices make it possible to distinguish *degrees* of autocracy, democracy, and anocracy in any system.[12] Most systems in fact have mixed authority

[11] I have resorted to a neologism, suggested by Harry Eckstein, because no conventional term describes well the kind of state I have in mind: one which has minimal functions, an uninstitutionalized pattern of political competition, and executive leaders constantly imperiled by rival leaders. The term has the same Greek root as anarchy, which means literally the absence of any sort of rule. *Anocracy*, on the same basis, means literally the absence of power or control, though of course I use it to signify states which approach but do not reach the extreme condition. Archetypical examples in history include the Holy Roman Empire and many lesser medieval principalities.

[12] The literature includes a number of indicators of "democracy." One is Fitzgibbon's set of judgmental indicators of the extent of democracy in Latin American countries; see Russell H. Fitzgibbon, "Measuring Democratic Change in Latin America," *Journal of Politics*, 29 (February, 1967), 129–166. An index of "democratic development" based on electoral characteristics is used by Deane E. Neubauer, "Some Conditions of Democracy," *American Political Science Review*, 61 (December, 1967), 1002–1009. Arthur S. Banks has recently assessed empirically the extent of what he calls "democratic performance" in a large number of nations in the 19th and 20th centuries, in "Correlates of Democratic Performance," *Comparative Politics*, 4 (January, 1972), 217–230. A critical review of this literature is John D. May, *Of the Conditions and Measures of Democracy* (Morristown, N.J.: General Learning Press,

patterns, as demonstrated by their scores on these indicators. An extreme example is the political system of Uganda immediately after independence: it was characterized by competitive elections, legislative parity with the executive, uninstitutionalized political competition, decentralization, and minimal scope of government. This combination translates into a "democracy" score of 5 and an "anocracy" score of 6.[13] This, however, was an unusual (and unstable!) case: most polities score low on two of the three indicators. These indicators are referred to below as *system indicators.*

Procedures. Three features of the operational procedures are described here: (1) the universe of analysis; (2) coding procedures, with special reference to the problem of identifying fundamental, abrupt polity changes; and (3) the data collection process.

Universe of Analysis: The initial universe of analysis comprised the 82 nations which Singer and Small identified as being members of the international system at any time between 1817 and 1940.[14] This list was used partly as a way of ensuring that the sample included the more important historical states as well as contemporary ones. Coding, however, was not limited to the periods during which these nations were members of the international system: their political systems were coded for all periods between 1800 and 1970 during which they had internal autonomy. Korea, for example, was a member of the international system by Singer and Small's criteria only from about 1889 to 1904; Korea's political system is coded in this study from 1800 through 1904 (when it came under Japanese control), and North and South Korea are coded separately from 1948 through 1970. This study also includes predecessor states to those on Singer and Small's list: an example is Gran Colombia, the predecessor state, from 1821 to 1830, of Colombia, Ecuador, and Venezuela. A second reason for relying on Singer and Small's list was that it gave at least preliminary assurance that a moderate amount of historical information would be available on these states. Only four states proved uncodable for our purposes: Mongolia, Mecklenburg, and the two Hessen. If a political system was in existence in 1800, its approximate date of ori but its "initial" authority ch coded as of 1800.

Three kinds of political enti from the universe of analysis s(them are (1) countries like I Bhutan, too small or peripher international system; (2) "new post-1940 era; and (3) states (colonial rule, except for colon internal autonomy and then in to 1940. New Zealand, for exam the beginning of effective inter although it did not obtain Dom 1907.

Identifying Polity Changes: The polity and (usually) the inceptic are defined by a major, abrupt (tern of political authority. Th termination of such changes is crucial to the validity of measu and adaptability. It is easy en theoretical distinction to the eff change *of* system is a polity c change *in* the system is not. But seldom manifest such clear-cut the speed and extent of politica tinua. Hence it was necessary t some criteria for "change of syst summarized below, are "reason the framework of this analysis, irreducible element of arbitrarin

An *abrupt change* is any o (defined below) that is accompl or less. *Major changes* in pat authority are defined operatio six of the authority dimensions example, a change in a polity' ticipation from "institutionalize(or vice versa, is major. A c stricted" to "suppressed," thou minor. The need for brevity pre of the rationale for each such r summarized in Figure 1: transi solid lines are "major changes,' nify "minor changes." Changes of two or more categories als "major," though they virtua abruptly except as a concomita major political transformatio shifts in Directiveness are "min

Since political change must b abrupt to constitute a "polity ch fication in the operational dist "major" and "minor" change cisions about polity terminati change (a) the number of p (b) their scores for persistence

1973). But there seem to be no attempts to index degrees of "autocracy" or any variable similar to "anocracy."

[13] Uganda is not among the countries included in the analyses of this paper; it was coded in an extension of the study to "new nations."

[14] J. David Singer and Melvin Small, "The Composition and Status Ordering of the International System, 1815–1940." *World Politics,* 18 (January, 1966), 236–282.

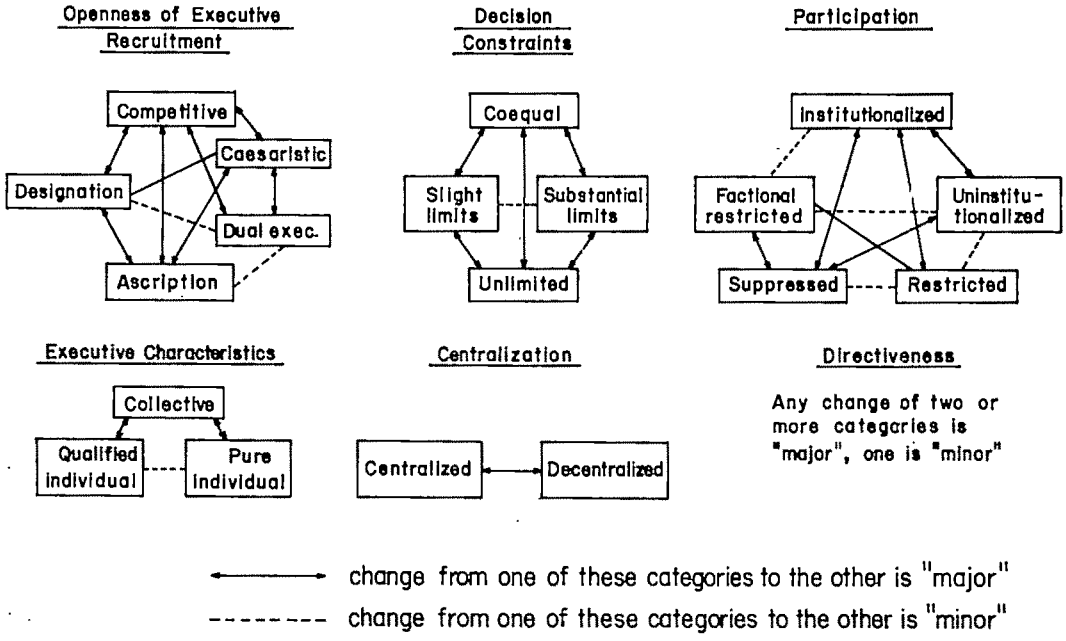


Figure 1. Major and Minor Changes in Characteristics of Political Authority

and, possibly, (c) the findings of this study. Some such reanalyses are being made, and the coded data and narrative documentation are available from the Inter-University Consortium for Political Research for those who wish to make others.[15]

Many stipulations other than the operational definitions of "abrupt" and "major" authority changes were used in coding decisions about the birth and death of polities. These are some of the more important:

(1) *Changes in form vs. practice:* Legal or constitutional enactments of major authority changes do not signify changes of polity unless accompanied by equivalent changes in practice. Formal changes were taken in coding as clues or indicators that changes in practice might have occurred, or were to be attempted. Only if other evidence testified to the reality of constitutionally prescribed change was the date of a formal enactment used as the change point. Conversely, major changes in authority practices were treated as "major" whether or not accompanied by formal, constitutional change.

(2) *Power seizures:* Seizures of power are not necessarily indicators of "major change." Additional evidence of basic changes in authority practice was needed to warrant a decision that one polity had died and that a new "revolutionary" one had been born. Ecuador provides a striking case in point: it has had dozens of coups since its founding, including several allegedly revolutionary ones, but in not one instance could we find evidence that a major change of political system followed therefrom. The state operates now with a political system that is little changed, except in Directiveness, from that established in the 1830s.

(3) *Transitional periods:* Not all new polities emerge phoenix-like out of the ashes of their predecessors; months, sometimes years may elapse while the ground-rules of the new polity are worked out and put into effect. In France, General de Gaulle's assumption of power in June, 1958, sounded the death-knell of the Fourth Republic; the four months until the referendum of October 1958, which approved the Constitution of the Fifth Republic, are coded as a transitional period. In the 20th century, transitional periods are relatively brief, as this one was; in the 19th century, when political systems were often more ephemeral and communications slower, they tended to be longer. About 45 per cent of the 336 polities identified in this study began with transitional periods; such periods are not included in the lifespans of either predecessor or successor polities.

[15] Preliminary results of analyses by Robert Harmel of Northwestern University, using more restrictive definitions of what constitutes a "polity change," are consistent with most of the findings reported below. An important exception is his finding that some sets of incoherent polities, especially in Europe, have been as durable as coherent democracies and more so than coherent autocracies.

Data Collection: Data were collected by a single coder working with increasingly-refined versions of category definitions and coding guidelines. On three occasions all information gathered to date was reviewed and recoded to ensure its consistency with more detailed guidelines. Multiple historical sources were used for each country, along with reference to such standard sources as the *Statesman's Yearbook*, the *Annual Register*, the *Encyclopedia of World History*, and Banks's *Cross-Polity Time Series Data*.[16] The procedure involved, first, identifying a number of historical and social science texts and monographs for each state, then compiling from them a basic political chronology. Periods of substantial political change were identified while compiling each chronology and then scrutinized, using diverse sources, to determine whether events met the specified criteria for changes of and in polities. Changes of system were thereby identified and separate coding sheets then were prepared for each polity. The same sources were searched for information on authority characteristics. Some characteristics could not be coded for some polities, but the total missing-data component for the six dimensional indicators was less than one per cent. Authority changes short of policy termination were also described, and for each altered polity a separate authority profile was coded at its termination (or as of 1971, for continuing polities). Altogether about 40 per cent of all polities underwent minor or gradual major changes in authority characteristics between their origin and termination.

Data other than that on authority characteristics and system change points were also collected, including considerable information about the circumstances of each polity change. This information was later subjected to content analysis, and each polity coded for the internal and external conditions of its birth and death—i.e., the conditions which precipitated each change of system. Data on population and on the source of each new polity's authority pattern also were recorded. None of these data are analyzed in this paper.[17]

[16] William L. Langer, comp. and ed., *An Encyclopedia of World History* (Boston: Houghton Mifflin, 4th ed., 1968); Arthur S. Banks, comp., *Cross-Polity Time-Series Data* (Cambridge, Mass.: MIT Press, 1971.)

[17] The following materials cannot be published for reasons of space but are available from the author and the Inter-University Consortium for Political Research: the coding guidelines and category definitions; frequency distributions of coded authority data; list of countries coded; and dates of polity transformation in each. Available from the Consortium only are the full set of coded data for all polities and narrative summaries of the political events and changes for each polity, with lists of sources used.

Reliability and Validity: The necessary condition for obtaining reliable coded data in social research is to use procedures that maximize its internal consistency. The reliance in this study on pretested and detailed coding guidelines, periodic reviews of prior coding, and a single knowledgeable, trained coder constitute a *prima facie* case for its reliability. There are four kinds of tests of the reliability thereby achieved. One is to have other coders replicate part or all of the coding using the same instructions and materials. This was not done in this study for want of resources and because alternatives were at hand. The second is to solicit expert opinions on the accuracy of coded judgments. This was done sporadically but not systematically in this study, by consultation with area experts, and their opinions are incorporated in the coded data. The third is to test the internal consistency and accuracy of the data. One such test applicable to this study is to ascertain whether each successive pair of polities are distinguished from one another by one or more changes in authority characteristics defined *a priori* as "major." This test was made and a few of the codings revised accordingly. One side benefit of this and other such tests was virtually to eliminate punching errors from the data set. The final kind of test is, technically, convergent validation: one determines whether other coded data on the same or similar variables yield similar results. A substantial test of this sort is described below, and others are in progress.

Ten of the countries in this study were included in the author's earlier study of political performance. In that study I identified the approximate dates of polity transformation between 1840 and 1970, applying less precise versions of the guidelines employed here to more summary sources.[18] The results are compared here:

(1) Instances of agreement about periods of polity transformation	36
(2) Instances of polity transformations identified in the current study but not the prior one	6
(3) Instances of polity transformation identified in the prior study but not the current one	3

The ratio of agreement between the two studies is thus 36/45, or .80. The nine instances of disagreement were checked against the detailed chronologies compiled in the second study. The dis-

[18] See Gurr and McClelland. This is a convergent validation test rather than a test of intercoder reliability because different instruments and sources were used in the two studies. Carryover from the first to the second studies was minimized by two circumstances: the coder in the present study did not examine the results of the first study before or during the coding, and the author compared the results of the studies only after the completion of data collection in the second study.

Table 3. Average Scores on Authority Indicators by Cluster of Polities

Cluster of Polities[b]	Authority Dimensions[a] Openness of Recruitment (range 1–5)	Decision Constraints (range 1–4)	Participation (range 1–5)	Directiveness (range 1–5)	Complexity (range 2–12)
All (336)	*2.99*	*2.03*	*3.03*	*4.25*	*4.44*
Polities by region:					
European (158)	2.92	2.29	2.82	4.04	4.83
Latin (120)	3.33	1.74	3.39	4.44	3.90
Afro-Asian (58)	2.45	1.93	2.86	4.45	4.46
Polities by date of establishment					
19th Century (161)	2.66	1.92	3.09	4.82	4.26
20th Century (175)	3.29	2.14	2.97	3.73	4.60
Continuing in 1971 (69)	3.42	2.47	2.96	3.09	5.04

[a] Operationally defined in Table 1. For the purpose of this analysis, polities which underwent minor or gradual changes in authority characteristics between their origin and termination, or between their origin and 1971, were given the average of their initial and final scores on the dimensions.

[b] *N*'s are given in parentheses. Some of the means are based on slightly smaller *N*'s because of missing data.

crepancies proved to be the result either of insufficiently-detailed information in the sources used in the first study, or of the more precise criteria used to define "abrupt, major change" in the present study. In short, these results substantially support the reliability with which this study identifies system change points. A more comprehensive assessment of the validity of this study's coding of authority characteristics and its identification of change points is now being made, via a comparison with Banks's time-series data on type of political regime.[19]

Data on Authority Patterns. Some of the data on authority characteristics are summarized in Tables 3 and 4, for all 336 polities and for some temporal and regional subgroups. The "19th-century" group includes all polities established prior to 1900—some as early as the 13th century. The "20th-century group" comprises those established between 1900 and 1970, while the 69 "continuing polities" are the political systems in the set which were functioning in 1971. These groups are referred to below as "clusters."

These are some of the more noteworthy generalizations about the dimensions of authority: (1) Recruitment of the chief executive has become increasingly open over time. It has been most open in the Latin polities, partly because of the prevalence there of the relatively open "Caesaristic" pattern. (2) Chief executives have operated with the fewest constraints in the 19th century and in Latin political systems; they are most circumscribed in European and the continuing polities. (3) Participation varies less among the clusters of polities than any other authority characteristic. The most highly participatory polities are the Latin, not because many have had institutionalized electoral systems (score of 5) as much as because of the prevalence among them of the "factional/restricted" pattern, which signifies more vigorous Participation than any of the remaining categories. (4) The scope of governmental Directiveness varies more markedly among clusters than any other authority characteristic. Few 19th-century polities had more than minimal scope (a score of 5), while the average for contemporary polities is at the "segmental" midpoint of the scale. (5) Complexity tends to vary with Directiveness and Decision Constraints: like both of them it is highest in the European polities, on the average, and has increased over time.

The average autocracy, democracy, and anocracy scores of polities in Table 4 provide a more familiar standard for evaluation than the authority indicators, though they are of course constructed from the same coded data. Three distinctive patterns deserve comment: (1) The average scores on the system indicators are low throughout, and the percentages of "pure types" are lower than might be expected. Less than two-thirds (63 per cent) of all polities have scores above the midpoint on any of the three indicators, the others

[19] Banks, *Cross-Polity Time-Series Data*, codes "type of regime" in four categories, civilian, military-civilian, military, other; and also rates the effectiveness of legislatures. Despite the lack of discrimination in these sets of categories, they permit some general comparisons with the coded data on polities. Michael Ward of Northwestern University has used the Banks data to construct approximately-comparable indicators of five authority dimensions. Scores on his indicators, taken at the midpoints of polity lifespans, have these Pearson *r* coefficients with the scores used in this paper: Openness .50; Decision Constraints .77; Participation .61; Directiveness .52; Complexity .81.

Table 4. Average Scores on System Indicators, by Cluster of Polities[a]

Group of Polities[b]	Autocracy		Democracy		Anocracy	
	M score	% 3.5+	*M* score	% 3.5+	*M* score	% 3.5+
All (336)	*2.48*	*30%*	*1.29*	*18%*	*1.97*	*15%*
Polities by region:						
European (158)	2.60	35%	1.56	25%	1.57	6%
Latin (120)	2.18	21%	1.08	10%	2.49	23%
Afro-Asian (58)	2.80	36%	1.03	14%	2.00	21%
Polities by date of establishment:						
19th Century (161)	2.43	23%	0.93	9%	2.78	24%
20th Century (175)	2.53	37%	1.63	26%	1.23	6%
Continuing in 1971 (69)	2.60	41%	2.32	39%	0.92	4%

[a] The possible range of scores for each characteristic is 0 to 7. The polities with scores of 3.5+ are relatively "pure" cases of Autocracy, Democracy, and Anocracy respectively. Also see footnote (a) to Table 3.

[b] *N*'s are given in parentheses. Some of the means and percentages are based on slightly smaller *N*'s than those shown because of missing data on some of the authority characteristics.

have "mixed" authority patterns. The mixed and anocratic polities together are more numerous than all those which approximate the two "classical," democratic and autocratic types. (2) Relatively autocratic polities are and have been more common than relatively democratic ones. Proportionally, polities with distinctly autocratic characteristics have been most prevalent in Afro-Asia and in the 19th century, as might be expected, but they also are more common than democracies in Europe and among 20th-century polities. (3) There are distinct temporal trends in the data. Autocratic and democratic traits have both increased markedly, while anocratic characteristics have decreased precipitously. As one consequence, the proportion of highly autocratic and democratic polities together has increased very substantially, from 32 per cent among polities established before or during the 19th century to 80 per cent among contemporary polities.

A number of causal or correlational questions are raised by these data. Some of them will be treated in the final section, after the data on durability are examined.

Measures of System Durability

The Components of Durability.

(1) *Persistence* is the number of years a polity endures without an abrupt, major change of pattern. Such scores are available for the 267 historical polities in the universe of 336, rounded to the nearest year.

(2) *Adaptability* is less precisely indicated by a polity's number of minor and gradual major changes in authority characteristics (see Figure 1), to which are added the number of imposed polity interruptions. "Imposed interruptions" are changes imposed by a foreign power; it is assumed to be a mark of adaptability if a conquered country, on regaining independence, re-establishes the polity that existed before intervention. In such instances the imposed change is not treated as a polity change. The formula is:

$$\text{Adaptability} = 2\times\text{no. of major gradual authority changes} + 2\times\text{no. of imposed polity interruptions} + 1\times\text{no. of minor authority changes}$$

The justification for combining these three characteristics into a single indicator is conceptual. The three components have low but positive intercorrelations (.09 to .23). All have substantial *r*'s with the composite indicator: adaptability correlates .67 with number of major gradual changes, .72 with number of minor changes, and .56 with number of interruptions.

The persistence and adaptability scores were correlated as an empirical test of the conceptual argument that they are two components of "the same" variable. They are significantly related in all clusters ($r = .48$ in the historical polities and .72 in continuing ones) but they generally share less than 50 per cent common variance. The measure of adaptability is particularly insensitive at lower levels of durability: half the historical polities had lifespans of 12 years or less, but more than four-fifths of these had (as might be expected) 0 adaptability scores. Persistence and adaptability therefore appear to be sufficiently different aspects of durability to warrant being treated as separate dependent variables in analyses below.

The Durability Data: Persistence. The persistence data can be summarized and interpreted in various ways. Figure 2 shows the distribution of lifespans of the 267 historical polities identified in this study, by decade, and of ages of 69 contemporary

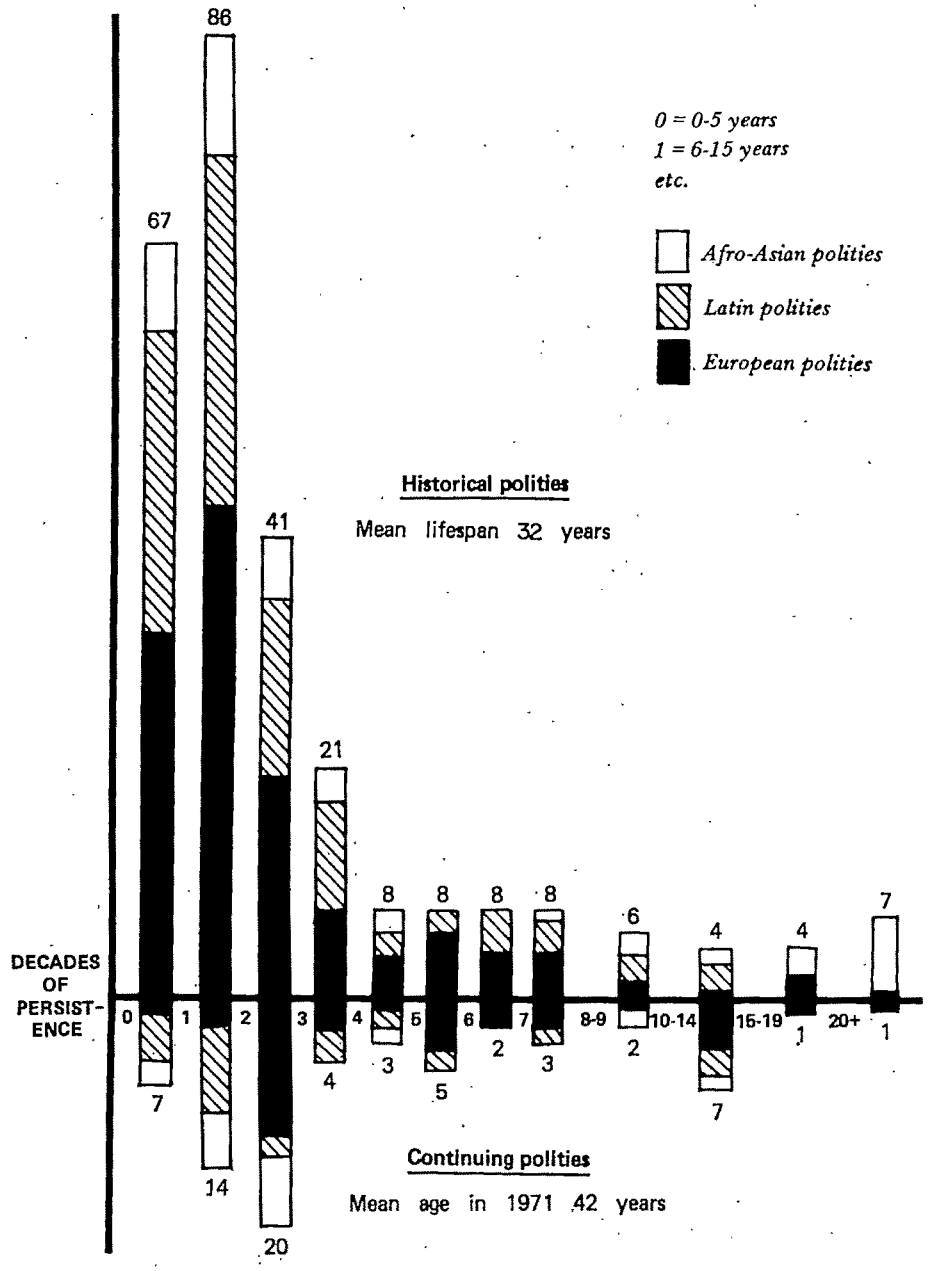


Figure 2. Persistence of Historical and Continuing Polities, by Decade

polities. Probably the most noteworthy aspect of Figure 2 is the high "infant mortality" rate of new polities. Although the average lifespan of historical polities is 32 years, fully half underwent a fundamental transformation before or during their 12th year. The median lifespans of the more important clusters are:

European polities	12.2 years
Third-world polities	12.3 years
19th century polities	19.9 years
20th century polities	9.0 years

There is no difference between the European and Third-World (Latin plus Afro-Asian) polities, but the difference between older and newer polities is quite significant; fundamental political change has been considerably more frequent in the 20th century than previously. It should also be noted, however, that the median attained age of the 69 "continuing" or contemporary polities is greater than any of the above, 22 years. Since they, as a group, have survived well beyond their predecessors, it is logical to infer that they include not only

Table 5. Survival Probabilities for Polities by Attained Age, All Contemporary and Historical Polities

Attained Age	No. of Polities "At Risk"[a]	Probability of Survival through Age[b]						
		5	15	25	50	75	100	150
0	330	.80	.52	.34	.21	.11	.09	.04
5 years	249		.65	.45	.28	.15	.11	.06
15 years	143			.71	.46	.25	.19	.10
25 years	93				.66	.38	.29	.16
50 years	53					.60	.47	.27
75 years	30						.80	.52
100 years	17							.70

[a] The number of historical polities which reached the attained age listed plus the number of continuing polities (as of 1971) which reached the next-higher tabled age. For example: The number of polities "at risk" at year 0 includes all historical polities plus the 62 continuing polities (of 69) which were more than 5 years old in 1971; this base is used in calculating the p of polities surviving through age 5. The base for calculating the 15-year survival p is all historical polities plus the 46 continuing polities which were more than 15 years old in 1971, and so forth. This procedure of adjusting the "at risk" population enables us to use information on the attained age of continuing polities without biasing the probability estimates. The persistence data used to construct this and the next table, and Figure 2, differ very slightly from that used in the remainder of this article.

[b] The computation formula for each probability estimate is:

$$p_X = \frac{\text{survivors at age } X}{\text{no. of polities at risk}},$$

where X signifies the survival age for which p is being estimated.

the survivors of stochastic processes of political decay but also a relatively high proportion of "successful," adapting systems.

Let us assume, though, that the incidence of system change has a substantial stochastic element, and that the persistence of a polity can be estimated on the basis of its attained age alone. Tables 5 and 6 are constructed on such assumptions. Table 5 uses data on all historical and contemporary polities to estimate their survival prospects at various ages. The current political system of Colombia was established in 1957. Therefore, as of 1972, when it had endured for 15 years, the tabled data indicate that it has a .7 chance of surviving without major, abrupt change for another ten years, but only a .2 chance of surviving through the year 2072—assuming, of course, that the processes affecting polity survival in the sample of 336 continue to produce the effects they had between 1800 and 1970. The historical data provide no firm basis for persistence estimates for more long-lived polities like those of contemporary Britain and the United States, though: very few polities in the sample survived longer than these without a system transformation, and those which did were "traditional" empires.

The "life expectancy" estimates of Table 6 are derived from survival estimates for various clusters of polities. They do not signify survival probabilities; rather they signify the "expected" lifespans of polities of different attained ages.[20] Like the estimates in Table 5, these figures are both a descriptive generalization from the data on 336 polities and a forecasting instrument. If one takes them at face value, they suggest that a contemporary 75-year-old polity is likely to endure a full 170 years or so. Such forecasts cannot be taken very seriously when applied to individual systems. But they do provide a sort of benchmark against which to evaluate proposed theoretical explanations of polity durability: Can those theories account for why particular polities persist either less or longer than would be predicted on assumptions of randomness? And, can those theories make more accurate predictions than can be made on a "life expectancy" basis?

The Durability Data: Adaptability. Table 7 provides an overview of the adaptability scores.

[20] The "life expectancy" estimates are of total polity lifespan, including attained age. For example, a 50-year-old Third-World polity would be expected to survive another 50 years for a 100 year total. The figures are equivalent to medians, not averages: they are the age beyond which less than half the polities of a cluster (including historical and contemporary polities) are known to have survived. The inclusion of contemporary polities increases the medians for polities at year 0 over the medians which are listed for regional and historical clusters of polities in the text, above.

Table 6. Life Expectancies of Polities at Various Attained Ages[a]

Attained Age	Life Expectancies ($p = .5$)				
	All polities	European	Third World[b]	19th Cent.	20th Cent.[c]
0	16	16	16	22	13
5 years	22	22	23	36	17
15 years	46	47	44	55	32
25 years	64	62	65	69	41
50 years	94	84	100	94	—
75 years	156	170	145	156	—

[a] The "expected" lifespan of polities attaining a particular age is the age at which $p = .50$, determined by interpolation from survival probabilities as shown in Table 5. For example, the life expectancy of a polity at birth, evaluated by reference to all polities as in Table 5, is between 15 years ($p = .52$) and 25 years ($p = .34$). Linear interpolation between the two indicates that $p = .50$ is about 16 years.

[b] The Latin and Afro-Asian polities are combined.

[c] Life expectancies beyond 15 years are based on very small N's because polities established in the 20th century are by definition a "youthful" cohort: the oldest is a mere 70.

Adaptability has been greater among European and Latin polities than Afro-Asian ones, and also greater among continuing polities and those established in the 19th century—but no more than can be accounted for by their greater average age. The greatest adaptability is manifested by the contemporary polities, many of them also long-lived. Their adaptability sharply distinguishes them from the long-lived, inflexible traditional polities in the set of 336. A comparison of the frequency of adaptations in three groups of polities is instructive. The following groups include all 31 polities which survived more than 75 years. For each group their average adaptability scores have been calculated (in scale points: minor changes = 1, major gradual changes = 2) per 100 coded polity years, excluding imposed interruptions and their pre-1800 histories if any. The results are these:

11 continuing polities averaged 4.5 adaptations per 100 years;

5 historical quasi-democracies averaged 6.2 adaptations per 100 years; and

15 historical autocracies averaged 1.3 adaptations per 100 years.

The historical autocracies though long-lived were manifestly inflexible, and their political systems collapsed, often in revolutionary circumstances. The long-lived historical democracies experienced very considerable changes *in* system before an abrupt change *of* system; these latter changes were usually in the "democratic" direction. The long-lived continuing polities, most of them also democracies, have demonstrated a lesser but very substantial capacity for adaptation.

Authority Characteristics as Determinants of Durability

The data and indicators constructed from them have been described in detail because they are new and reflect concepts most of which are not in common currency. Some of their many theoretical

Table 7. Polity Adaptability Scores

	All polities	European polities	Latin polities	Afro-Asian polities	19th Cent. polities	20th Cent. polities	Continuing polities
No. of polities[a]	324	152	120	52	149	175	69
Distribution of scores, %							
0	61%	54%	68%	65%	64%	58%	38%
1–2	23	26	18	27	17	29	33
3+	16	20	15	8	19	13	29
Mean	1.09	1.30	1.02	0.67	1.29	0.93	1.96

[a] Includes all 336 polities, except for 12 polities whose pre-1800 lifespan exceeded their post-1800 lifespan, and whose adaptability scores were therefore judged to be unreliable.

uses are explored here by evaluating three general, but specifically "political," hypotheses about effects of authority characteristics *per se* on the persistence and adaptability of polities.[21]

Hypothesis 1: Persistence and adaptability tend to be greater in polities of high Directiveness and Complexity.

This might be called the "bigger is better" hypothesis. Its first part is a codification of the contention, or assumption, that powerful, multifunctional states have better survival prospects than states which do little. This view has considerable currency among Third-World leaders, for example, many of whom have argued that the viability of less-developed nations requires the state to control economic functions and mobilize social and political forces. Their principal models are the successful Communist states. Both parts of the hypothesis are consistent with a "general systems" type of argument, to the effect that complex systems with substantial control over their environments are more likely to adapt successfully in response to stress than are others. Huntington's views about the importance of "institutionalization" for political stability also converge on the hypothesis: complexity and autonomy are among the characteristics he attributes to "institutionalized" political structures.[22]

Hypothesis 2: Persistence and adaptability tend to be enhanced most by democratic authority traits, less by autocratic traits, and least by anocratic traits.

The superiority of democratic authority patterns derives from the virtues conventionally attributed to plural democracies. They are allegedly more responsive to citizen and interest group opinion, hence more adaptable and less susceptible to abrupt, revolutionary transformation than other polities. Autocracies, in turn, are likely to be more durable than anocracies because they can better command the resources and coercive force necessary to control disaffected subjects.

Hypothesis 3: Persistence and adaptability tend to be greater in polities with consistently democratic and autocratic authority patterns than in polities with mixed or anocratic authority patterns.

This hypothesis follows from Eckstein's argument that organizations with coherent authority patterns ought to outperform—including outlast—organizations with incoherent patterns.[23] The authority characteristics of "coherent" polities are logically or psychologically consistent with one another and thereby enhance the polity's capacity to endure. The authority characteristics of "incoherent" polities are mutually dysfunctional, and under stress such systems are subject to abrupt, fundamental change. For example, a polity which has institutionalized political competition but a nonelected chief executive who is subject to few or no constraints is "incoherent." Two more coherent, mutually functional patterns are (1) institutionalized competition in conjunction with an elected, responsive executive, or (2) suppressed competition with a nonelected executive holding unlimited powers. The characteristics of highly democratic polities, as operationally defined in this study (see Table 2), are coherent in this sense, and so are the characteristics of highly autocratic polities. The highly anocratic and mixed polities are not, or at least they are operationally defined so that they include a greater proportion of inconsistent patterns.

Research Strategies. The dependent variables of all three hypotheses are persistence and adaptability. Three interrelated strategies are used to assess their hypothetical causes. (1) One is to correlate indicators of authority characteristics with the durability measures for all cases; this tests for the existence and strength of systematic relationships across the full range of cases. (2) The second strategy is to identify two "extreme" subsets of polities, the most short-lived and long-lived, and then to determine which of the postulated authority characteristics distinguish significantly between them, and between them and the "middling" cases. This procedure gives us information on authority characteristics that make most difference at the extremes of durability. (3) Finally, both kinds of comparisons are replicated for different clusters of polities: (a) The results of comparisons for European, Latin, and Afro-Asian polities are reported separately. If the alleged effects of authority characteristics on durability are universals, then statistical relationships should be similar within each of these clusters. But should relationships vary markedly from one cluster to another, they suggest that other, more remote or controlling conditions are systematically influencing the effects of authority characteristics on

[21] Note that the independent variables are structural properties of the political system, not properties of other social institutions: data are not available here to test directly Harry Eckstein's proposition that congruence between nongovernmental and governmental authority patterns determines the performance and durability of political systems. See his "Authority Relations and Governmental Performance: A Theoretical Framework," *Comparative Political Studies*, 2 (October, 1969), 283.

[22] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), pp. 1–92.

[23] Eckstein, "Authority Relations and Governmental Performance," pp. 300–307.

durability. (b) Separate comparisons for historical and contemporary polities also are reported. If the correlates of durability are different between these two groups, the inference is that the survival-enhancing characteristics of polities have changed systematically over time.

The authority and durability indicators discussed above were subject to a few manipulations, described below, to facilitate hypothesis-testing.

Reduced Set of Polities: Some of the 336 political systems identified in this study ended not as a result of internal processes but as a consequence of external intervention or territorial union with another state. They were eliminated from the hypothesis-testing set *unless* they had already survived 70 years. (Any polity which endures 70 years without a major, abrupt change has demonstrated a high degree of durability whatever happens to it thereafter; less than 15 per cent of all polities ever attained this age.) Forty-two polities were eliminated on this basis; the analyses which follow are based on a maximum N of 294.

Estimated Persistence Scores: The known lifespans of historical polities are not comparable to the attained ages of contemporary polities: the latter can be expected to persist for varying, but often considerable, years into the future. So life expectancies were determined for the 69 contemporary polities, using the estimates in the first column of Table 6. For example, 15-year-old polities are shown there to have an average life span of 46 years; this estimate was used as the persistence score for the polities aged 14 to 18 in 1971. Four polities which were older than 125 in 1971 were assigned high but arbitrary persistence values of 250 or 300.[24] So the final measure of persistence includes known values for 225 historical cases and life expectancy values for 69 contemporary cases. Because the distribution of scores is strongly skewed, as shown in Figure 2, a square root transformation was performed.[25]

Durability Subsets of Polities: Subsets of especially durable and ephemeral polities were distinguished using these operational definitions:

1) *Low Durability:* Historical polities which persisted five years or less, $N=57$. All but one of these polities had 0 adaptability scores.
2) *High Durability:* Historical polities which persisted 55 years or more *and* had adaptability scores of 2 or more, $N=20$; plus continuing polities with life expectancies of 85 years or more *and* adaptability scores of 1 or more, $N=22$; total $N=42$.
3) *Middling Durability:* All other polities, $N=195$.

Note that persistence and adaptability are combined in defining the high durability subset, by making both necessary conditions for inclusion.

Authority Subsets of Polities: Three subsets of polities were distinguished based on their type-of-system scores, as in Table 4:

1) *Autocratic polities*, autocracy score 3.5+, $N=82$
2) *Democratic polities*, democracy score 3.5+, $N=57$
3) *Anocratic polities*, anocracy score 3.5+, $N=45$[26]

Three other subsets were defined using higher thresholds than these, to test the "coherence" hypothesis:

1) *Highly coherent polities*, autocracy score 5.0+ *or* democracy score 4.5+, $N=81$
2) *Anocratic polities*, anocracy score 4.0+, $N=38$
3) *Incoherent polities*, those not in the above two sets, $N=175$

Results. The bivariate relations between authority variables and durability are summarized in Tables 8 through 12. Tables 8 and 10 list the results of correlational analysis: each dimensional indicator, and system indicator, is correlated with persistence and adaptability, respectively. In both sets of comparisons no consistency is found among the clusters: durable European polities have different authority traits from Latin and Afro-Asian polities; and durable historical polities frequently have traits that are opposite to those of durable contemporary ones.

Table 9 contrasts the authority characteristics of especially durable and ephemeral polities. Generally the highly durable European and continuing polities are substantially different in structure from the less durable polities; but none of the authority dimensions makes much difference for any other clusters of polities. The polities of lowest and middling durability—more than 85 per cent of all polities—are virtually indistinguishable in their authority characteristics.

Table 11 compares the average persistence and adaptability of polities by system type. These comparisons include only the polities with relatively high democracy, autocracy, and anocracy scores, but they too manifest the kind of inconsistency of results apparent in Table 8 through 10. The durability of relatively "pure types" of polities—those with highly coherent authority

[24] The life expectancy values for all polities are used rather than those of the regional or temporal clusters because, first, the N's for the subsets are too small, and second, because the subsets are categorical variables whose effects on persistence are to be assessed separately.

[25] A logarithmic transformation is conceptually less appropriate because too strong: it compresses the upper range of persistence valves more than is justified given this study's special focus on the determinants of longevity.

[26] Five polities are double-counted: three are above both the autocratic and anocratic threshold of 3.5, two are in both the democratic and anocratic subsets.

Table 8. Authority Dimensions as Determinants of Polity Durability: Correlations[a]

	Correlation with:	
Authority Dimensions[b]	*Persistence*[c]	*Adaptability*
Openness of Recruitment[d]		
European polities (124)	.15**	.10
Latin polities (113)	.16**	−.08
Afro-Asian polities (57)	.41***	−.15
Historical polities (225)	.33***	−.07
Continuing polities (69)	.25**	.13
Decision Constraints		
European polities	.26***	.20***
Latin polities	.16**	.15*
Afro-Asian polities	−.21	−.16
Historical polities	−.08	.04
Continuing polities	.48***	.33***
Participation		
European polities	−.01	.00
Latin polities	.08	.14*
Afro-Asian polities	−.25**	−.08
Historical polities	−.18***	−.02
Continuing polities	.23**	.18*
Directiveness		
European polities	−.25***	−.13*
Latin polities	−.12	−.04
Afro-Asian polities	.14	.02
Historical polities	.20***	−.07
Continuing polities	.20**	.16*
Complexity		
European polities	.31***	.25***
Latin polities	.04	.03
Afro-Asian polities	−.27*	−.16
Historical polities	−.07	.03
Continuing polities	.45***	.31***

* significant at .10.
** significant at .05.
*** significant at .01.

[a] Pearson product-moment *r*'s.

[b] Authority indicator scores used in these correlations are the average of scores at polity birth and termination. The dimensions are defined operationally in Table 1.

[c] Actual and estimated persistence scores with square-root transformation.

[d] Maximum numbers of polities in each subset are shown under Openness of Recruitment; the *N*'s actually used in each correlation are sometimes a few less because of missing authority data.

patterns, whether democratic or autocratic—is examined in Table 12 and contrasted with the durability of anarchic and "incoherent" polities. There is considerably more consistency to these results.

Tests of Hypothesis 1: The hypothesis is that the more Complex and Directive are political systems, the more likely they are to persist and adapt. These results from the comparisons in Tables 8 and 9 are needed to sustain the "complexity" part of the hypothesis:

(a) positive correlations of Complexity with persistence and adaptability (Table 8);
(b) mean Complexity scores for Low Durability polities < Middling Durability < High Durability (Table 9).

Both kinds of comparisons show that Complexity is associated with durability only in the European and continuing polities. In the Afro-Asian cluster the relationship is reversed: Complex systems there have tended to be more short lived. Most important, there is no consequential difference in Complexity between the most and least durable of historical polities—and the historical systems comprise fully three-quarters of all the cases. So significantly high levels of Complexity characterize only the more durable contemporary polities.

These results are required to support the "Directiveness" part of hypothesis 1:

(a) negative correlations of Directiveness with persistence and adaptability (Table 8);
(b) mean Directiveness scores for Low Durability polities > Middling Durability > High Durability (Table 9).

No consistent results of this sort are found for any cluster of polities. Among the continuing polities, in fact, the relation is reversed: the more durable of contemporary polities are the less (more highly-scoring) Directive ones.

Tests of Hypothesis 2: These results from the comparisons in Tables 10 and 11 would support hypothesis 2:

(a) Persistence and adaptability should correlate most strongly with degrees of democracy, less so with autocracy, and least so with anocracy (Table 10), and
(b) the average persistence and adaptability scores of democratic systems should exceed those of autocratic systems, which in turn should exceed those of anocratic systems (Table 11).

The empirical evidence in the two tables does indeed suggest that some systemic characteristics enhance durability more than others—but with great variation among clusters. The empirical tendencies can be summarized as follows, where D> symbolizes "more durable" and $\underline{D}$ means "not significantly more durable":

European:	Democracy	D>	Autocracy	D>	Anocracy
Latin American:	Democracy	D	Autocracy	D	Anocracy
Afro-Asian:	Autocracy	D	Anocracy	D>	Democracy
Historical:	Autocracy	D>	Anocracy	D	Democracy
Continuing:	Anocracy	D>	Democracy	D>	Autocracy

In short, only the results for the European cluster support the hypothesis. In every other group of polities it is contradicted in whole or part.

Tests of Hypothesis 3: The preceding two hypotheses attributed durability to substantive authority traits of political systems. This hypothesis attributes durability to "consistency of pattern" among authority traits rather than to the traits themselves. The empirical question is whether polities of relatively coherent or "pure" authority types, whether democratic or autocratic, manifest

Table 9. Authority Dimensions as Determinants of Polity Durability: Comparison of Extreme Cases

Authority Dimensions[b]	Mean Scores by Subset of Polities[a] Low Durability ($N=57$)		Middling Durability ($N=195$)		High Durability ($N=42$)
Openness of Recruitment[c]					
European polities (25/73/26)	2.80	=	2.88	<<	3.58
Latin polities (24/79/10)	3.42	=	3.38	=	3.00
Historical polities (53/151/21)	3.21	=	2.93	>>	2.14
Continuing polities (4/44/21)	2.00	=	3.14	<<<	4.29
Decision Constraints					
European polities	1.96	=	2.37	<<<	3.10
Latin polities	1.48	<	1.80	=	2.11
Historical polities	1.78	=	2.03	=	1.93
Continuing polities	1.50	=	2.10	<<<	3.40
Participation					
European polities	3.00	=	2.86	=	3.35
Latin polities	3.42	=	3.31	=	3.80
Historical polities	3.34	=	3.16	=	2.79
Continuing polities	2.25	=	2.60	<<<	3.86
Directiveness					
European polities	4.28	=	3.92	=	3.94
Latin polities	4.50	=	4.40	=	4.45
Historical polities	4.50	=	4.62	=	4.69
Continuing polities	3.50	=	2.80	<<<	3.64
Complexity					
European polities	4.08	<<	5.00	<<<	6.10
Latin polities	3.63	=	4.01	=	4.00
Historical polities	4.02	<	4.46	=	4.26
Continuing polities	3.75	=	4.53	<<<	6.36

= Means not significantly different using a two-tailed *t*-test and pooled variance.
< Significantly different at .10.
<< Significantly different at .05.
<<< Significantly different at .01.

[a] The Afro-Asian subgroup is not tabled because of the small *N* of cases in its low and high durability subgroups (9 and 5 respectively); there are no significant differences among any of its subgroup means.

[b] Authority indicator scores used in these comparisons are the average of scores at polity birth and termination. The dimensions are defined operationally in Table 1.

[c] Maximum numbers of polities in each durability subgroup are shown under Openness of Recruitment; the *N*'s actually used in dimensional comparisons sometimes are a few less because of missing authority data.

Table 10. System Authority Types as Determinants of Polity Durability: Correlations[a]

Authority Types[b]	Correlations with: Persistence[c]	Adaptability
Degree of Democracy		
All polities	.12**	.11
European polities	.35***	.19**
Latin polities	−.04	.02
Afro-Asian polities	−.18*	−.23**
Historical polities	−.13**	−.04
Continuing polities	.35***	.20**
Degree of Autocracy		
All polities	.08*	−.06
European polities	−.01	−.03
Latin polities	−.07	−.13*
Afro-Asian polities	.31***	.11
Historical polities	.21***	.01
Continuing polities	−.31***	−.22**
Degree of Anocracy		
All polities	−.04	−.09*
European polities	−.16	−.12*
Latin polities	−.04	−.04
Afro-Asian polities	.20*	.03
Historical polities	.10*	−.09*
Continuing polities	.33***	.26**

* significant at .10.
** significant at .05.
*** significant at .01.

[a] Pearson product-moment *r*'s.

[b] System indicator scores used in these correlations are the average of scores at polity birth and termination. The system indicators are defined operationally in Table 2. For *N*'s of polities see Table 9.

[c] Actual and estimated persistence scores with square root transformation.

greater persistence and adaptability than do polities with anocratic or mixed authority patterns. The comparisons of Table 12 provide a rather persuasive answer. Without exception, the average persistence of the highly coherent polities exceeds the persistence of the other two subsets. Recall that the persistence scores are $\sqrt{\text{years}}$: thus in the all-polity comparison the highly coherent polities have a mean lifespan of $7.45^2 = 55$ years, compared with 21 years for anocratic polities and 24 years for "incoherent" ones. Among the historical polities, whose evidence repeatedly falsified the previous two hypotheses, the difference in persistence is also substantial, even if not statistically significant: coherent historical polities had an average lifespan of $5.59^2 = 30$ years, compared with 20 years for the less coherent sets.

Coherence of authority patterns predicts less well to adaptability. Four of the 11 comparisons of adaptability means show coherent polities to be significantly more adaptable than others, four other comparisons are in the predicted direction. But in the remaining three, including the very numerous "historical" polities, incoherent polities prove to have undergone more adaptations in authority than either coherent or anocratic ones. This outcome seems to support Eckstein's suggestion that social units with inconsistent authority patterns are subject to pressures to modify those patterns.[27] In these data, large and abrupt modifications in authority patterns are reflected in polity termination, hence curtailed persistence; but if such modifications are incremental, they are registered in higher adaptability scores.

Summary and Conclusion

This is a study of political persistence and change, based on the codification of some basic authority characteristics of 336 political systems—polities—as they have functioned in 91 nation-states between 1800 and 1971. The persistence and change of these polities were defined in terms of their authority characteristics. By definition, polities persist so long as they avoid abrupt, fundamental change in their authority characteristics. When such change does occur, it signals the end of a polity and, usually, its transformation into another. The persistence of polities in years is, in effect, a measure of the periodicity of abrupt, major changes of system. The structures of political systems may be transformed other than by abrupt, step changes, of course. The nature and number of gradual changes in the 336 polities were also registered in this study and used to construct a measure of their adaptability. Durability is the term employed here for the variable capacity of political systems both to persist and adapt. As might be expected, the two properties are positively correlated.

These are some of the more striking descriptive generalizations from the data:

(1) The incidence of system-transforming political change has been remarkably high and pervasive, not only or mainly in the Third World but in the European culture zone as well. The median persistence of historical Latin and Afro-Asian polities was 12.3 years; in the European nations it was 12.2.

(2) The incidence of abrupt political change has increased markedly from the 19th to the 20th century. Of the 150 historical polities in the sample which were established before 1900, half survived for 20 years; but for the 117 historical

[27] "Authority Relations and Governmental Performance," pp. 315–322.

Table 11. Relative Durability of Democratic, Autocratic, and Anocratic Polities[a]

Clusters	Democracies		Autocracies		Anocracies
All polities, *N*'s:	57		82		45
$\overline{M}$ Persistence[b]:	6.42	=	6.17	=	5.01
$\overline{M}$ Adaptability:	1.54	=	1.09	=	.96
European polities, *N*'s:	37		38		7
$\overline{M}$ Persistence:	7.77	>	5.95	=	4.52
$\overline{M}$ Adaptability:	2.24	=	1.47	=	1.71
Latin Polities, *N*'s:	12		24		26
$\overline{M}$ Persistence:	3.80	=	4.54	=	4.13
$\overline{M}$ Adaptability:	.33	=	.79	=	.92
Afro-Asian polities, *N*'s:	8		20		12
$\overline{M}$ Persistence:	4.10	<	8.52	=	7.22
$\overline{M}$ Adaptability:	.13	=	.70	=	.58
Historical polities, *N*'s	30		54		42
$\overline{M}$ Persistence:	3.34	<<	5.74	=	4.56
$\overline{M}$ Adaptability:	.57	=	1.06	=	.69
Continuing polities, *N*'s	27		28		3
$\overline{M}$ Persistence:	9.84	>>>	6.99	<<<	11.37
$\overline{M}$ Adaptability:	2.63	>>	1.14	<<<	4.67

See Table 9 for explanation of symbols.

[a] Including only those polities with system scores of 3.5+.

[b] The persistence means are square roots of persistence in years.

polities established after 1900, the "half-life" was only nine years.

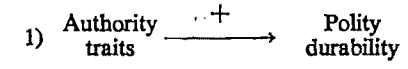
(3) Despite the heightened incidence of political transformation in the 20th century, the 69 polities which were operative in 1971 had already survived, on the average, longer than the full lifespan of any historical group of polities. Their median attained age in 1971 was 22. This is to say that the contemporary polities (excluding those of the post-World War II "new nations," which are not surveyed here) are not simply fortuitous survivors whose turn for political transformation will shortly come. Rather they include a substantial number of systems which have been able to adapt to their environments more successfully than their predecessors.

(4) Though there are distinct differences in the authority characteristics of polities of different regions, the most striking differences among clusters of polities are temporal. The single most substantial change in authority over time has been in Directiveness, i.e., the scope of governmental activity. These data document the generally-recognized decline of the "minimal state" which prevailed in the 19th century and the growth of "activist" polities of both autocratic and democratic form. Another expected trend of similar importance, though lesser degree, is toward institutional democratization. Marked increases were found between the 19th- and the mid-20th-century polities in the extent of "democratic" authority characteristics such as institutional constraints on chief executives and the openness of the processes by which they attained office. But levels of political participation, that other bellwether of pluralist democracy, did not change significantly over time. In short, the typical 19th-century polity was an autocracy with minimal functions. The typical mid-20th-century polity was either an activist plural democracy or, only slightly less likely, an activist autocracy.

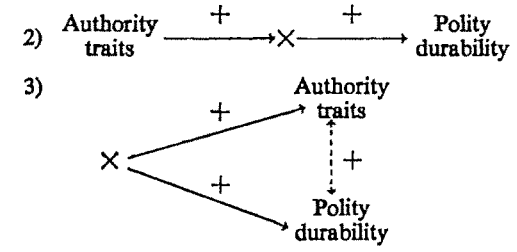
The hypotheses tested here all derive from the literature on "political stability," with specific reference to alleged relationships between the authority characteristics and the durability of polities, rather than to the ecological and cultural variables that have attracted much recent empirical attention. "Institutionalization" arguments about the stability-enhancing effects of systems' Complexity and Directiveness received no consistent support. Conventional beliefs about democracies being relatively more durable than autocracies, or anocracies, received not much more support. The more Complex, Directive, and democratic systems did prove to be consistently more persistent and adaptable *in the European culture zone and in the 20th century*. But then this is the time and place which suggested such hy-

potheses, and in which those hypotheses have most often been tested. When the hypotheses are tested in other regions and in the 19th century, they not only fail to gain support but are often flatly contradicted. The most striking disconfirmation is provided by the analysis of all historical polities, for all regions combined. For them, autocratic and anocratic authority traits proved to *enhance* durability while democratic traits *reduced* it.

A basic principle of causal modelling helps make sense of these results. If specific traits of authority enhance the durability of polities in one era and region but reduce it in others, then the relationship between the traits and durability is conditional, intervening, or spurious. Diagrammatically, this kind of "causal" sequence must be rejected:



Instead, one of these two causal models is likely:[28]



Of course, this is what the "historical determinists" and "ecological determinists" have been claiming all along: that the association of specific authority traits like democracy or Complexity with political stability is a function of particularistic developmental sequences, or certain kinds of socioeconomic class relationships, or the extent of material wealth, or high levels of literacy and communication, or the presence of cross-cutting rather than reinforcing social cleavages, or—the list could be expanded indefinitely. The problem is not a lack of specification of what "X" is but overspecification. Lack of space and data preclude reformulating and testing any of the plethora of historical and ecological hypotheses here, but Hurwitz has summarized some recent empirical evidence.[29] The search for universal linkages between authority characteristics of political systems and their durability ought not to be abandoned prematurely, either. The third hypothesis tested here attributes the durability of systems not to specific authority traits but to the internal consistency or "coherence" of those traits. In operational terms, the prediction was that highly—consistently—democratic *and* highly autocratic political systems ought to have been more durable than systems of "mixed" authority traits. Unlike the other hypotheses, this one received consistent support. Most important, the relationship was strong among the historical polities, which provided a reef of contradictory evidence on which the other two hypotheses foundered. The evidence is unmistakable that the more closely a polity resembles a pure democracy *or* pure autocracy, as defined here, the longer it is likely to have persisted.

The "coherence" hypothesis is derived from Eckstein's "congruence-consonance" theory of political performance. The theory, very briefly, is that political institutions perform—and endure—best if (a) their authority patterns are congruent with the authority patterns of "adjacent" social institutions, and (b) their authority patterns are internally consonant.[30] The premises of congruence-consonance theory also suggest that consonance is less consequential than congruence as a condition of high performance. "Coherence" is only one facet of consonance. And the operationalization of "coherence" here is based on indicators which permit only gross discrimination among authority characteristics. Despite these conceptual and operational slippages, the hypothesis is supported. Inasmuch as the hypothesis is deduced by Eckstein from the same set of assumptions on which the "congruence" hypothesis depends, congruence-consonance theory as a whole is indirectly supported by the results.

Time and again the evaluation of the evidence summarized above led to speculation about long-term trends in "political development." A few generalizations about such trends have been mentioned in passing but most have been held in abeyance pending longitudinal analyses of the data. It may be worth outlining, however, in a tentative way, a "developmental" interpretation of the political changes surveyed here from 1800 to the present. The typical state at the onset of the

[28] It is assumed here that the variables represented by "region" and "era" control the effects of authority traits on durability, and that there is no consistent relation between "region" or "era" and durability. If these assumptions are not warranted, then other causal sequences are also possible. See, for example, Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1964), and Hayward R. Alker, Jr., *Mathematics and Politics* (New York: Macmillan, 1965), chap. 6.

[29] See Leon Hurwitz, "Democratic Political Stability: Some Traditional Hypotheses Reexamined." *Comparative Political Studies*, 4 (January, 1972), 476–490; and "An Index of Democratic Political Stability: Methodological Note." *Comparative Political Studies*, 4 (April, 1971), 41–68.

[30] "Authority Relations and Governmental Performance."

Table 12. Relative Durability of Polities with Highly Coherent, Anocratic, and Mixed Authority Patterns[a]

Clusters	Mean Durability Scores for Highly Coherent Polities[b]		Anocratic Polities[c]		Incoherent Polities[d]
All Polities, N's:	81		38		175
$\bar{M}$ Persistence:	7.45	>>>	4.53	>>>	4.87
$\bar{M}$ Adaptability:	1.60	>>>	.32	>	1.16
European polities, N's:	49		5		70
$\bar{M}$ Persistence:	8.06	>>>	3.01	>>>	4.31
$\bar{M}$ Adaptability:	2.20	>	.20	>>>	1.12
Latin polities, N's:	16		24		73
$\bar{M}$ Persistence:	4.97	=	3.77	=	4.82
$\bar{M}$ Adaptability:	.81	=	.42	=	1.33
Afro-Asian polities, N's:	16		9		32
$\bar{M}$ Persistence:	8.07	=	7.38	=	6.23
$\bar{M}$ Adaptability:	.56	=	.11	=	.84
Historical Polities, N's:	37		37		151
$\bar{M}$ Persistence:	5.59	=	4.49	>	4.47
$\bar{M}$ Adaptability:	.78	=	.32	=	1.12
Continuing polities, N's:	44		(too few cases		24
$\bar{M}$ Persistence:	9.02		for comparison)	>	7.40
$\bar{M}$ Adaptability:	2.30			=	1.42

See Table 9 for explanation of symbols.

[a] The persistence means are square roots of persistence in years. Note that the tests of significant difference shown under the "incoherent polities" heading are for the comparison between highly coherent and incoherent polities, not between anocratic and incoherent polities.

[b] Polities with democracy scores of 4.5+ or autocracy scores of 5.0+.

[c] Polities with anocratic scores of 4.0+.

[d] All polities not included in the "highly coherent" and anocratic subsets.

19th century was a long-lived, traditional autocracy. Some were very large, like the Spanish, Ottoman, Austro-Hungarian, Czarist and Manchu empires. Others, like pre-Tokugawa Japan and Persia, as well as many smaller European monarchies, were rather smaller. As systems, they had distinct characteristics in common: a high degree of centralization and institutionalization of political power, but few responsibilities for social and economic activities; a lack or suppression of "modern" means of interest articulation; and a lack of adaptability. They also had a common fate: the only traditional autocracy which in 1971 remained essentially unchanged was the Empire of Ethiopia, and now it too is passing. In almost every instance one can trace the demise of these traditional states to their inability to cope with the stresses generated by socioeconomic "modernization."

Largely because of the stresses of modernization, the 19th and early 20th centuries were times of great political experimentation, not only in the "new nations" of the Americas but in older European and Asian societies. There were innumerable "experiments" in those regions, both in the establishment of new nations—120 years ago neither "Germany" nor "Italy" existed as political entities—and in devising new political systems. The very high incidence of political mortality apparent in these data reflects this experimentation. But out of this trial-and-error process, increasing numbers of viable, adaptable political systems began to emerge, so that by the 1970s it is possible to speak with some confidence about the durability of "new" systems as diverse as those of the United States and the USSR. The most salient characteristics of these "modern" systems is not their democracy or autocracy, however, but their Directiveness. The most durable political systems now are those which have responded to the stress of socioeconomic change by assuming the task of managing it, whether directly by state control of the institutions of socialization and production, or indirectly by regulation and support of allied

but quasi-autonomous institutions. The "totalitarian" and "social democratic" polities, in short, seem to be variant means to the same end of political survival through managed change. The difference between them seems—at least conceptually—less great than the difference between the directive and the minimal state.

I do not suggest that socioeconomic modernization necessarily generates a process of polity selection which culminates in the establishment of durable and directive political systems. In 150 years of Latin American political independence and experimentation only one such polity, that of contemporary Mexico, has evolved, even though Latin America has undergone more substantial socioeconomic change in that span than any world region other than Europe and Anglo-America.[31] This also seems the place to raise a skeptical eyebrow about the assumption that the "durable" polity is necessarily a good thing. If durable political arrangements in the "modern" world also require a highly directive and powerful state, many people might regard frequent, abrupt political change as the lesser evil. One could speculate that Latin America has a common political culture based on that very premise.

One final comment: This speculative conclusion is not intended as a forecast that highly-directive polities are destined to prevail indefinitely where they now have a firm foothold, namely in the European culture zone and Asia. They have demonstrated a capacity to survive and adapt in periods of rapid and continuous socioeconomic change. But whether they can adapt and survive in times of stasis and decline seems now a moot question. Or perhaps it is not moot. In one of the most provocative recent books in political science, Robert G. Wesson has surveyed the rise and decline of large, powerful states across five millennia. One of his observations coincides precisely with conclusions here about the scope of power in most contemporary states: it is that "Societies suggestive more of imperial Rome than of democratic Athens largely fill history and our own world."[32] Our convergent observations can be linked to one of his principal theses and conclusions about the fate of such polities: "Imperial decay is . . . primarily a result of the misuse of power which follows inevitably from its concentration."[33] The observation is at least a forecast if not a prophecy, and it might prove to be an epitaph.

[31] There are other activist polities in Latin America now than Mexico, of course; their ideologies and approaches are as diverse as those of Brazil, Peru, and Cuba. But they are all young, and whether they will endure and adapt as has the Mexican system is problematical; the "life expectancy" of Third-World polities at their inception is 16 years (Table 6).

[32] *The Imperial Order* (Berkeley and Los Angeles: University of California Press, 1967), p. 443.

[33] Wesson, *The Imperial Order*, preface (unnumbered).

On the Origins of Interest-Group Theory: A Critique of a Process*

G. DAVID GARSON
Tufts University

The history of political science is one of gradual encroachment of ideas rather than the sequential building of scientific study upon scientific study. Yet the general idea of a science of politics, or at least of an accepted general framework for political analysis, is as old as the discipline and promises to remain an enduring part of its professional ethos. Of the many general frameworks which have been advanced in recent decades few can claim the prominence of the body of ideas associated under the label of "interest-group theory."

In the early years of the postwar wave of interest in group theory, Avery Leiserson reviewed David Truman's modern classic, *The Governmental Process* (1951), predicting that political scientists would receive the work enthusiastically because of its utility as a framework for presenting "the great mass of empirical, historical, and descriptive materials that political science has accumulated about the 'realities' of political organization."[1] This prediction proved accurate and Truman's work, along with the interest-group approach to politics which it advocated, rapidly became established as a central part of what was perceived as a hopeful new period of growth within the discipline.

A decade later Robert Golembiewski could still write, "only small further innovations in method (if they imply much effort) are required to achieve a major research breakthrough in 'the group theory,' to push beyond what Bentley cannot be expected to have known and beyond his incomplete break with the restraints of the leading ideas of his time."[2] New excitement was attached to the development of interest-group studies on a comparative basis. Yet, as Graham Wootton noted recently, "the excitement engendered at Rome, Pittsburgh, and Geneva from the mid-fifties to the mid-sixties has long since evaporated." This decline, Wootton noted, was associated in part "with the discovery that, in certain contexts, neither pressure groups nor lobbyists enjoyed the importance popularly or even academically attributed to them."[3]

With this ebbing of intellectual interest, group theory finds its domain intruded upon by a legion of "newer" frameworks: systems theory, role theory, communications theory, symbolic interactionism, phenomenology, and many others. As with other great conceptions of political process, the decline of group theory has not been due to "scientific" refutation in the ordinary sense. Rather, it has subsided amid the accretion of other ideas, other issues, other terminologies.

In many ways the "scientific" appraisal of interest group theory is less important than seeking to understand this process of encroachment of ideas itself, as illustrated in group theory. The present essay focuses on this process, using as "data" a survey of all articles and book reviews pertaining to the group theory of politics appearing in the *American Political Science Review* since its inception in 1906. Articles and reviews were considered to fall into the purview of this essay if they (1) discussed the role of groups *qua* groups in political processes; (2) treated specific organizations in the nongovernmental sector; or (3) treated classes of such organizations (e.g., labor groups, church groups, farm groups). In addition, the works of twenty frequently-cited group theorists were included. The study was limited to consideration of non-governmental groups except where a governmental body was analyzed in group-theoretic terms.

Reading of the *American Political Science Review* since its beginning gives one a unique and interesting account of American political science thinking about groups. In spite of its pervasive effect on our discipline, this history today is remembered little if at all. This tendency to neglect the history of thought within our discipline reinforces the "growth" of political science through a process of accretion in which the shifting of basic premises, questions, and terms occurs largely without explicit or even conscious consideration.

The very purpose of theory is to simplify all-

* An earlier draft of this paper was presented at the 69th Annual Meeting of the American Political Science Association, New Orleans, September 4–8, 1973. This essay represent the first of three; the second treats group theory between 1930 and 1951 and the third, the period since 1951.

[1] Avery Leiserson, rev. of *The Governmental Process*, by David Truman, *American Political Science Review* (hereinafter *APSR*), 45 (December, 1951), 1192–1193.

[2] Robert J. Golembiewski, "The Group Basis of Politics: Notes on Analysis and Development," *APSR*, 54 (December, 1960), 971.

[3] Graham Wootton, "Interest Groups in Britain and the United States: Contributions to Public Policy," a paper prepared for the 68th Annual Meeting of the American Political Science Association, Washington, September 5–9, 1972, p. 1.

too-complex reality by discarding less important information until what is left can be utilized to understand and to predict. As the evolution of group theory illustrates, what is dangerous for a discipline is not selection *per se*, but rather the making of that selection without sufficient explicit consideration of the criteria for shifting premises, questions, and terms. All too often the intellectual modes and ideological needs of the times sweep over the discipline, followed by a "scientific" reaction seeking to insulate political science from such forces. Rather than seek a wedding of theory and practice, necessarily requiring a base in normative theory, the discipline vacillates between these poles. In this process the hopes for "scientific" advance are surrendered to the gradual encroachment of ideas, each of which in turn is treated as if it were better than the last, and the fittest theories are equated with those which have survived. The history of that process with respect to group theory is the focus of the remainder of this essay.

Pluralism and the Critique of Formal Jurisprudence

I believe it is commonly accepted among contemporary political scientists that the turn away from "nineteenth-century jurisprudence" was an unmitigated good. Illustrative is the following excerpt from a contemporary text on interest-group theory:[4]

> The fundamental characteristic of political science at this [Bentley's] time was its preoccupation with law. This meant that the legal aspects . . . embodied in formal constitutions served as an adequate guide to the realities of politics. . . . As David Easton rightly declares, 'Most tended to act as though the study of the distribution of power as delineated in the constitution constituted the core of political research.

The authors of this text emphasize the "institutional bias" of nineteenth-century American political scientists, and in this one must agree they are right as far as they go.

But there was much more to juristic political science than simply "narrow institutionalism." While institutionalism *does* serve to differentiate early political science from its later phases and to highlight Arthur Bentley's contribution in *The Process of Government* (1908), other aspects of jurisprudence relate very directly and in theoretically important ways to the group theory of politics. Paradoxically, it is these ways which are largely ignored by contemporary group theorists, who prefer to attack the straw-man of nineteenth-century institutional focus.

This is not to belittle the constraints which nineteenth-century institutionalism placed on political science. Bentley was merely one author among a great many who recognized this more than half a century ago. Among the most influential in those years was not Bentley, who was little known, but rather James Bryce. Bryce, in his presidential address to the annual APSA meeting in 1908 stated that, "Political institutions . . . used to be treated as abstract entities, if the term be permissible: things existing *in vacuo*, like the Platonic ideal, with a definite and permanent type."

In place of this perspective Bryce called on political scientists to study not only formal institutional aspects of politics, but also (1) "the needs it was meant to meet and the purposes it actually serves;" (2) "the character of the men who work it;" and (3) "the traditional color of the institution." These approaches, which today would be called functional, social psychological, and political cultural respectively, were very broad. Bryce noted, "You may ask: Are we then to study everything in the light of everything else? and if so, what limit can be set to investigation? The answer is: No limit. Every political organism, every political force, must be studied in and cannot be understood apart from the environment in which it plays."[5]

One senses in the phrase "No limit" the sense of liberation which came with the turn away from institutionalism in political science. Yet the debate about jurisprudence and the theory of sovereignty continued in the forefront of attention for more than a decade after the writings of Bentley and Bryce in 1908. Why was this so? I shall argue that it was not because of some mere residual inertia but rather that jurisprudential theory continued as a vital center of political thought precisely because its major theoretical concerns went beyond institutionalism alone.

The reader may wonder here what relation this has to group theory. Among the most important theoretical concerns to which I have alluded was the problem of the proper relation of groups in the state. In traditional jurisprudential theory that relation was prescribed as one of subordination. Moreover, traditional theorists customarily distinguished between this relation as norm and as practice. As Theodore Woolsey noted, "The quality of sovereignty, however, does not necessarily imply unlimited power or unchecked power; much less undelegated power."[6]

On subjects such as the relation of the state to religious groups, much attention was given to

[4] L. Harmon Zeigler and G. Wayne Peak, *Interest Groups in American Society*, 2nd ed. (Englewood Cliffs: Prentice-Hall, 1972), p. 5.

[5] James Bryce, "The Relations of Political Science to History and to Practice," *APSR*, 3 (Feb., 1909), 6–8.

[6] Theodore Woolsey, *Political Science, or The State* (New York: Scribner's, 1893), I, 203.

comparison of normative models in relation to divergent national practice.[7] Moreover, the symbolic uses of sovereignty were recognized, as was the gap between a power unchecked in theory and that power not expressed in practice.[8] Finally, the relation of the state to various interests was discussed under the rubric of theories of representation.

Juristic political scientists wrestled, for example, with John Calhoun's theory of the state as based on interests. This view, Calhoun wrote, "regards interests as well as numbers, considering the community as made up of different and conflicting interests, as far as the government is concerned, and takes the sense of each through its appropriate organ, and the united sense of all as the sense of the entire community."[9] This problem was discussed not only in terms of Calhoun's reference to the South as an interest, but also in terms of narrower groups such as the cotton interest or the sugar interest. In other contexts, even informal groups such as the family received extended treatment.[10]

Contrary to the impression given by Bryce and later political scientists, nineteenth-century authors routinely brought in historical and political evidence of a concrete nature to illuminate their normative theories, whether to support their applicability in practice or to demonstrate the divergence of theory and reality. As W. W. Willoughby wrote,

> It is thus seen that a complete system of Political Science includes three main divisions: first, the determination of fundamental philosophical principles; second, the description of political institutions . . . ; and third, the determination of the laws of political life and development, the motives that give rise to political action, the conditions which occasion particular political manifestations, the circumstances when certain forces are applicable, either for the good or for the bad.[11]

Willoughby, whom modern interest-group theorists have sometimes characterized as an extreme legalist,[12] was in no small part also an empiricist. He held, for example, that "Not until a sufficient number of facts have been observed, and the consequences of cause and effect in political life remarked, is the formation of adequate philosophical conceptions possible. When formed, however, these conceptions serve to explain and harmonize the facts that have appeared confused and contradictory."[13]

In considering the twentieth-century reaction against jurisprudence among American political scientists, one must remember that the theoretically important contrast at the time (and now) was not the shift in substantive focus (governmental bodies and political parties remained the central emphasis) nor even the rise of empirical description as a preferred style. Rather, controversy centered on the crisis of the normative theory of the state advocated by most juristic political scientists. More specifically, the crisis was precipitated by changing views about the relation of organized groups to the sovereign state.

Latham is absolutely correct in emphasizing the origins of the modern group theory of politics in the pluralist critique of sovereignty.[14] That critique, however, is less interesting for its rebuttal of the straw-man of the absolutist state into which the pluralists (and implicitly, Latham) simplified the theory of sovereignty than for its implications for the normative theories of the day. Contrary to frequent assumptions, the pluralist critique assumed importance because of its prescriptive rather than descriptive aspects, though the two were intertwined. Before turning to this subject, however, it is necessary to treat two additional, more popular intellectual currents which underlay pluralism and gave it no small part of its momentum.

Pressure Politics, Economic History, and Pluralism

With the consolidation of corporate capitalism and the rise of organized labor in the late nineteenth century, American social scientists were increasingly confronted with the contesting claims of economic groups as these vied for popular support, institutional position, and the mantle of government power. The activities of these groups seemed to contravene with some frequency the norms of political process held by political scientists and, indeed, most Americans.

Among some of the earliest works that dealt explicitly with interest groups in the pages of the *American Political Science Review* were those treating pressure politics. Although called "muckrakers" by their opponents, these authors shared many of the perspectives found in the writings of leading political scientists such as James Bryce.[15] For example, in a review of Lincoln Steffens's *The*

[7] Woolsey, II, ch. 12.
[8] Woolsey, I, p. 203.
[9] Quoted in Elisha Mulford, *The Nation: The Foundations of Civil Order and Political Life in the United States* (Boston: Houghton Mifflin, 1881; orig. 1870), p. 212. See also Woolsey, II, 292 ff.
[10] Mulford, ch. 15.
[11] W. W. Willoughby, *An Examination of the Nature of the State* (New York.: Macmillan, 1896), pp. 382–383.
[12] Zeigler and Peak, p. 6.
[13] Willoughby, p. 383.
[14] Earl Latham, "The Group Basis of Politics: Notes for a Theory," *APSR*, 46 (June, 1952), 376–397.
[15] Louis Filler, *The Muckrakers: Crusaders for American Liberalism* (Chicago: Regnery, 1968; orig. 1950), p. 17.

Struggle for Self-Government (1906), S. E. Sparling endorsed the call for reform, noting "The value of Mr. Steffens' volume consists largely in a vivid portrayal of conditions which have made possible the work of the system—a composite of social, political, and business interests. . . . The alertness of business interests in politics, coupled with the supineness of the people, has done much to make possible the conditions pictured in this volume."[16]

The new attention given to pressure politics was not an abstract methodological change but was associated with a quite specific historic moment. In his *The American Commonwealth* (1894), Bryce had forecast that the time would come when "the chronic evils and problems of old societies and crowded countries will have appeared on this soil." In the 1910 edition of the book, Bryce felt it appropriate to add that "the demand of the multitude to have a larger share of the nation's collective wealth may well have grown more insistent." Henry Jones Ford, in reviewing this edition, drew attention to this insertion stating "The remark notes a sign of the times that portends a new pressure upon our political institutions which is already manifesting itself and which must deeply affect constitutional development."[17]

This "new pressure" soon translated itself into new perspectives on American government. By 1915 one may find a basic text such as James T. Young's *The New American Government and Its Work* (1915) starting not with the discussion on the legal framework that was nearly universal among earlier texts but rather with a discussion of the influence of business groups on political processes, contrasting actual practices with legal norms. In this shifting perspective the normative emphasis of the nineteenth century was retained, not at the level of legal philosophy, but rather at the more practical level of political reform.

In his presidential address to the 1916 annual meeting of the American Political Science Association, Jesse Macy called on his colleagues to reflect the new "scientific spirit in politics."[18] By this Macy meant not value-free methodology but the contrary idea of applying the knowledge of political science to the great tasks of reform, notably the attempt to control selfish interest groups. Similar views were then being expressed in sociology by Macy's contemporary, Albion Small, one of Arthur Bentley's teachers at the University of Chicago.

A more elegant theoretical framework for the study of economic pressures was presented in the work of Charles A. Beard, later a president of the American Political Science Association. Contrary to popular conceptions, Beard was neither an economic determinist nor a Marxist but instead professed belief in the views of Madison in *Federalist No. 10*, excerpted below:[19]

> The diversity in the faculties of men, from which the rights of property originate, is not less an insuperable obstacle to a uniformity of interests. The protection of these faculties is the first object of government. From the protection of different and unequal faculties of acquiring property, the possession of different degrees and kinds of property immediately results; and from the influence of these on the sentiments and views of the respective proprietors, ensues a division of society into different interests and parties . . .

Beard's influential views were expressed in *An Economic Interpretation of the Constitution of the United States* (1913), *Economic Origins of Jeffersonian Democracy* (1915), *The Economic Basis of Politics* (1922) and later works. Beard interpreted party development in terms of group interests, attacking Bryce's idealist emphasis on political principles (e.g., centralism versus states' rights). He noted that parties were prone to switch rhetorical ideals when interest dictated.[20]

In addition to Beard, four other economic historians had their works reviewed in the *American Political Science Review* during this period, including Alvord, who likewise emphasized American politics as a reflection of factional interest apart from principle.[21] Finally, other related historical works not reviewed, such as Gustavus Myers's *The Great American Fortunes* (1910), achieved widespread popularity at this time and may therefore be presumed to have contributed to the intellectual climate of American political science when Harold Laski was presenting the pluralist critique of sovereignty to this audience.

At the core of the pluralist critique was the argument that the state was not sovereign as jurisprudential theory held, but rather that the state was one of many associations in society. Associations other than the state, such as the church, could be shown empirically to be sovereign over important matters in which the state could not in fact intervene, contrary to traditional theory about absolute sovereignty being vested in the state. The writings on pressure politics and economic history dovetailed nicely with pluralist

[16] S. E. Sparling, review of *The Struggle for Self-Government*, by Lincoln Steffens, *APSR*, 2 (November, 1908), 658–659.

[17] Henry Jones Ford, rev. of *The American Commonwealth*, by James Bryce, *APSR*, 5 (August, 1911), 476–477.

[18] Jesse Macy, "The Scientific Spirit in Politics," *APSR*, 11 (February, 1917), 1–11.

[19] Cited in Charles Beard, *An Economic Interpretation of the Constitution of the United States* (New York: Macmillan, 1913), pp. 14–15.

[20] Charles Beard, *The Economic Basis of Politics* (New York: Vintage, 1957; orig. 1922), pp. 158–9.

[21] Clarence W. Alvord, *The Mississippi Valley in British Politics* (Cleveland: Clark, 1917), vols. I and II.

theory, documenting the weakness of the state in exerting jurisdiction over economic interests.

The Rise and Fall of Classic Pluralism

Because of the intellectual currents just discussed, much of the pluralist critique of sovereignty was accepted implicitly even before Laski performed the pleasant task of rendering explicit an idea whose time had come. The pluralists portrayed the jurisprudential theorists as believing that absolutist state jurisdiction was both desirable and actual. This portrayal was an injustice, since the jurists devoted a large proportion of their attention to discussing the desirability of limited government and to the distinction between ideal and reality. Only by an unrepresentative selection from their works is it possible to portray them as advocating "a sort of mystic monism as the true path of thought."[22]

In contrast to this alleged monism, Laski presented the "pluralist theory of the state" as a more scientific view:[23]

> So I would urge that you must place your individual at the center of things. You must regard him as linked to a variety of associations to which his personality attracts him. You must on this view admit that the State is only one of the associations to which he happens to belong, and give it exactly that preeminence—and no more—to which on the particular occasion of conflict its possibly superior moral claim will entitle it. In my view, it does not attempt to take that preeminence by force; it wins it by consent. It proves to its members by what it performs that it possesses a claim inherently greater than, say, their church or trade union. It is no dry *a priori* justification which compels their allegiance, but the solidity of its moral achievement.

In reviewing Laski's work, Walter Shepard (later to become APSA president) called it "an important contribution" representing a "brilliant school of political writers" who were developing a "more satisfactory theory of the nature of the state."[24]

On the surface, pluralism was a devastating rebuttal of traditional theory. The jurists *did* claim the state was sovereign absolutely; partial sovereignty *was* considered a contradiction in terms. Yet, as Laski showed, the Hegelian concept of the omnicompetent state was abandoned in practice during nineteenth-century struggles over such matters as the independence of religious groups from state intervention.[25] Moreover, this conception of the state created a framework for interpretation of the increasingly important economic pressure groups which appeared at this time. Finally, and not least in Laski's own motivation, it provided a rationale for protecting the growth of trade unionism from the intervention of a generally conservative and unsympathetic state. Later, when progressive preferences emphasized the need for a liberal government to intervene in business affairs Laski adapted his views accordingly, abandoning the pluralist defense of group autonomy.

At a deeper level, however, the pluralist critique was premised on insufficiently examined assumptions about "absolutist" theories of sovereignty. In a relatively short space of time American political scientists recognized this and abandoned the theory, though in the process juristic political science suffered blows from which it never recovered.

The concept of "absolute sovereignty" put forward by nineteenth-century scholars was akin to what would later be called an "ideal type." It was neither a normative model nor a description. The normative theory of these scholars, not only American political scientists but also classic theorists such as Bodin and Hegel, led them to constitutionalism, not absolutism. Further distinction was made between constitutional theory and actual practice, though perhaps not to the degree scholars today would desire.

For Laski and the pluralists to say that "absolute sovereignty" was not an accurate description was to misconstrue the nature of jurisprudential theory. Moreover, what Laski presented as an accurate description was *itself* a normative model. In the passage excerpted above, for example, it is not a simple observation of fact to assert that the state gains the allegiance of and preeminence over its citizens by "consent" in a form of transaction in which it "proves" the superiority of its benefits, achievements, and performance as compared with other groups. This concept of government by uncoercive consent through the citizen's rational calculation of comparative group achievements is a sort of neo-utilitarian normative model, not an empirical description.

The normative model latent in classic pluralism suggests an important element in understanding the evolution of American political science. The jurisprudential approach had the potential for evolving the following paradigm: (1) to clarify the nature and utility of ideal types in political science; (2) to define more rigorously both alternative normative models of the state, and the role of

[22] Harold Laski, *Studies in the Problem of Sovereignty* (New Haven: Yale University Press, 1917), excerpted in *Leviathan in Crisis*, ed. Walter R. Browne (New York: Viking, 1946), p. 113.

[23] Laski, *Studies*, in Browne, p. 120.

[24] Walter Shepard, rev. of *Studies in the Problem of Sovereignty*, by Harold J. Laski, *APSR*, 11 (August, 1917), 582–583. The school to which Shepard alludes includes Figgis, Maitland, Duguit, von Gierke and others on whom Laski drew. Because of space limitations only Laski is considered here, because of his pre-eminence in interpreting pluralist theory to American political scientists.

[25] Cf. Harold Laski, "The Political Theory of the Disruption," *APSR*, 10 (August, 1916), 437–464. Laski frequently used church history for examples, generally as an analogue for the history of trade unions.

groups within it; and (3) to compare the degree to which various social systems combined various degrees of fulfillment of the different models. Instead, after Laski, pluralism led into the search for the one accurate descriptive framework, to the obscuring of distinctions between norm and practice, and to the isolation of political theory from much of the rest of political science. The banner of empirical description was raised, frequently masking, as in Laski's case, an implicit and unexamined normative system.

Laski Abandons Pluralism. The group theory of the classic pluralists provided a defense of associational autonomy. While pluralism was at first hailed by American political scientists as superior in its descriptive aspects, this quickly ceased to be the issue (not surprisingly, since the jurists refused to consider the pluralist description as antagonistic to their normative constructs) and debate turned to the implicit normative aspects of pluralism. Ironically, these prescriptive implications gave the controversy its depth and greatest theoretical significance.

In Laski's day, American political scientists were not generally oriented toward theories of group autonomy. Rather, they shared some version of the jurists' norm of the pre-eminence of the state over factions and groups within it, albeit constrained by vaguely defined rights of individuals and groups. In this they were in some ways closer to Bertrand Russell than to Laski, specifically Russell's emphasis on the danger of political power in the hands of undemocratic private groups.[26] The norms of scientific reform were not those of pluralism.

Thus, in reviewing Laski's subsequent work, *Authority in the Modern State* (1919), Walter Shepard posed the question, "How, it may be asked, are conflicts between opposing groups to be adjusted except upon the basis of the supreme will of the state? It is in affording an answer to this question that Mr. Laski is perhaps least convincing."[27] Not only did questions arise regarding the normative premises of pluralist group theory, but its description fell under attack as well. In an article on "The Pluralistic State" the following year, Ellen Deborah Ellis acknowledged that pluralism was "timely in that it calls attention to the present bewildering development of groups within the body politic, and to the fact that these groups are persistently demanding greater recognition in the governmental system." But then she added that pluralism as a description was inaccurate, applying only to a polity in "a very advanced state of disorganization."[28]

A sharper critique by Francis Coker appeared a year later. Coker noted that the limited status of government was beside the point: *everyone* agreed on that. To construe the theory of sovereignty as holding government to be unlimited was to misinterpret it.[29] Coker went on to find in pluralism the normative models underlying professional representation, administrative democracy, administrative syndicalism, distributivism, guild socialism, and syndicalism.[30]

The latent radical implications of pluralism had some influence in political science in the early 1920s. Group autonomy, if it were not to be the private tyranny forecast by Russell, had to be accompanied by democratization of hitherto autocratic organizations. Laski himself avoided commitment to this path, phrasing his advocacy of pluralism in more abstract terms which implied only some sort of functional democratic organization.[31] Traces of Laski's philosophy are discernible in work in this period on international law,[32] administrative democracy,[33] and guild socialism.[34] These traces of influence were few, however, compared with the general criticism of pluralism on both normative and descriptive grounds.

In this debate, George Sabine tried to stake out a middle ground, holding "The one clear lesson from these disputes is that both monism and pluralism are inescapable aspects of our world."[35] Monism was faulted for implying that conflict among groups was settled by law, while pluralism was criticised for implying such conflict was settled by negotiation, when in fact both processes occurred. "For my part, then," Sabine wrote, in 1923, "I must reserve the right to be a monist when I can and a pluralist when I must."[36] Yet this formulation clearly favored "monist" norms while finding both approaches inadequate in terms of empirical description.

The last major articles on pluralism to appear in the *American Political Science Review* (1907–

[26] Bertrand Russell, *Political Ideals* (New York: Century, 1917).

[27] Walter Shepard, rev. of *Authority in the Modern State*, by Harold Laski, *APSR*, 13 (August, 1919), 493.

[28] Ellen Deborah Ellis, "The Pluralistic State," *APSR*, 14 (August., 1920), 407.

[29] Francis Coker, "The Technique of the Pluralist State," *APSR*, 15 (May, 1921), 194–195.

[30] Coker, p. 197.

[31] Cf. Harold Laski, *The Foundations of Sovereignty* (New York: Harcourt, Brace, 1921); Walter Shepard, rev. of *The Foundations of Sovereignty*, by H. Laski, *APSR*, 16 (February, 1922), 130–131.

[32] Baron S. A. Korff, "The Problem of Sovereignty," *APSR*, 17 (August, 1923), 404–414.

[33] Jennie McMullin Turner, "Democracy in Administration," *APSR*, 17 (May, 1923), 216–230.

[34] Niles Carpenter, *Guild Socialism: An Historical and Critical Analysis* (New York: Appleton, 1922).

[35] George Sabine, "Pluralism: A Point of View," *APSR*, 17 (February, 1923), 34.

[36] Sabine, 50.

1927) returned to Coker's linkage of pluralism to radicalism, criticizing both. Ellis, for example, noted the connection between pluralism and guild socialism, associating these ideas with discontented groups who naively seek the overthrow of the state as well as the government, not understanding the difference.[37] W. Y. Elliot also analyzed Laski's work in terms of its radical implications, concluding, "It is curious that Mr. Laski can arrive at a conclusion which strips the majesty from the law without seeing that he is rendering the very individuals helpless whom he set out to protect against the absolute sovereignty of the absorptive state."[38] With the growing anxiety over fascism and communism, Elliot returned to this theme in a later essay in 1925, castigating pluralism as "as appeal to that violence which M. Sorel has eulogized, whose only immediate issue is the reappearance of a sovereign equally despotic, whether he be blackshirted or red."[39]

By the time Elliot's second attack appeared, however, Laski himself had abandoned pluralism. In *A Grammar of Politics* (1925) Laski's focus had shifted away from the sovereignty of groups to the way groups exert "the thousand varying influences which go to shape the sovereign will."[40] Rather than advocate guild socialism, much less the revolutionary syndicalism implied by Elliot, Laski moved toward favoring government regulation of the private sector through representative councils made up of management, worker, consumer, and government delegates. The orders of these councils would require the approval of a central Ministry of Production and would be subject to the control of the parliament. In later years Laski moved increasingly toward a state planning solution to the problem of coordination of groups within the sovereign state, embracing "monist" concepts far more than most of his earlier critics.

Bentley and Descriptive Political Science

In the wake of the antiradical hysteria and intellectual disillusionment which followed World War I, the idealism of reformism and jurisprudence seemed out of place, even though their erstwhile challenger, pluralism, became discredited because of its flawed conception and, more important, its normative (radical) implications. In the prestige of science and the practicality of empirical description many saw a desirable new direction for the growth of political science.

[37] Ellen Deborah Ellis, "Guild Socialism and Pluralism," *APSR*, 17 (December, 1923), 584.

[38] W. Y. Elliott, "The Pragmatic Politics of Mr. H. J. Laski," *APSR*, 18 (May, 1924), 259–260.

[39] W. Y. Elliott, "Sovereign State or Sovereign Group?," *APSR*, 19 (August, 1925), 499.

[40] Harold Laski, *A Grammar of Politics* (New Haven: Yale University Press, 1925), p. 9.

The new concern for descriptive political science in the 1920s had roots in the comparison of practice with ideal in jurisprudence, in the practical concerns of the reformers, and in the empirical historical research of the economic historians and of the pluralists (e.g., the history of their church-state relations). Precedents could be found, for example in Woodrow Wilson's *Congressional Government* (1885) and its attempt to de-emphasize law in order to focus on "realistic" analysis of "the most characteristic practical features of the federal system."[41]

Another forefather of descriptive science was Bentley's teacher, Albion Small. Like Beard and Laski, Small was an exacting historical scholar.[42] But where Wilson treated fragmentation in *government*,[43] Small focused on the social fragmentation of every large *society* made up of groups, each having particular ideas and feelings.[44]

Small's writings contain many of the central points of interest group theory: (1) society conceived as composed of a large number of groups; (2) no one of which can claim to represent the general will; hence, (3) the need for elections to determine a rough approximation of the collective volition; (4) determined by group forces at various stages of the political process; (5) in the context of a state characterized as a relatively neutral agency for determining and implementing collective decisions made through this process, which was seen as neutral in the sense of absence of elite or class domination.

Also prefiguring later interest-group theory was Small's contrast between normal group process and the debilitating effects of political protest outside conventionally accepted channels. Indicative is Small's treatment of labor strikes and other social movement forms under the heading of "The Pathology of Social Organs—Characteristics of Social Disease."[45] The "pathology" of labor strikes, for example, was contrasted with the "genuine reform" associated with "social amelioration," philanthropy, and specific efforts such as Civil Service reform. Small warned fellow sociologists not to become "social agitators" but to emulate the role of "scientific social reformer."[46] Although in later years Small abandoned group theory (which he had never labeled as such) in favor of a more elitist concept of America as "an

[41] Woodrow Wilson, *Congressional Government* (Boston: Houghton, Mifflin, 1971; orig. 1885), p. xv.

[42] Albion Small, *The Cameralists: The Pioneers of German Social Policy* (Chicago: Univ. of Chicago Press, 1909).

[43] Wilson, 331–332, bk. 3, ch. 3.

[44] Albion Small and George Vincent, *An Introduction to the Study of Society* (New York: American Book, 1894).

[45] Small and Vincent, bk. 4, ch. 4.

[46] Small and Vincent, pp. 74, 239.

aristocracy of political pull,"[47] this earlier orientation was quite similar to that of Macy and the political science reformists of the period before World War I.

A last set of writings originated in German scholarship, influencing Bentley directly. The American introduction to Ludwig Gumplowicz's *The Outlines of Sociology* (1899), written in behalf of the American Academy of Political and Social Science, described the work as "distinguished from all earlier sociological works by the character of the sociological unit upon which it is based, which is the group."[48] While this may seem unexceptional today its importance derived from two aspects of its historic context. First, group theory was a clear alternative to class analysis and its radical implications. (Gumplowicz himself incorporated major elements of class theory, but these aspects were largely ignored by his American interpreters.) Equally important, group theory was an attack on individualism in sociology. Individualism was tied both to concepts of free will, and to the reification of abstract "needs," "wills," and "feelings" as causal social "forces" difficult to define, much less observe. Either as free will or as social force, individualism seemed to frustrate the development of scientific sociology. In contrast, group theory seemed to hold the key. "The fate which befalls the individual in society," stated the introduction to Gumplowicz's volume, "is not the fate which he merits always, but it is necessarily that which his group makes inevitable."[49]

A second German scholar influencing Bentley was Rudolph von Jhering, whose work also emphasized the role of interest groups. Von Jhering argued that "Being interested in a purpose, or briefly, interest, is an indispensable condition for every action."[50] One finds in von Jhering's work a theory that (1) political change is based on social (not individual) action; (2) conditioned by individual interest; (3) grouped in associations; (4) the "social mechanics" of which determine what group theorists would today call homeostatic equilibrium.

Reasons for the Neglect of Bentley in His Own Time. Bentley was an early advocate of a descriptive political science based on a group conception of political life. He wrote:[51]

> We must deal with felt things, not with feelings, with intelligent life, not with idea ghosts. We must deal with felt facts and with thought facts, but not with feeling as reality or with thought as truth. We must find the only reality and the only truth in the proper functioning of the felt facts and the thought facts in the system to which they belong.

Statements such as "We must not deal with thought as truth" were out of touch with the mood of political science prior to World War I. The pluralist critique of juristic normative theory had not yet done its work, nor had postwar disillusion undermined the capacity of reformism to present itself as the heir to the mantle of science.

Arthur Bentley's *The Process of Government* (1908) was ignored by political scientists for some two decades before it achieved any significant recognition. Almost without exception discussion on groups in politics in the *American Political Science Review* omitted mention of Bentley until he was resurrected by modern group theorists nearly half a century later. Then, as in the 1967 edition of Bentley's work, he was praised for developing a theory of government as "a process in which interest groups are the players and protagonists. They are not individuals but groups; for the individual himself is but the product of group action and conditioning."[52]

At this point, the student of political science may well wonder what was exceptional about this viewpoint and why Bentley's contemporaries were not right in attaching to his work far less importance than was done in retrospect by contemporary group theorists. His view was unexceptional insofar as "group" was defined sweepingly as any relationship among two or more individuals, ranging from a family to a nation or a race. "Interest" was equated with "group."[53] At this basic level, Bentley's thesis was controversial only for those who wished to interpret political change in terms of the personalities of great individuals apart from their relation to family, race, class, ethnicity, religion, educational affiliation, or other "group" traits. Since virtually no one (and certainly not Albion Small, whom Bentley grossly misrepresented) defended such an extreme "great man" theory it is not surprising that political scientists of the day did not find this view controversial. Though the attack on individualism in sociology was then timely, it was not perceived as being important in a political science weighted toward institutionalism, not individualism.

The Nation found Bentley's work "critical rather than constructive," and Charles Beard, reviewing the volume for the *Political Science Quarterly*, also found the volume unoriginal in theory on

[47] Albion Small, *Origins of Sociology* (Chicago: University of Chicago Press, 1924), p. 285.

[48] Frederick W. Moore, intro. to *The Outline of Sociology* (Philadelphia: American Academy of Political and Social Science, 1899), by Ludwig Gumplowicz, p. 5.

[49] Moore, p. 6.

[50] Rudolf von Jhering, *Law as a Means to an End* (New York: Macmillan, 1913; orig. 1877), p. 40.

[51] Arthur Bentley, *The Process of Government*, ed. Peter Odegard (Cambridge: Harvard University Press, 1967. orig. 1908), p. 172.

[52] Peter Odegard, intro. to Bentley, pp. xiii–xix.

[53] Bentley, p. 211.

argument.[54] Its tone was characterized in various reviews as "flippant," "impatient," and "unjudicial," and, as Beard noted, "unnecessarily rude to his predecessors."[55]

The Process of Government did make two contributions in Beard's view: presenting an alternative to Marxist class analysis and going beyond formal descriptions of politics. While neither was sufficiently original to place the work in the mainstream of political science discussion at that time, its contemporary importance warrants a brief discussion of these two points.

In discussing the development of group theories of politics, Bentley wrote, "The starting-point for practical purposes is, of course, Karl Marx." In Marx, Bentley found "a very crude form of group interpretation."[56] Bentley considered the working class and what Marx termed the ruling class to be simply two more interest groups, ones which happened to find "unusually vehement verbal expression" in the mid-nineteenth century, thereby causing Marx to mistake them as the primary groups in the political process.[57]

Leaving aside Bentley's misinterpretation of Marx as holding all political conflict to be class conflict, one can see that the importance of this argument to later group theory lay in its view of working class and elite groups as simply other types of groups, not more deserving of discussion than families, farm organizations, racial groups, political parties, discussion societies, interest associations and other groups. Implicit is the image of government as "the steady appeasement of relatively small groups." In contrast, elite theorists have generally argued that business, for example, is a pre-eminent group whose institutional position cannot be placed in the same category with, say, voluntary associations for purposes of political analysis.

On the second point, that of going beyond formal description, Bentley's work was part of a long-standing trend away from institutionalism, preceded by Wilson and Bryce and followed by Beard and Laski. In Bentley's time this tendency was being accelerated by the work of the "muckrakers" on pressure politics. But where most political writers drew out the reform implications of this new literature on groups, Bentley ignored it. Thus it is hardly surprising that reform forces such as *The Nation* found *The Process of Government* "not constructive."

In his introduction to a reissue of Bentley's work, Peter Odegard expressed his failure to understand how Bentley could have neglected bringing the evidence of the "muckrakers" to bear on his theory of interest groups since they had already established the importance of such interests as Standard Oil and other trusts. Far from being surprising, however, this omission was a direct corollary of Bentley's rejection of prevailing definitions of the terms "interest" and "opinion."

Popular theories of governmental process emphasized the role of elites, using "interest" to refer to elite will and "opinion" to refer to popular will. This popular definition cast governmental process as interest group politics to be sure. Odegard was correct in this regard, but he neglected (as Bentley did not) that this usage cast interest groups as elite groups, viewing routine politics on important decisions to be elite conflict. Where the popular view emphasized public opinion and social reform movements as mediating instruments between the mass of active citizens and political elites, Bentley redefined "interest group" to comprehend both elite interest and public opinion blocs, social movements, the family, lobbies, etc. "Nothing turns on the distinction between interest and opinion," he misleadingly wrote.[58] Bentley's redefinition obscured important theoretical distinctions theretofore emphasized in political writing, serving its intended purpose of defining out of existence the elite theory premises of discussions of "interest groups" in that period. This obfuscation was another factor in Bentley's neglect.

Closely related were Bentley's attitudes toward social change. Where Bentley's foil, Albion Small, had linked "genuine change" to social crises, Bentley's mentor, von Jhering, had interpreted historical change in terms of laws of "social mechanics" operating through interest-based associations. The vagueness, self-contradiction, and changing views of all authors concerned warns against portraying this difference in emphasis as an unambiguous contrast, but that difference was nonetheless sufficient for Bentley to praise the "power" of von Jhering's thinking in contrast to the "confusion" of Small's thought on social psychology and social change.[59]

While Bentley was condemning research on psychological "soul stuff," the general trend in political science was in precisely the opposite direction, discovering the importance of the individual as interpreted by social psychology. In this regard, Graham Wallas's *Human Nature in Politics* (1909) was seminal. Between 1908 and 1928, twenty-five articles and book reviews on psychology and politics appeared in the *American Political Science Review*, including works by illustrious

[54] Rev. of *The Process of Government*, by Arthur Bentley, *The Nation*, 87 (July 30, 1908), 94; Charles Beard, rev. of *The Process of Government*, by Arthur Bentley, *Political Science Quarterly*, 23 (December, 1908), 739–741.

[55] In addition to above, see *Outlook*, 89 (May 30, 1908), 263.

[56] Bentley, p. 465.

[57] Bentley, pp. 465–467.

[58] Bentley, p. 244.

[59] Bentley, p. 56.

names such as Wallas, Merriam, Lasswell, MacIver, and Ellwood. Charles Merriam, in his review of "The Present State of the Study of Politics," emphasized the importance of psychological approaches and the influence of Albion Small, omitting mention of Gumplowicz, von Jhering, or Bentley.[60] Bentley's reference to psychological factors as "spooks" flew in the face of an important push toward interdisciplinary research in political science.

David Easton wrote that "Bentley stands as the watershed between the simple realism of Wilson and the more complex realism of the group approach."[61] It is difficult to see how, in turning away from the study of early social psychology or from a focus on political change in terms of periodic social crises and toward the equilibrium theory of German jurisprudence that Bentley was drawing closer thereby to "complex reality." Insofar as psychological factors, social movements and crises, and elite theory embody an important part of that "complex reality," Bentley was a force in the divergence between interest group theory and the study of collective behavior.

In itself, this evolution of group theory might not have been harmful to political science had it not become eventually the dominant political science approach to informal political processes. While undercurrents and exceptions could be cited, it is more indicative to note that within the structure of academia, courses on social movements, social change, and collective behavior fell to the departments of sociology and social psychology. It was adherents of these disciplines who developed the pertinent theories to the greatest depth. The "group" theory of politics came to focus in practice on normal lobbying processes, rarely treating social movements and crises. The period in which Bentley wrote was one of increasingly intense concern over labor, anarchist, socialist, and bolshevist movements, however, and Bentley's perspective was therefore not one whose time had come, to say the least.

Finally, the neglect of Bentley was also attributable to his position on public interest theories of the state. Bentley held that "we shall never find a group interest of the society as a whole . . . the society itself is nothing other than the complex of the groups that compose it."[62] This attack on the idea of democracy as founded on what Rousseau called the general will set the stage for a redefinition of democracy. As Pateman has demonstrated lucidly, the redefinition of democracy by later group theorists gave prime importance to group processes rather than accountability to policy preferences of an electoral constituency.[63] Thus pluralist group conflict within electoral machinery and civil liberties became the defining characteristic of democracy. As Bachrach has noted, this emphasis stripped democratic theory of normative standards which might be used to judge the degree of attainment of democracy as an ideal construct.[64] Moreover, by reinterpreting democracy as a social relationship already fulfilled in normal group processes, scholars eventually came to relate democracy to the equilibriating processes of group conflict rather than to a conflict theory perspective emphasizing tensions between the state and elites on the one hand and democratic aspirations of various groups on the other.

Group Theory and Descriptive Political Science in the 1920s

Bentley's work cannot be characterized as being in advance of its time, ignored because of the shortsightedness of his contemporaries. This is so with regard to his attack on normative theory, his critique of the relevance of social psychology, his advocacy of a mechanistic equilibrium model, his discussion of the theory of public interest, and his obscuring of the issues raised in elite theory interpretations of the state.

Prior to Bentley's resurrection by modern group theorists, the only significant endorsement of his work in the pages of the *American Political Science Review* came from a sociologist, Harry Elmer Barnes, representing the quite different concerns of another discipline.[65] Moreover, Barnes's apparent enthusiasm for Bentley grew from his incorrect equation of Bentley's views on *governmental* process with Ratzenhofer's and Gumplowicz's views of *society* as a complex of competing interest groups over which the state imposed necessary restraint not deriving from the sum of conflicts among interests. Even with Barnes's high praise, Bentley remained largely ignored during this period.

Much later, Bentley was remembered less for his particular theories than for his general orientation toward the importance of groups and the methods of "empirical theory."[66] In terms of

[60] Charles Merriam, "The Present State of the Study of Politics," *APSR*, 15 (May, 1921), 173–85.

[61] David Easton, *The Political System* (New York: Knopf, 1953), p. 177.

[62] Bentley, 222.

[63] Carole Pateman, *Participation and Democratic Theory* (Cambridge: Cambridge University Press, 1970), esp. chaps. 1 and 2.

[64] Peter Bachrach, *The Theory of Democratic Elitism: A Critique* (Boston: Little, Brown, 1967).

[65] Harry E. Barnes, "Some Contributions of Sociology to Modern Political Theory," *APSR*, 15 (November, 1921), 487–533.

[66] R. E. Dowling, "Pressure Group Theory: Its Methodological Range," *APSR*, 54 (December, 1960), 951; Robert Golembiewski, "The Group Basis of Politics: Notes on Analysis and Development," *APSR*, 54 (December, 1960), 963.

operationalization of definitions and measurement of critical variables, Bentley's work did not constitute a useful guide,[67] but political scientists such as Golembiewski have been prone to credit Bentley for his contribution to scientific orientations in political science.

Nonetheless, Bentley did not play a significant role in the development of a political science constructed around descriptive or empirical theory. Indeed, much of this credit must go to the nineteenth-century "institutionalists" such as Willoughby who spelled out the role of descriptive research in a comprehensive political science. There one finds a more sophisticated and certainly more influential view than Bentley's.

In the early years of the twentieth century, the felt need to focus empirically on groups was widely shared among American political scientists. Summarizing popular ideas on government, for example, Albert Hart noted that "more and more people tend to accept the theory that all government in America—national, state, municipal or local, springs from one source, the American people as a whole, who choose to exercise their power through a variety of organizations."[68] At the same time, a reform element felt that "individual interests without number have been built up" and "they have not been harnessed to a common cause."[69]

Given such concerns, these years were characterized by a search for the common, uniting interest. This humane orientation was sometimes coupled with a distaste for the concept of political "science," as in the writings of Woodrow Wilson:[70]

> I do not like the term political science. Human relationships, whether in the family or in the state, in the countinghouse or in the factory, are not in any proper sense the subject matter of science. . . . I prefer the term politics. . . . Know your people and you can lead them; study your people and you may know them. But study them, not as congeries of interests, but as a body of human souls, the least as significant as the greatest,—not as you would calculate forces, but as you would comprehend life.

Yet the reformers were very much advocates of empirical research, and Wilson himself is correctly cited by later group theorists as being among those who guided political science in the direction of realistic description of political process. Statements such as that quoted above are properly understood not as attacks on empiricism but as a call to link pragmatic study continually with normative concerns—a call which merely rephrased similar views of "institutionalists" such as Willoughby.

It was only with the disillusion following World War I that advocacy of a non-normative science of politics came to the fore and that the concepts prefiguring modern "value-free" empirical theory received their real development. Ironically from the point of view of those who emphasize Bentley, the push for descriptive science after World War I first became prominent in relation to the development of research on social psychology and public opinion. Later, its development was associated with such theorists as Michels, Catlin, Ellis, Munro, and Dickinson, and reflected in group studies by authors such as Herring, Odegard, and Rice.

James Mickel Williams's *Principles of Social Psychology* appeared in 1922 and was reviewed favorably for its emphasis on "the standpoint of the conflict of interests which shows itself in economic, political, professional, family, cultural and educational relations."[71] The following year, Horace Kallen's essay on "Political Science as Psychology" appeared in the *American Political Science Review*, criticizing radical pluralism and saying of behaviorist psychological analyses that "what is important is that they cannot formulate a variant conception of political society without at the same time grounding it upon a variant definition of human nature whose variant trait is established in the special group interest to be advanced, defended, and vindicated. So, then," Kallen asked, "if political science is not psychology, what is it?"[72]

In 1924, in reporting the results of a survey taken by a committee of the American Political Science Association, Charles Merriam noted that psychology seemed to hold "rich possibilities" for political analysis, writing, "As social psychology develops, the whole study will be correspondingly enriched by the addition of the analysis of groups, of group relations, and individual-group relations, and indeed the whole interlocking series of cycles in the complex social process."[73] Merriam linked this projection to an appeal to develop these promised new avenues of insight through empirical analysis, not through traditional theoretic consideration. Likewise, Ellwood's *The Psychol-*

[67] Dowling, pp. 949–50; Myron Q. Hale, "The Cosmology of Arthur F. Bentley," *APSR*, 54 (December, 1960), 956.

[68] Albert B. Hart, "Growth of American Theories of Popular Government," *APSR*, 1 (August, 1907), 558.

[69] Woodrow Wilson, "The Law and the Facts," *APSR*, 5 (February, 1911), 5.

[70] Wilson, "The Law . . . ," pp. 10–11.

[71] Rev. of *Principles of Social Psychology*, by James M. Williams, *APSR*, 16 (November, 1922), 715.

[72] Horace M. Kallen, "Political Science as Psychology," *APSR*, 17 (May, 1923), 203.

[73] Charles Merriam, "The Significance of Psychology for the Study of Politics," *APSR*, 18 (August, 1924), 485–486.

ogy of Human Society (1925) presented a more detailed framework for the empirical study of "the origin, development, and structure of group life."[74] Works by Lasswell, Floyd Allport, and Sorokin added fuel to this perspective.[75]

Among the most important of the areas studied empirically was that of public opinion. Here was the empiricist's domain that corresponded to the focus of earlier theorists on the concept of the public interest or general will. Lippman's work on public opinion was most influential in spreading the newly discovered "facts" about the relative absence of rationally or intelligently held common opinion on most issues.[76] The role of imitation, suggestion, and emotions was also emphasized in Lowell's *Public Opinion in War and Peace* (1923). Bernays reported the rise of public relations as a new profession,[77] and Weeks's concerns in *The Control of the Social Mind* (1923) became generally accepted. Allport's empirical research on the convertibility and similarity of extremists further supported this orientation.[78]

This new wave of psychologically oriented empirical work on public opinion both reflected and reinforced the new pessimism about the common man, a view given further impetus by the rise of fascism in Italy. At one pole the new empiricism seemed to lead to the conclusion that political scientists must "face squarely the fact that the voice of the people is usually the voice of Satan."[79] At the other end of the spectrum were those, such as Eldridge, who sought to deal with the problem of "the phantom public" raised by Lippman through grand reform proposals for civic education.[80]

One of the outstanding political scientists of that day, George E. G. Catlin, looked toward psychological studies of opinion for a possible key to the scientific measurement of influence. "Politics," Catlin held, "must . . . be founded on the observation of some distinguishable political 'process' . . . and this, it may be tentatively suggested as a focus for convenient study, is to be detected in the recurrent situation of the relationship of wills in a fashion of control."[81] These behavior patterns were seen as " 'lines of conduct' of an individual or interest-group character, pursued in relation to other individuals or groups."[82] The scientific problem for political scientists was to find a measure of the strengths of opposing individual and group wills in any given case. After a quite modern critical discussion of the problems of measuring influence, Catlin indicated that his hope for quantification of political science lay along lines suggested by the psychological studies of opinion.[83]

By this point, Ellis could characterize political science as being "at the crossroads," turning away from law and jurisprudence toward descriptive science. In contrast to the traditional idea of "the sovereign state," the new concept of "the political state" was viewed as a scientific advance. "All that it (the concept of the political state) seeks to do," Ellis wrote," is to describe and account for political organization, leaving to the moralist the justification or otherwise of the use made of political power."[84]

When MacIver's *The Modern State* (1926) appeared, its reviewer, W. Y. Elliot, criticized it for failing to clarify that the state still retained ultimate sovereignty over the groups within it.[85] Traditionalists within political science perceived the growth of the movement for a purely descriptive science and condemned it roundly. W. W. Willoughby attacked Krabbe's *Modern Theory of the State* for its critique of the concept of sovereignty as resting on a confusion of ethical and descriptive studies of the state.[86] Sabine defended Krabbe against Willoughby, but in the process also emphasized the continuing need for normative theory.[87] Edwin Corwin pleaded that political science "must still retain its character as a 'normative,' a 'telic' science."[88] And in his presidential

[74] Charles Ellwood, *The Psychology of Human Society* (New York: Appleton, 1925).

[75] Harold Lasswell, "Two Forgotten Studies in Political Psychology," *APSR*, 19 (November, 1925), 707–717; Harold Lasswell, "The Study of the Ill as a Method of Research into Political Personalities," *APSR*, 23 (November, 1929), 996–1001; Floyd Allport, *Social Psychology* (New York: Houghton Mifflin, 1924); Pitirim Sorokin, *The Sociology of Revolution* (New York: Lippincot, 1925).

[76] Walter Lippman, *Public Opinion* (New York: Harcourt, Brace, 1922); Walter Lippman, *The Phantom Public* (New York: Harcourt, Brace, 1925).

[77] Edward Bernays, *Crystallizing Public Opinion* (New York: Boni and Liveright, 1923); Edward Bernays, *Propaganda* (New York: Horace Liveright, 1928).

[78] Floyd Allport and D. A. Hartman, "Measurement and Motivation of Atypical Opinion in a Certain Group," *APSR*, 19 (November, 1925), 735–760.

[79] Rev. of *The Public Mind*, by Norman Angell, *APSR*, 21 (August, 1927), 695.

[80] Seba Eldridge, *The New Citizenship* (New York: Crowell, 1929). Civic education ideas are traced in G. David Garson, "Research on Policy Alternatives for America in the 1930's," *Political Inquiry*, 1 (Winter, 1973–4), 50–77. G. David Garson, "Citizenship as Ideology," *Maxwell Review*, 10 (Winter, 1973–4), 25–38.

[81] George Catlin, "The Delimitation and Mensurability of Political Phenomena," *APSR*, 21 (May, 1927), 257.

[82] Catlin, 255.

[83] Catlin, 262–268.

[84] Ellen Deborah Ellis, "Political Science at the Crossroads," *APSR*, 21 (November, 1927), 789–790.

[85] W. Y. Elliot, rev. of *The Modern State*, by R. M. MacIver, *APSR*, 21 (May, 1927), 432–434.

[86] W. W. Willoughby, "The Jurist Theories of Krabbe," *APSR*, 20 (August, 1926), 509–523.

[87] George H. Sabine, "Political Science and the Jurist Point of View," *APSR*, 22 (August, 1928), 553–575.

[88] Edwin S. Corwin, "The Democratic Dogma and

address to the American Political Science Association, Jesse Reeves defended political philosophy, holding that "each group has something to receive from the other."[89]

In spite of pleas for mutual tolerance on the part of men like Sabine and Reeves, the enthusiasm for descriptive science was pronounced and its practitioners were not inclined to relate their work to the more theoretical work on groups which had gone before. Citing the example of physics, Munro told his colleagues in a presidential address to the American Political Science Association that "our immediate goal, therefore, should be to release political science from the old metaphysical and juristic concepts upon which it has traditionally been based. . . . It is to the natural sciences that we may most profitably turn."[90] Similarly, in discussing how modern courses of introductory political science should be taught, Spencer warned against political scientists playing the role of reformer. Instead, he wrote, "It is the function of the political scientist to observe political phenomena and to tell what he sees."[91]

In illustration of the new descriptive approach and its relation to group theory, one may cite the brilliant work of John Dickinson. Dickinson used historical and anthropological evidence to argue that "conflict between interests is inevitable" and that order is not suppression of conflict; order is the readjustment of human relations in the course of conflict; order is a neverending process.[92] In this view, "Government, like the human will, is motivated by the very forces which it governs; its function is only to arrange them in a more orderly pattern."[93] Conflict is partially resolved through voluntary group agreements, but the inequities of unchecked application of this means alone is inadequate. Custom, therefore, plays a major role in social control, and the state is constrained by its own habits or culture. Custom and voluntarism resolve most conflicts, but authority backed up by force is held in reserve. Indeed, "an important element in the art of governing is to predict successfully whether such voluntary and natural adjustments will occur."[94]

Dickinson's work, based on theoretical inference from historical and anthropological data, presented an interpretation of politics revolving around group interests. It transcended Bentley's mechanistic model by emphasizing the role of culture and the use of authority as social control. Other important studies of a "modern," descriptive nature, focusing on groups, were Odegard's *Pressure Politics* (1928), Herring's *Group Representation Before Congress* (1929), and Rice's *Farmers and Workers in American Politics* (1924). Of these, Rice's study, with its emphasis on culture areas and psychological traits in analyzing group interests in legislation, was the most theoretically complex. As Zeigler has noted, Odegard's work was "essentially a descriptive study with little or no emphasis on theoretical problems," a characterization which applied to Herring's volume as well.[95]

In *The Science and Method of Politics*, George Catlin wrote that "politics, as a theoretical study, is concerned with the relations of men, in association and competition, submission and control, insofar as they seek, not the production and consumption of some article, but to have their way with their fellows."[96] Thus, with the rise of descriptive political science in the 1920s the study of politics came to be viewed as the study of influence and power, not the study of government and the state. The crucial distinction between the two lay in the abandonment of normative theory by the empirical students of group power.

This difference was not primarily one of institutional focus or even of emphasis on the importance of group interests in politics. Extra-institutional concerns such as the family and religion had been treated by the jurists. And few gave more importance to the role of groups in political life than the later generation of scientific reformers. To be sure, the increasing importance of organized economic groups was reflected in increasing attention to this phenomenon over the years by writers of all orientations, but the crucial shift was in the questions that were asked about these groups.

While a traditional "theory of politics" was obliged to wrestle with the question of how groups within a society ought to be coordinated, relating these models to historical origins, ethics, practice, and many other concerns, the new descriptive approach merely sought to establish what practice was. Where descriptive studies were extrapolated into a generalized description of political process, as in the work of John Dickinson, that model was so broad that virtually any normative model from monarchism to socialism could be contained within it. Since many polities in varying degrees fulfilled many different normative models of how groups ought to be coordi-

the Future of Political Science," *APSR*, 23 (August, 1929), 591.

[89] Jesse S. Reeves, "Perspectives in Political Science, 1903–1928," *APSR*, 23 (February, 1929), 16.

[90] William Bennett Munro, "Physics and Politics: An Old Analogy Revised," *APSR*, 22 (February, 1928), 10.

[91] Richard Spencer, "Significance of a Functional Approach in the Introductory Course in Political Science," *APSR*, 22 (November, 1928), 961.

[92] John Dickinson, "Social Order and Political Authority," *APSR*, 23 (May and August, 1929), 299.

[93] Dickinson, 619.

[94] Dickinson, 625.

[95] Zeigler and Peak, 12.

[96] George Catlin, *The Science and Method of Politics* (New York: Knopf, 1927), 210–211.

nated within the state (the "problem of order"), the search for a descriptive theory of politics in relation to interests and groups was reduced to a search for lowest common denominators, such as the universality of interest conflict or the universal relevance of custom and culture.

Of course, the descriptive orientation was inevitably violated by frequent implicit and sometimes explicit normative statements. Dickinson's work, for example, implied the desirability of the state's deferring to voluntary resolution of conflict and to custom in most matters, a model strikingly in contrast to that implied by advocates of social planning as this was developed in the decade which followed.[97] Lippman's work was deeply imbued with conservative principles. But more important than the reintroduction of relatively unexamined normative theories through the back door was the acquiescence of political science in the post–World War I mood and its shift from prescription to description. Its importance lies not only in the neglect of enduring issues and questions, but also in the theoretical setback which political science suffered by the artificial divorce of normative theory from empirical research on American politics.

Conclusion

The study of the origins of group theory is important because the shortcomings of the past recur in the present. The reasons that Bentley was ignored in his day, for example, are at the core of contemporary criticisms of group theory. For example, Rothman's critique of Truman's *The Governmental Process* emphasized the relative neglect of social movements, crises, and change and the rejection of Marxist analysis while reintroducing economic explanations standing in tension with a purely group explanation of politics, criticisms which would apply to Bentley as well as Truman.[98]

Similarly, R. E. Dowling wrote, "We can see in Truman, that is, over and above his observations about the place of groups and their pressures in the matter that comes to the political scientist, a faith in something called 'the group interpretation of politics.' This faith has had an influence on his political science which has been, if I am not mistaken, almost wholly evil."[99] Dowling observed that this appeared most clearly where Truman sought to defend interest-group theory from the charge that it "leaves something out." "He does not see that the *merit* of a 'group interpretation' of politics," Dowling wrote, "would, if it were worked out, be precisely that it does '*leave something out*'."[100] The concept of theory is lost by making "value-free" political science more "accurate" through including all considerations within it.

The purpose of theory is to simplify reality. That everything is related to everything else, and that groups are important in politics is a view which may be accurate, but it is not helpful in advancing the discipline. Bentley's mechanistic physics of group pressures was less accurate, but it was, like Marxism, a relatively strong construct. That it was not developed, like jurisprudential models, as a normative type among a set of types reflects the underlying drive in political science for the one correct theoretical framework, a drive which misunderstands the literally essential role of normative theory in social science methodology and leads to a literature which sustains a high level of generality through concentration on lowest common denominators. The effect has been less than intellectually satisfying to many, precipitating factions within the discipline which seek to reassert normative theory on methodological as well as moral grounds.

The history of political science, it was stated at the outset, has been one of gradual encroachment of ideas rather than the sequential building of scientific study upon scientific study. Jurisprudence fell to the "facts" marshalled by the pluralists and their counterparts in the 1920s not because the facts refuted their theories (the facts were largely irrelevant to the normative questions raised by the jurists), but because the new perspectives were in accordance with the more pessimistic mood and the hopes for science after World War I.

Reformism, perhaps because of its untheoretical articulation, was not so much rejected as ignored. While reformism had seemed earlier to be closely allied to science, after World War I it could be dismissed by Spencer as moralism in obvious conflict with the serious study of political phenomena. Pluralism itself fell, partly from its insecure foundations on a caricature of jurisprudence, but also from the equation of its normative implications with radicalism. With the emergence of descriptive science in the 1920s came an optimism about the vast possibilities for a science of politics now that the discipline had been freed from the fetters of metaphysics. But these possibilities went unrealized and were forgotten in the

[97] See Garson, "Research on Policy Alternatives . . ." for further discussion of social planning thought.

[98] Stanley Rothman, "Systematic Political Theory: Observations on the Group Approach," *APSR*, 54 (March, 1960), 24, 29. In a brief reply, Truman disclaimed followers who treated his work as a group theory of politics, stating the book's purpose was simply to "examine interest groups and their role," not develop a general model of politics. David Truman, "On the Invention of 'Systems'," *APSR*, 54 (June, 1960), 494–495.

[99] Dowling, p. 953.

[100] Dowling, 953.

main, thereby enabling the discovery anew of "value-free" science after the next world war.

The history of ideas inevitably conveys a greater sense of coherence and sequence than actually existed. The change is better categorized under the term "accretion" than that of "development." Development connotes either change by explicit choice, or growth in accordance with some inner telos. The change of political science was neither of these for the most part; rather the discipline spread in response to outside stimuli: the rise of economic groups, the tides of progressivism and disillusion, the birth and excitement of modern psychology, and many other factors.

This is not to deny that much may be found in the way of explicit analysis of this or that tenet or belief. But in the debates, the shift in basic premises and questions was all too often overlooked, and political science changed in accord with these unacknowledged premises. This change is not one of deterioration from a golden age of juristic political science. Much was genuine advance: increasing scope and depth of study in terms of subject matter, increasing sophistication of technique, increasing interdisciplinary borrowings from psychology, sociology, anthropology, economics, and other disciplines.

But the change in political science cannot be seen as simple advance; in the shifts in mood and predisposition, opportunities were lost as well—losses from which we are only now recovering. With the passing of jurisprudence, the normative debate over integration of ideal constructs of the state in relation to practice faded as a possibility. This stripped political science of carefully formulated norms by which to categorize practice, as Bachrach has shown, obscuring the critical role of ideal types in social science research.

With the turning away from reformism, not only was the discipline drained of much compassion and vision, but an implicitly dynamic focus on political change gave way to a static focus on describing political life as it existed. Though pluralism was rejected for logical reasons in part, its contribution to historical approaches to political science and its relevance to normative theory (even if from an ostensibly "descriptive" stance) tended to drop from sight with the rise of descriptive political science after World War I. This orientation too passed in turn, leaving for later political scientists the rediscovery of its contribution in scientific method, social psychology and socialization, political culture and custom, and group interest conceptions of political process.

Representatives of all orientations emphasized the role of group interests in politics in one manner or another. A group interest conception of the state, going back to Madison was a perennial problem for the jurists, especially with the consolidation of enormous economic interests toward the end of the nineteenth century. The jurists emphasized the importance of group interests in exacerbating the gap between practice and various normative models of the state. Like the social planners of the 1930s they were optimistic about the ability of the state to insulate itself from these interests and to control them autonomously when necessary.

The reformers' view of group interests was closely allied to this perspective, though often implying elitist rather than constitutionalist models of the state. The pluralists showed how the expanding center of private power posed a growing challenge to the pre-eminence of the state, reducing the state to merely one among many groups. The fact of developing private power eventually led Laski to Marxism and social planning, while other lines of influence may be traced toward advocacy of democratization of groups and guild socialism on the one hand, or to acceptance of the beneficial effects of competing private powers and laissez-faire on the other. It is more difficult to summarize the orientation of the empiricists toward groups in politics, but they shared a highly influential focus on the irrational nature of group identifications and the negative consequences of this for public opinion and democracy.

The cases for the pre-eminence of the state, for social planning, for liberal reform, for organizational democracy, for laissez-faire, or for resignation to the mixed blessings of group process may still be made. In fact, all these normatively premised perspectives are still advocated under new labels. And the actual operation of American politics displays in varying degrees the practice associated with each perspective. What has not yet come to the fore is the explicit comparison of normative models of the state and their integration with empirical study. Rather than adopt a systematically multifaceted orientation which incorporates the many strands of our discipline's past, most political scientists remain predisposed to select one or another orientation as "the most nearly correct framework."

Ethnic Voting and Political Change in South Africa*

STANTON PEELE
Graduate School of Business Administration, Harvard University

STANLEY J. MORSE
Pontifícia Universidade Católica de São Paulo (São Paulo, Brazil)

In the South African general election of 1970, a generation-long trend toward consolidation of power by the Afrikaner-dominated National Party was reversed as the Nationalists, while retaining a comfortable majority in Parliament, lost ground to the opposition United Party. Immediately prior to the election, the authors conducted an attitude survey among a sample of potential white voters in three representative constituencies in Cape Town. The purpose of this study was to test the cross-cultural validity of findings which have emerged from research on voting behavior conducted in the United States and to explore the way in which social-psychological factors affect voting behavior and political attitudes.

Theoretical and Historical Background

Studies of American presidential elections—particularly those conducted by Lazarsfeld and his co-workers and by the University of Michigan's Institute for Social Research[1]—have emphasized the nonideological, nondeliberative nature of the voting decision, along with the low level of political involvement and awareness of issues that characterizes a large portion of the electorate. This work shows, among other things, that party identification, shaped and maintained by primary group contacts, is the major determinant of electoral choice, and furthermore, that party identification can largely be predicted from knowledge of a voter's sociological background. Almond and Verba's[2] demonstration that "political culture" differs from one country to another, however, has raised questions about the cross-cultural applicability of these American findings, and a good deal of electoral research in other countries has resulted.

Cross-cultural research has pointed to a number of key dimensions along which political systems differ, and which in turn affect the nature and extent of party identification. Studies of European parliamentary systems, for example, have uncovered a variety of voting patterns, based on different combinations of ideological, class, and traditional-symbolic motivations.[3] In examining the social-psychological mechanisms underlying electoral behavior, one must obviously take into account an electorate's historical and cultural traditions, the nature of the system (whether presidential or parliamentary), the number and longevity of political parties contesting the election, and so on.

The Republic of South Africa is an interesting and useful subject for comparative political analysis, especially in view of the unique set of circumstances that have shaped its history and national existence.[4] From the unification of

* For a more complete report of this study see Stanton Peele, "The Ethnic Basis of Voting Behavior in South Africa" (doctoral dissertation, University of Michigan, Ann Arbor, 1973). Thanks are due to Daniel Katz and the other members of Dr. Peele's dissertation committee. The authors would like especially to thank Archie Brodsky for his invaluable help in the preparation of this manuscript. In addition, the authors would like to express their appreciation to John von Rynesveld, James Midgley, George Rabinowitz (and other graduate assistants in the political behavior bay at the Institute for Social Research), Laura Klem and Joyce Alexson for their help with various stages of this study. Financial and institutional support were provided by the Ford Foundation, the Research Division of the Harvard Business School, the University of Michigan, the University of Cape Town, and New York University.

[1] See: Paul F. Lazarsfeld, Bernard R. Berelson, and Hazel Gaudet, *The People's Choice*, 2nd ed. (New York: Columbia University Press, 1948); Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting: A Study of Opinion Formation in a Presidential Campaign* (Chicago: University of Chicago Press, 1954); Angus Campbell, Gerald Gurin, and Warren E. Miller, *The Voter Decides* (Evanston, Ill.: Row, Peterson, 1954); Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960); Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: Wiley, 1966).

[2] Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown, 1963).

[3] See: Angus Campbell and Henry Valen, "Party Identification in Norway and the United States," in Campbell et al., *Elections and the Political Order*, pp. 245–268; Philip E. Converse and George Dupeux, "Politicization of the Electorate in France and the United States," in *Ibid.*, pp. 269–291; David Butler and Donald E. Stokes, *Political Change in Britain* (New York: St. Martin's Press, 1971); Ole Borre and Daniel Katz, "Party Identification and Its Motivational Base in a Multiparty System: A Study of the Danish General Election of 1971," *Scandinavian Political Studies*, 8 (October, 1973), 69–111.

[4] For historical background, see Leonard M. Thomp-

South Africa's four provinces into a British dominion in 1910, South African politics have been conditioned by two overriding facts: the need felt by the ruling white minority to protect itself against the black majority, and the continuing division of the white minority into two distinct ethnic groups—the Afrikaners, descended from the original Dutch and French Huguenot settlers of the 17th and 18th centuries, and the more heterogeneous English-speaking South Africans descended from British colonists of the 19th century and from later immigrants. Because the Afrikaners have always constituted a majority of the white population,[5] the country's leaders have all been Afrikaners, but not all have been such hard-line Afrikaner nationalists as the present leadership. Up to 1948, the government had for some years been in the hands of the United Party (UP), which was led by moderate Afrikaners who stressed cooperation between the two white groups. But in that year the National Party (NP or Nats), capitalizing on Afrikaner distaste for the UP's pro-British stand in World War II, gained control of Parliament with only 40 per cent of the popular vote. Subsequently, the NP increased its parliamentary majority and its share of the popular vote in each of four successive elections, the last of these occurring in 1966.

The party achieved these gains through its strength in rural Afrikaans areas (which are overrepresented in Parliament in comparison with urban, English-dominated districts) and by exploiting its role as the political voice of exclusive Afrikaner nationalism. As Afrikaners have moved into the cities and even some of the newer suburbs, the NP has been leaving fewer and fewer urban seats uncontested, in this way becoming a more truly national party. Against this thrust, the United Party has been unable to establish an ideological position distinct from that of the governing party. While opposing implementation of the specific Nat policy of *Apartheid* ("separate development"), the UP has been understandably unwilling to challenge the fundamental Nationalist principle of white supremacy (*baaskap*). As a result, the opposition party has not been able to reverse the tide of increasing Nationalist solidarity among Afrikaners. It has also been unable to prevent defections by some English-speaking voters to the party which was so successful in engendering national pride by, among other accomplishments, bringing about the establishment of a Republic in 1961.

When elections were called in 1970, the NP held 127 seats in the House of Assembly (which corresponds to the British House of Commons), as against 38 for the UP and 1 for the liberal, anti-*Apartheid* Progressive Party. In this election, which has been described in some detail by Henry Lever in *The South African Voter*,[6] the already overwhelming NP majority was expected to increase, although Nationalist leaders were worried by a challenge from the Herstigte Nasionale Partie (Purified National Party), a right-wing offshoot of the NP. Instead, the HNP failed to elect a single M.P., and the Nats lost 9 seats to the United Party. The Progressive Party improved its poll as well, coming close to electing a second M.P. in a liberal Cape Town constituency.

Lever's study (which also examines the 1966 election) is the first to apply American models of electoral behavior to South Africa. His data support the common wisdom that Afrikaners vote Nationalist while English-speaking South Africans vote UP. He also found that such variables as social class and education predict well the voting responses in two "marginal" constituencies in Johannesburg. These factors covary with ethnicity to such an extent, however, that they seem to account for little additional variance beyond that accounted for by ethnicity. Throughout the country, Afrikaners tend to be less well educated, of lower socioeconomic status, and more heavily concentrated in rural areas than English-speaking South Africans. Furthermore, Afrikaners almost always belong to one of the Dutch Reformed Churches, which are among the leading organs for the dissemination of National Party principles, while the English-speaking are generally (though not as invariably) Anglicans. Thus, in terms of the kinds of sociological dimensions that are used to predict voting response in America, the South African population divides along ethnic-subcultural lines, and the *independent* predictive value of all other sociological categories remains to be determined.

One focus of the present study, then, is on the determinants of party identification in South Africa. Who votes for which party, and why? Is the American demographic model for determining party identification applicable to South Africa, and, if so, are the relevant demographic categories

son, *The Republic of South Africa* (Boston: Little, Brown, 1966); C. W. de Kiewiet, *A History of South Africa* (London: Oxford University Press, 1962); Gwendolen M. Carter, *The Politics of Inequality: South Africa Since 1948* (New York: Praeger, 1958); Leo Marquard, *The Peoples and Policies of South Africa* (London: Oxford University Press, 1962).

[5] As of 1968, Afrikaans-speaking people made up 58 per cent (and Jews 4 per cent) of the 3,639,000 whites in South Africa—this against 1,912,000 Coloureds (racially mixed), 574,000 Asians (mostly Indians), and 13,042,000 black Africans of "Bantu" extraction. See Muriel Horrell, *A Survey of Race Relations in South Africa: 1968* (Johannesburg: South African Institute of Race Relations, 1969).

[6] Henry Lever, *The South African Voter* (Cape Town: Juta, 1972).

the same? That is, do we find something comparable to the additive formula proposed in *The People's Choice*[7]—where a voter's social class, religion, and place of residence all contribute independently to his predisposition to vote one way or the other—or does the extreme ethnic polarization of the South African electorate dictate a more unidimensional representation of these sociological determinants? Beyond this, we want to explore the social-psychological meaning of party identification as a variable mediating between the voter's sociological characteristics and his voting decision. In this context, the peculiar intensity which the issue of ethnicity achieves for South Africans can be seen as an illustration of a more universal phenomenon which Borre and Katz[8] have begun to explore in the Danish context, i.e., the way party identification—and hence a vote—can be a statement about oneself, an expression of one's self-image, perceptions, and needs.

A second major focus of this study is on electoral change. The halt in National Party momentum may or may not represent a significant trend —perhaps an ideological shift away from the Nats, or else a transition from one-party emergent nationalism to stable two-party parliamentary democracy. Such trends can be demonstrated only by further elections, but the 1970 election seems worth examining for what it may reveal both about the prospects for South Africa's immediate political future and about the general process of electoral change.

American election studies have found that individuals who change their votes are less interested in and informed about political events than individuals who show a stable party preference. This avoidance of electoral politics is said to be characteristic of the "cross-pressured" voter, i.e., the voter whose inconsistent group memberships or social status positions cause him to be subjected to conflicting political pressures.[9] More recent studies, however, have challenged this formulation. Concentrating on socially mobile voters, who represent one important category of the cross-pressured, Barber[10] found levels of political interest among both the upwardly and downwardly mobile to be *intermediate* between the greater interest shown by people of high status and the lesser interest shown by people of low status. Barber's general contention is that mobile individuals have political attitudes and predispositions which lie between those of their status of origin and their current status group because new patterns of socialization are gradually coming to supplant their previous socialization.

In South Africa the issue of social mobility is at present a very real phenomenon among the politically decisive Afrikaans majority as Afrikaners move into administrative and entrepreneurial positions formerly monopolized by the English-speaking. The political significance of this movement lies in the fact that, as in the United States, political attitudes in South Africa can be observed to vary with socioeconomic status (SES). But, in contrast to the United States, higher-SES South Africans tend to be more liberal. One reason is that these people are usually English-speaking or Jewish (as well as urban). Another is that individuals who have more economic power and more information about the economy are in a better position to develop a sophisticated, internationalistic outlook. The question we are concerned with is whether Afrikaners who enter the more cosmopolitan world of business show a broadening of political perspective as well, and vote according to their economic interests (UP) rather than ethnic identification (NP).

Another group whose potential as a source of electoral change deserves close examination is the young, especially the college-educated young,[11] who elsewhere in the world have been a liberal or even a radical force. Their attitudes take on added weight in view of the American and British finding[12] that long-range shifts in party strength come about through the recruitment of new voters, not the conversion of an existing electorate. The emerging Afrikaner entrepreneur and the young seem to constitute jointly one of the only sources of possible political change in South Africa. Since some political commentators have assumed from the 1970 returns that these groups have already begun to shift to a more liberal stance, it is worth finding out how they, and other population segments, actually did intend to vote.

Procedure

A sample of 462 white[13] voters was interviewed

[7] Lazarsfeld et al., *The People's Choice*, pp. 25–27.

[8] Borre and Katz, "Party Identification and Its Motivational Base in a Multiparty System."

[9] Lazarsfeld et al., *The People's Choice*, p. 62; Berelson et al., *Voting*, pp. 283–284.

[10] James Alden Barber, Jr., *Social Mobility and Voting Behavior* (Chicago: Rand McNally, 1970), pp. 165–168.

[11] Because of the country's racially based class system, South African whites have one of the highest per capita rates of university attendance in the world. As reported in Horrell, *A Survey of Race Relations*, there are 68,549 university students out of a white population of 3,639,000.

[12] See Campbell et al., *The American Voter*, and Butler and Stokes, *Political Change in Britain*.

[13] Only the white electorate participates in national parliamentary elections. Coloured (see Note 5) voters elect a majority of members of the essentially powerless Coloured Representative Council, as do Africans in the case of the equally ineffectual Bantu urban councils. As yet, those few "Bantustan" (African homelands) legislative assemblies which exist are made

during the two weeks preceding the parliamentary election held on April 22, 1970. The sample, stratified according to income, was selected from three election districts in the city of Cape Town. Within each district, respondents were chosen by a random procedure in which interviewers visited alternate flats or houses, starting from randomly selected intersections (and skipping corner houses), until six respondents per designated intersection had been found. At each house, an additional random procedure proposed by Kish[14] was employed to determine which occupant (among the eligible voters residing there) was to be interviewed. The refusal rate among the individuals selected was only 4.7 per cent—lower than the usual rate for American and European election studies.

The interview schedule, which was initially constructed in English, was translated into Afrikaans and checked by the double translation method. Interviewers were female undergraduate students at the University of Cape Town, where the junior author was a visiting lecturer. Selected for their bilingualism wherever possible (with native or at least fluent Afrikaans speakers used in heavily Afrikaans-speaking areas), interviewers were thoroughly trained in interview procedures through videotapes, training sessions, and detailed written briefings. Respondents were invited to choose whether they wanted the interview conducted in English or Afrikaans. Curiously, 102 of the 180 respondents who listed Afrikaans as their current language chose to answer questions in English. (All white South Africans must study both languages for their twelve years of public schooling.) They did this, presumably, either to make things easier for the interviewers (whom they saw as being predominantly English-speaking) or to gain their approval. In our analysis we will return to the implications of this choice for evaluating these respondents' answers to the political and attitudinal questions that formed the substance of the survey.

The survey instrument consisted mainly of closed-ended questions. Respondents were asked whether and how they intended to vote in the upcoming election, how many seats they wanted each party to hold in Parliament, how much they cared about the outcome of the election, and how they had voted in the previous (1966) election. In addition, they were asked questions designed to elicit their knowledge of political and demographic realities in South Africa, their sources of political information, their reasons for liking or disliking the various parties, their personal and family backgrounds (income, occupation and father's occupation, current and childhood language, religion, place of origin and parents' place of origin), their feelings of "closeness" toward different ethnic and racial groups in the country, including their own, and their perceptions of the various parties' ideological stances on a number of substantive political issues being debated at that time in South Africa. For the social distance questions, the "ladder" approach developed by Cantril[15] was used.

The three geographical districts surveyed were chosen so as to reflect the diverse composition of the national as well as the Cape Town electorate. Bishopscourt, falling within the constituency of Constantia, is an upper-class, English-speaking area in which the UP typically engages in a lopsided contest with the Progressives (Progs). Gardens, almost identical to the election district called Cape Town Gardens, is a heterogeneous middle-class area. As one of the older English and immigrant suburbs, it was once a solid UP constituency, but in recent years it has seen increasing penetration by upwardly mobile Afrikaners, and with them the National Party. Epping, part of the election district of Tygervallei, is a lower-class area containing many newly urbanized Afrikaans railroad workers living in mass-produced government housing projects. Along with rural Afrikaans districts, it is an example of the kind of constituency that returns the Nats to power in election after election.

The numbers of respondents allotted to each district were proportioned so as to match the socioeconomic breakdown of the voting population of Cape Town. Sampling was based on the 1960 census figures, since the 1970 figures had not yet been made public. The sample base was the total income-earning white male population of Cape Town, which we divided on an *a priori* basis into the following income and socioeconomic status categories: (1) above 5500 rands a year, (2) between 1600 and 5500 rands and (3) below 1600 rands. (One rand is equal to about $1.40, but cost-of-living differences make the correspondence between South African and American status groupings only an approximate one.) According to census statistics, 5 per cent of the sample base fell into the upper-class category, 51 per cent into the middle-class, and 44 per cent into the lower-class.

up of a majority of appointees. For a study of the Coloured election held in 1970, see Stanley J. Morse and Stanton Peele, "'Coloured Power' or 'Coloured Bourgeoisie'? A Survey of Political Attitudes Among Coloureds in South Africa," *Public Opinion Quarterly* (Fall, 1974).

[14] Leslie Kish, "A Procedure for Objective Respondent Selection Within the Household," *Journal of the American Statistical Association*, 44 (September, 1949), 380–387.

[15] Hadley Cantril, "A Study of Aspirations," *Scientific American*, 208 (February, 1963), 41–45.

Table 1. Distribution of Sample by Constituency

Area	*N*	% of Total *N*	Modal Income Group
Bishopscourt	25	5.4	above R6000
Gardens	239	51.7	R3000–R5999
Epping	198	42.9	R1200–R1999

These percentages were then used as quotas for the three sampling areas.

The class breakdown of our final sample of 462 respondents closely parallels the percentage distribution of upper-, middle-, and lower-class voters in Cape Town. Table 1 shows that the desired percentage breakdown according to area was achieved. By listing the modal reported income range for each area (from the five income groups into which the total sample was eventually broken down for analytic purposes), the table also shows the accuracy of the class designations assigned the three areas, allowing for a general upgrading of income in South Africa over the last decade, especially for the working class. The sample also matched the Cape Town census on other important demographic characteristics, although the choice of Epping, an almost exclusively Afrikaans-speaking area, as our lower-class district caused the overall sample to contain a higher percentage of Afrikaans speakers than does the Cape Town population (39 per cent vs. 33 per cent). We welcomed this deviation, however, since we were more concerned with finding an area representative of the country as a whole (which is predominantly Afrikaans-speaking) than with sampling according to Cape Town's demography.

Since voting intention was to be our primary dependent variable, we compared this response as given by our three sample districts with the actual election returns. Here, too, the only problem arose in Epping. In Constantia, David Duncan Baxter of the UP won approximately two-thirds of the votes cast in a contest with a Prog opponent. Our Bishopscourt sample reported that 50 per cent intended to vote UP, 11 per cent Nat, and 39 per cent Prog. The difference (favoring the Progs as against the UP) is not very meaningful, since the small number of respondents our sample allotted to Bishopscourt made a sampling error unavoidable here. In addition, as one of the wealthiest, most exclusive sections of Constantia, Bishopscourt could be expected to be a Prog stronghold within that constituency. Gardens gave a much closer approximation of the actual vote, probably because it is more nearly coterminous with its electoral district, Cape Town Gardens. There the UP candidate, Henric Adolphe van Hoogstraten, received 58 per cent of the vote, as compared with the 61 per cent of Gardens respondents who indicated that they would vote UP.

In Epping, however, the prototypically Nationalist allegiance of the Tygervallei constituency meant that no election was held, since the Nat candidate went unopposed. Nonetheless, 55 per cent of the Epping sample reported that they intended to vote in the election; of these, 70 per cent said that they would vote Nat. Before inquiring into the lack of political awareness that produced these responses, we can review two checks on the validity of these intentions as actual indices of electoral sentiments. One is the remarkable consistency in the party preferred for national leadership (based on the number of seats respondents wanted each party to have in Parliament) by Epping respondents who did intend to vote and those who did not. In both cases, almost exactly two-thirds of respondents preferred the NP. The other was the equally strong similarity between the reported voting intention of Epping respondents (70 per cent for the NP) and the actual vote in Germiston, a similarly composed Afrikaans blue-collar district near Johannesburg (69 per cent for the NP). From these figures, we concluded that the intentions expressed by Epping "voters" offer an accurate projection of the electoral tendencies of a constituency whose disposition was all too well known to the parties which did not contest the election.

Since Epping residents who thought there would be an election were obviously misinformed, we hypothesized that Epping would show a reversal of the usual finding that people who do not intend to vote are less politically informed and involved than those who do. Table 2 lists the mean

Table 2. Level of Political Information by Constituency and by Intention to Vote

Constituency	Voting Intention: Intend to vote	Voting Intention: Don't intend to vote
Bishopscourt and Gardens	3.2 ($N=199$)	3.8 ($N=60$)
Epping	4.1 ($N=108$)	3.6 ($N=88$)

F (constituency)* = 23.1, $p<.001$
F (interaction) = 23.6, $p<.001$
Cell entries are mean *errors* made on information index.

* In this and all other tables which employ analyses of variance, F ratios and levels of significance are given only for predictor variables (or factors) which are significant, and these are identified within parentheses.

general information scores[16] for respondents who did and did not intend to vote in Epping and in the other two districts. An analysis of variance does show a significant interaction effect (see Methodological Appendix for clarification of use of analysis of variance in this and other cases). That is, while those people who said they were not going to vote were *less* politically knowledgeable in the other areas, they were *more* so in Epping. There is also a significant main effect for area; i.e., Epping respondents were less well informed than those in Bishopscourt and Gardens (although the information scores for the entire sample are generally low, confirming the findings of other election studies).

The fact that a majority of the Epping electorate was unaware of the most elementary fact about the election—its nonexistence—led us to examine sources of political information for such voters. The interviewers asked respondents how much they had heard about the election from newspapers, radio, friends, family, people at work, ministers, and employers. One-way analyses of variance comparing Epping with the other two areas show that Epping respondents heard more about the election from their ministers ($F=6.2$, $df=1, 411$, $p<.05$), while those in Bishopscourt and Gardens obtained more information from the radio ($F=21.7$, $df=1, 441$, $p<.001$) and from newspapers ($F=13.9$, $df=1, 447$, $p<.001$). In addition, residents of these two districts read the newspaper with much greater frequency than did residents of Epping ($F=41.8$, $df=1, 446$, $p<.001$). These results emphasize the meaninglessness of an ideological campaign (even a campaign like that conducted by the HNP, which addressed itself to the prejudices of an Afrikaans constituency) in an area like Epping, where political information is disseminated primarily through the traditional Nat-controlled channel of the church.

Demographic Determinants of Respondent's Vote

Intended Vote. Our analysis is concerned first with the various demographic and sociological predictors of party vote, using the respondent's intended vote as the dependent variable. To begin with, age by itself makes no difference at all ($\chi^2=6.0$, $df=8$). Thus the widely hoped for movement of young voters from the Nats to the UP is not discernible in our data, although Prog support does increase in the younger age groups (up to 8 per cent of those under 25).[17] This does not, however, remove the possibility that age may be significant in conjunction with other variables, such as education. We will return to that question.

The next set of variables to be examined consists of measures of socioeconomic status. Starting with father's occupation, 34 per cent of the UP voters and 92 per cent of the Prog voters come from families in which the father held a white-collar or professional position, as compared with only 23 per cent of the Nat voters, the other 77 per cent of whom had fathers who worked as farmers, or blue-collar or unskilled workers ($\chi^2=28.8$, $df=8$, cont. coeff. $=.33$, $\text{lambda}_{\text{vote}}=.14$, $p<.001$). These distinctions among party supporters diminish somewhat when the respondent's own (or in the case of a housewife, her husband's) occupation is used in place of father's occupation ($\chi^2=20.6$, $df=8$, cont. coeff. $=.30$, $\text{lambda}_{\text{vote}}=.11$, $p<.01$). The statistical diminution from father's to own occupation is a reflection of the rapid rise of Afrikaners into the middle class.

Dividing respondents into groups according to their income and education and crossing these categories with intended vote responses does not produce strongly significant relationships. With income, we find no significant differences between UP and NP voters, although Prog voters place disproportionately into the highest category, and out of the lowest. With educational level, on the other hand, we obtain a chi-square value about as high as that obtained for father's occupation ($\chi^2=27.1$, $df=8$, cont. coeff. $=.30$, $\text{lambda}_{\text{vote}}=.10$, $p<.001$). Better-educated voters are somewhat more likely to vote for the UP than the NP. At the level of university attendance, we find 46 per cent of the Prog voters placing into this category as against 12 per cent of UP and 5 per cent of Nat voters.

The SES-vote relationships are summarized in Table 3, which is based on a combined SES index constructed from the component variables of occupation, education, and income. Average scores for the three measures (or two, if one question wasn't answered) were computed, and then assigned in equal numbers to the three categories so as to achieve an even distribution. The expected relationships appear unambiguously in the table, but the significance test results are disappointing.

SES, then, is only of moderate value as a predictor of voting behavior, and the relationships which do emerge from the data may well be a

[16] This score consists of the number of *incorrect* responses to items which asked (1) the name of the respondent's current M.P., (2) the current M.P.'s party, (3) the name of the Nat candidate in respondent's constituency, (4) the total number of seats in Parliament (scored as correct if in the 150–180 range, the actual figure being 166), and (5) the total population of South Africa (scored as correct if in the 15,000,000–25,000,000 range, the actual figure being approximately 20,000,000). A score of 0 indicates all correct answers, a score of 5 all incorrect answers.

[17] The small number of Prog (and HNP) voters makes their impact on the overall statistics negligible, and the statistics regarding these parties should be regarded merely as providing a descriptive profile of their supporters.

Table 3. Voting Intention by SES Level

SES	Intended Vote NP	UP	Prog
High	26%	41%	73%
Middle	40%	38%	27%
Low	34%	21%	0%
	100% (117)	100% (122)	100% (11)

$\chi^2 = 14.2$, $p < .01$
c.c. = .23, $\text{lambda}_{\text{vote}} = .11$

function of the covariance between SES and ethnicity. To explore this possibility, we must turn to other demographic variables which appear to offer a closer approximation to ethnicity. One such variable is the urban-rural dimension. While everyone in our sample now lives in an urban environment (Cape Town), respondents split decisively in their intended votes according to their having had a rural or urban birthplace, as shown in Table 4. In this table, the contingency coefficient and lambda are higher than those for any of the SES indicators. Religion, which ties in even more closely with ethnicity than does birthplace (since almost all Afrikaners are members of the Dutch Reformed Churches) shows an even stronger relationship to vote. DRC members are overwhelmingly Nats, while Anglicans, Catholics, and Jews are overwhelmingly UP partisans (with the Jews also giving the Progs heavy support), and non-Anglican Protestants show a slight UP preference ($\chi^2 = 92.7$, $df = 12$, cont. coeff. = .50, $\text{lambda}_{\text{vote}} = .39$, $p < .001$).

The importance of religious differences as a mirror, and possibly a determinant, of political differences suggests that the religiosity of a respondent may have something to do with his political preference. We would expect supporters of the more conservative (National) political party to attend church more frequently, especially since the staunchly Calvinist Dutch Reformed Churches demand a more total involvement of their members than do any other religious bodies in South Africa. In fact, Nat voters do attend church more frequently ($\chi^2 = 14.8$, $df = 60$, cont. coeff. = .23, $\text{lamda}_{\text{attendance}} = .16$, $p < .05$). These results characterize the Nat voter as a participant in what is more a cultural than a merely religious institution. As Thompson comments, all of the three branches of the DRC

> have been intimately associated with the rise and triumph of Afrikaner Nationalism and have always propagated the ideal of an exclusive Afrikaner nation with a divine mission. . . . Consequently, the typical Dutch Reformed Church congregation is as much a racial corporation as the Afrikaner family or school: a racial corporation with the special function of invoking the awe-inspiring rituals and sanctions of fundamental Calvinism in support of the separation of South African society into racial units.[18]

With this link between the Church, the Afrikaner ethos, and Nationalist politics, we should expect childhood language (as the closest approximation of ethnicity) to be the best predictor of voting behavior. Table 5 shows this to be the case. The lambda for childhood language is the highest we have yet found. More important, childhood language is a better predictor of vote than the respondent's *current* language (the lambda for current language predicting intended vote is .36). If we can tell better how a person is going to vote on the basis of his father's occupation rather than his own, and on the basis of the language he spoke in childhood rather than the language he speaks now, then it is his *ethnic background*, rather than his *current status in society*, that most strongly determines his party preference.

We have seen that we can use childhood language as a good approximation of ethnicity. In addition, we can improve it for this purpose by eliminating people who spoke foreign languages as children, and by assigning childhood bilinguals to either the Afrikaans or English-speaking category on the basis of which social distance questions they choose to answer (i.e., do they answer "How close do you feel to other Afrikaners?" or "How close do you feel to other English-speaking South Africans?"). With these adjustments in the predictor variable, the contingency coefficient rises to .48 and the lambda to .45. In addition, the lambda value for the vote predicting the social category (.48) is now higher than that for the social category predicting the vote—something that was not true of any of the other variables we tested. In other words, we are just as able to predict a person's ethnic background from the party he votes for as vice versa. These data enable us to

[18] Thompson, *The Republic of South Africa*, p. 103.

Table 4. Voting Intention by Rural-Urban Background

Background	Intended Vote NP	UP	Prog	Total
Urban	34%	60%	6%	100% (152)
Rural	70%	29%	1%	100% (87)

$\chi^2 = 30.8$, $p < .001$
c.c. = .34, $\text{lambda}_{\text{vote}} = .29$

term our revised predictor variable "ethnicity" rather than childhood language. (The resulting placement of the Jews and other non-British immigrants with the "English" accurately reflects these groups' identification with the English-speaking subculture.)

Intended Vote with Ethnicity Controlled. If other predictor variables achieve significance only because they are related to ethnicity, then the tables for these variables can be read as simply providing descriptive profiles of the supporters of each of the parties—e.g., that three-quarters of the Prog voters, but only one-quarter of the Nat voters, come from the highest socioeconomic class. By controlling for ethnicity and looking at the operation of other ostensible predictor variables *within* ethnic groups, we can test whether they have any independent impact on voting behavior, and we can also isolate some significant phenomena which occur only for one of the two ethnic groups.[19]

We can begin by returning to age, which proved insignificant as a predictor of vote over the entire sample. Since political change in South Africa must come from the Afrikaans majority, it is instructive to see whether the intended vote responses of younger Afrikaners reveal any disillusionment with the NP. Our data show no such trend. We get a different picture, however, when the effects of education are added to those of age. (Age itself is not related to income, and there is only a small negative relationship between age and education: $r = -.26$.) For Afrikaners under 35 years of age, the suggestive results in Table 6 point to a possible scenario for electoral change. It appears that the young Afrikaner is lured away from total commitment to the National Party when he has a chance to expand his outlook through formal education.

[19] A statistical caveat: The very effectiveness of ethnicity as a predictor makes for small *N*'s in some cells of the controlled data. Only 33 of the 149 English respondents who answered the voting question said they would go Nat. And only 28 of the 117 Afrikaans *R*s who gave their intended vote reported that it would be for the UP. In the tables presented in this section, actual *N*'s will accompany percentages so that no mistakes about the extent and significance of a trend can occur. (For further comments, see also the Methodological Appendix.)

Table 5. Voting Intention by Childhood Language

Childhood Language	Intended Vote			
	NP	UP	Prog	Total
English	23%	68%	9%	100% (134)
Afrikaans	76%	24%	0%	100% (112)
Bilingual	32%	63%	5%	100% (19)
Other	47%	47%	6%	100% (19)

$\chi^2 = 73.0$, $p < .001$
c.c. = .45, $\text{lambda}_{\text{vote}} = .40$

Table 6. Voting Intentions of Afrikaners Under 35 Years Old by Educational Level

Educational Level	Intended Vote	
	NP	UP
Standard I–V	6% (2)	0% (0)
Standard VI, VII	28% (9)	18% (2)
Standard VIII, IX	41% (13)	27% (3)
Standard X	19% (6)	37% (4)
University	6% (2)	18% (2)
	100% (32)	100% (11)

c.c. = .28.

Without the age control, education alone does not produce significant results for the Afrikaner sample. The slight trend for the English ($\chi^2 = 16.1$, $df = 8$, cont. coeff. = .32, $p < .05$) is really distinct only for Prog supporters. The other SES measures (income, father's and respondent's occupation), with the ethnic control, are even less meaningful, as is evident from Table 7, which employs the combined SES index that was used for the overall sample in Table 3. Once respondents have been stratified according to ethnicity, further variations in their voting intentions cannot be explained by the independent operation of socioeconomic factors.

Nonetheless, one finding of consequence does emerge from these data. On all of the SES measures the contingency coefficients are considerably higher for the English than the Afrikaans subsample. This consistency is reflected in the overall contingency coefficients for Table 7. The same relationship holds for the influence of area. With ethnicity controlled, there is still a significant variation in the vote produced by constituency (Bishopscourt, Gardens, or Epping), but it is much stronger among the English-speaking than the Afrikaners (for English, $\chi^2 = 29.3$, $df = 4$, cont. coeff. = .40, $p < .001$; for Afrikaners, $\chi^2 = 3.8$, $df = 1$, cont. coeff. = .18, $p < .05$). The fact that both SES and area influence the English-speaking person's vote more than the Afrikaner's says

Table 7. Voting Intention by Ethnicity and SES Level

	Intended Vote (Afrikaners)		Intended Vote (English)		
SES	NP	UP	NP	UP	Prog
High	20% (17)	25% (7)	37% (10)	45% (38)	64% (7)
Middle	35% (29)	39% (11)	52% (14)	39% (33)	36% (4)
Low	45% (37)	36% (10)	11% (3)	16% (14)	0%
	100% (83)	100% (28)	100% (27)	100% (85)	100% (11)

c.c. (Afrikaners) = .08
c.c. (English) = .18

something important about the flavor of South African politics. This is that ethnicity is more nearly an all-powerful force in determining voting behavior among the Afrikaners. It must be reiterated, though, that the difference between them and the English in this regard is only one of degree.

Applying the ethnic control to demographic predictor variables which are themselves closely associated with ethnicity, such as birthplace and religion, seems almost a futile gesture. Nevertheless, some findings are of interest. The urban-rural birthplace comparison is meaningless with the overwhelmingly urban-born English respondents, but for Afrikaners the results approach significance ($\chi^2 = 3.3$, $df = 1$, cont. coeff. = .18, $p < .10$). Four times as many rural-born Afrikaners vote for the NP as the UP, while the corresponding ratio for urban-born Afrikaners is only 2 to 1. This means that urbanization may lessen the Afrikaner resistance to voting UP, though it is probably safer to say merely that of those Afrikaners who turn to the UP, a disproportionate number were born in urban settings.

With religion, the few Afrikaans respondents who are Anglican or Catholic, and the few English respondents who are members of the Dutch Reformed Church, split evenly between the Nats and the UP. Affiliation with a church traditionally associated with the other ethnic group is clearly the kind of basic cultural change that brings with it political deviation as well. The numbers of respondents showing this ethnic-religious conflict, however, are too minuscule to be of either statistical or political significance.

A more substantial illustration of the phenomenon of the "ethnic mobility" implied by membership in an ethnically atypical church is provided by examining current language as a predictor of vote, controlling for ethnicity. Since our ethnicity variable is based primarily on childhood language, we are in effect comparing the political behavior of people who have changed their language since childhood with that of people who have not. Hardly anyone, of course, who speaks English as a primary language in childhood shifts later to Afrikaans, and political change among English-speaking South Africans does not occur by a process of ethnic assimilation. Rather, an Englishman's voting for the Nats is probably a more purely political gesture. The winds of cultural assimilation in South Africa blow almost always in the direction of Afrikaners assimilating themselves to English culture. And Table 8 shows that a majority of Afrikaans respondents who now speak English also vote for the UP. This is far and away the most significant table produced with the control for Afrikaans ethnicity. Unlike more superficial, external measures of sociological change, the current-language variable (the wording of the question was "What language do you speak at home now?") does locate the Afrikaner who has, to some extent, internalized the press of the surrounding English culture, the Afrikaner whom we might label as "conflicted."[20]

The importance of linguistic self-identification as an index of ethnic self-identification recalls the fact that a majority of respondents who listed Afrikaans as their current language still carried out their interview in English. Since 31 per cent of Afrikaans-speaking respondents who answered in English, as against only 8 per cent of those answering in Afrikaans, indicated an intention to vote for the UP ($\chi^2 = 6.7$, $df = 1$, cont. coeff. = .26, $p < .01$), we might ask whether these respondents were tailoring their actual answers to questions, along with their choice of language for the interview, to suit the English interviewers. That is,

[20] Many writers have attributed great psychological and political significance to a shift in home language for the Afrikaner. Cf. Edwin S. Munger, *Afrikaner and African Nationalism* (London: Oxford University Press, 1967).

Table 8. Voting Intention of Afrikaans Respondents by Current Language

	Intended Vote		
Current Language	NP	UP	Total
English	46% (6)	54% (7)	100% (13)
Bilingual	79% (11)	21% (3)	100% (14)
Afrikaans	81% (72)	19% (17)	100% (89)

$\chi^2 = 10.8$, $p < .01$, c.c. = .29.

were their responses shaped by "interviewer effect"?[21]

We have two reasons for thinking that this was not the case. One is the close correspondence, reported earlier, between the intended vote responses and the actual election results in the same districts. The second is that Afrikaans-speaking respondents who answered in English were significantly younger, better educated, more urbanized, wealthier, and therefore presumably less purely malleable, than those who did not (using the summary SES index, $\chi^2 = 29.2$, $df = 2$, $p < .001$). These are Afrikaners who would be susceptible to the immediate influence of an English interviewer only because of their larger involvement with English culture, and the overall psychological pressures leading them to act like the English, as by actually voting UP.

Conflicted Voters—the Ambiguous Afrikaner. To delve more deeply into the motivational and behavioral makeup of the conflicted Afrikaner, we performed a median split on Afrikaans respondents according to their answers to the question, "How close do you feel to English-speaking South Africans?" Our prediction was that those who felt closer to the English would be better prepared psychologically to support the UP. Disappointed at the insignificant results, which involved a large number of cases with no response to the closeness question, we found that many Afrikaans respondents who had not answered this question had answered the questions, "How close do you feel to *other* English-speaking South Africans?" and "How close do you feel to Afrikaners?" rather than "How close do you feel to other Afrikaners?" and to "English-speaking South Africans?" as we had intended. In other words, we had uncovered a group of respondents who regarded themselves as English even though our best objective ethnic indicator placed them in the Afrikaans category.

We have labeled these respondents "ambiguous Afrikaners." For this group, a small majority of whom support the UP, subjective ethnic label is slightly better than objective label in predicting to vote—which is what the authors of *The People's Choice*[22] found for subjective versus objective SES measures in America. Table 9 shows that Afrikaans respondents who identify themselves as English are far more likely to vote for the UP than those with a consistent (Afrikaans) objective and self-identification. That ethnic self-identification is a more successful predictor than current language is not surprising, since it more directly

[21] Daniel Katz, "Social Psychology and Group Process," in *Annual Review of Psychology*, ed. C. P. Stone (Stanford, Calif.: Annual Reviews, Inc., 1951).

[22] Lazarsfeld et al., *The People's Choice*.

Table 9. Voting Intention of Afrikaans Respondents by Ethnic Self-Identification

Subjective Ethnic Placement	Intended Vote		
	NP	UP	Total
Afrikaans	85% (76)	15% (13)	100% (89)
English	44% (12)	56% (15)	100% (27)

$\chi^2 = 17.3$, $p < .001$, c.c. = .36

embodies the social-psychological process which is involved in a change of language. The nearly even UP-NP split among ambiguous Afrikaners shows, however, that we still cannot specify which voters of Afrikaans extraction will desert to the UP. And there is no immediately obvious way to subdivide the ambiguous Afrikaner category so as to achieve this prediction, since the NP and UP voters within this group show similar distributions on such key dimensions as current language and feeling of closeness to Afrikaners. Furthermore, we cannot decisively distinguish the group as a whole from other Afrikaners on such dimensions as SES (where they are almost evenly spread among the three categories), religion (three-quarters still belong to the DRC), or language (less than half use English exclusively). It is particularly worthy of note that the ambiguous Afrikaners' self-identification as English refers to something deeper than current language usage. We will return to these questions when we analyze the data on respondents' political and social attitudes. But it should be apparent that the mere existence of this group of Afrikaners who give up their ethnic identity,[23] together with its size (numbering 43 out of 210 Afrikaans respondents in our study), looms as a social fact of major importance for South Africa.

Ambiguous Afrikaners are one focus of the cross-pressures hypothesis as applied to South Africa. Along with them, we can look at voters who actually express ethnically atypical choices (Afrikaans UP supporters and English NP supporters), and those who appear to be in a sociologically "conflicted" position (especially urban or high-SES Afrikaners), for signs of cross-pressure conflict. Unfortunately, our data do not permit us to examine some of the usual signs of

[23] Afrikaans sociology characterizes such individuals as having the same self-hate that some blacks in the United States and Jews in Europe are said to exhibit. S. Pauw, in *Die Beroepsarbeid van die Afrikaner in die Stad* (Stellenbosch: Pro Ecclesia, 1946), speaks of the "spiritual slavery" which Anglicization meant for Afrikaners who sought admission to the English economic, cultural, and educational elites.

such conflict. Abstention from voting, for example, is ruled out as an indicator by the altered significance of the nonvoting response in Epping. As for partisan instability, only 21 English-speaking respondents and 9 Afrikaners reported that they would vote differently from the way they had in 1966. These *N*'s are too small to work with statistically, but there was a trend for greater electoral change to take place among the English, and (as the cross-pressures hypothesis would predict) among voters of both ethnic groups who broke away from ethnically determined voting. Changers do not appear differentially by SES within either ethnic category, which again suggests that social mobility, as it is usually defined, is not operating to cause political change in South Africa.

On the whole, our results do not provide much substantiation of the cross-pressures hypothesis. Out of a number of possible tests measuring information avoidance and reduced interest in the election, only a few yield significant results, and these at low levels of significance. Within the English sample there is no evidence whatever of cross-pressure conflict, which is just what we would expect from the less highly charged cultural implications of deviant voting among the English-speaking. Even among Afrikaners there is no overall pattern of information avoidance in high-SES or UP-supporting respondents, although there are a few significant instances of avoidance of information from particular sources. Afrikaans UP supporters get somewhat less information from the family than NP supporters do ($F=3.6$, $df=1$, 114, $p<.10$), which is revealing since party supporters over the entire sample do not differ on this dimension. That nonconforming Afrikaners rely less on the family for political information is consistent with the Lazarsfeld-Berelson emphasis on family disagreement as an important manifestation of cross pressures.

A finding of even stronger significance is that Afrikaans Nat voters care more about the election than do UP voters ($F=6.3$, $df=1$, 114, $p<.05$). Again, this finding does not hold for the two parties' supporters in the overall sample. Finally, urban-born Afrikaners are significantly less concerned about the outcome of the election than the English urban-born ($F=5.0$, $df=1$, 199, $p<.05$). Since Afrikaners as a whole are slightly *more* concerned about the election than the English are, this could be a sign of psychological withdrawal because of cross-pressure conflict. These are the strongest indications we have of such conflict, and by themselves they are not conclusive.

Party Preferred. The respondents' party preference at the national, parliamentary level can be used to supplement voting intention as a dependent variable. Information on this variable was obtained by asking respondents, "How many seats would you like the _____ Party to have in Parliament?" the question being repeated for each party. Responses to this series of questions yield two different measures of party preference—which party the respondent wants to see gain the most seats, and the strength or degree of his preference as indicated by the percentage of seats he assigns to each party.

Because the anomalous character of the intended vote response in Epping does not carry over to party preferred, and because abstainers in every district usually had an opinion about which party they wanted to control Parliament, a much larger percentage of respondents could be coded on this measure than on intended vote. Also, the interval data generated by the desired percentage breakdowns make it possible to use correlations to gauge the strength of party allegiances and to relate attitudes and perceptions to party preference. Beyond these methodological advantages, party preferred has distinct substantive implications. In a parliamentary system such as South Africa's, the voter does not express his preference directly for national leaders. By presenting the respondent with the hypothetical issue of control of the national government, the party preferred question can tap a range of feelings and beliefs which go unexpressed under the constraints of a concrete (and limited) electoral choice in a particular constituency. Such attitudes may include inchoate discontents and desires for change that are submerged under the reality of National Party dominance.

An observer might well suppose that the party preferred dimension would show cracks forming in the Nationalist façade. But the results of comparing the actual vote and preferred party responses, as shown in Table 10, suggest just the opposite. Of course, the increased Nat total derives at least in part from the full representation of Epping voters (45 per cent of whom abstained from giving an intended vote response) on this question. And the larger vote for the minor parties (especially the Progs) is a natural consequence of the fact that potential supporters living in districts where these parties do not run candidates, or do not stand a chance of winning, can only indicate their support in some hypothetical form. But it still appears that the UP, the official opposition party, does not benefit from a shift from the real to the hypothetical plane.

The significance of these figures is made more apparent by Table 11, which shows how those intending to vote for each party stand in terms of their preference for which party they would like to control Parliament. Twenty-nine per cent of the UP voters defect on the latter question, as

Table 10. Comparison of Intended and Preferred Voting Profiles for Overall Sample

	NP	UP	Prog	HNP	Total
Intended Vote	46% (134)	49% (142)	5% (13)	0% (1)	100% (290)
Preferred Vote	52% (203)	34% (135)	13% (50)	1% (3)	100% (391)

against 16 per cent of the Nats and only 8 per cent of the Progs. Owing to the infusion of Epping respondents, a majority of the nonvoters is now seen actually to prefer the Nats, but it is the Progs whose potential support comes in greatest proportion from nonvoters, as well as from those voting for other parties. It is not surprising that the highly sophisticated, nonconformist Prog voters are the most resolute in backing their votes with an expressed preference for their party. But what is truly informative here is the confirmation of Lever's[24] finding that UP voters are less firm supporters of their party than Nat voters. It seems that many people vote for the UP without any real desire that it overthrow the Nationalist regime, while others fall back on the UP as a substitute for their real choice, the Progressive Party.

It is most meaningful to compare actual versus preferred NP and UP support in Gardens, the one district included in our survey in which both parties ran candidates in a reasonably close contest. Here again, as shown in Table 12, the UP loses more of its voters on the party preferred question than do the other two parties. More strikingly, twice as many abstainers prefer the Nats as prefer the UP, so that this ratio in the overall sample is not merely a function of the Epping effect.

When these data are examined from the other

[24] Lever, *The South African Voter.*

Table 11. Preferences for Control of Parliament by Voting Intention

Intended Vote	Party Preferred				
	NP	UP	Prog	HNP	Total
NP	84%	10%	4%	2%	100% (116)
UP	18%	71%	11%	0%	100% (123)
Prog	8%	0%	92%	0%	100% (13)
No Vote	58%	26%	15%	1%	100% (139)

Row entries indicate expressed national preferences of respondents intending to vote for a given party.

Table 12. Preferences for Control of Parliament by Voting Intention in Gardens

Intended Vote	Party Preferred			
	NP	UP	Prog	Total
NP	84%	14%	2%	100% (56)
UP	18%	69%	13%	100% (91)
Prog	0%	0%	100%	100% (5)
No Vote	50%	26%	24%	100% (50)

$\chi^2 = 8.8$, $p < .01$.

direction (that is, percentaging by column), a chi-square test shows that respondents who preferred the Nats were significantly less likely to vote for the Nats than those who preferred the UP were to vote for the UP (combining appropriate rows, $\chi^2 = 8.8$, $df = 1$, $p < .01$). In particular, many people who regard themselves as Nat supporters apparently did not vote at all in this election. Lever[25] hypothesizes that these abstentions were precipitated by the HNP campaign, which created in some right-wing Nats a conflict between their historical allegiance to the NP and their agreement with the HNP platform. This seems implausible on several grounds. One is that the abstentions significantly affected the election results in relatively liberal NP-UP swing districts, such as Gardens, where the HNP did not run candidates. In addition, our data show that a majority of the Nat-preferring abstainers in Gardens were English-speaking people for whom the HNP would have no appeal, but who were passively expressing an acceptance of Nat rule while being unable to bring themselves to vote for this traditionally Afrikaans party. Finally, the abstentions are tied up with the related phenomenon of self-proclaimed Nat supporters voting for the UP or the Progs—a kind of disaffection which could be interpreted as being liberal or possibly nonideological, but not reactionary. We can conclude that the Nat-preferring abstainers represent two phenomena—a disillusionment of traditional Nat voters, and a reversion to the UP by those converted in previous elections. On the other hand, many of these people could be counted on to return to the Nats if the party were under any real electoral pressure.

With the interval data of percentage distributions of desired seats for the various parties, we move further into the realm of hypothetical party preferences. Comparing the percentage of seats

[25] *Ibid.*

Table 13. Correlations Between Percentage of Seats Desired for Each Party by Ethnicity

	(Afrikaans Respondents)					(English Respondents)			
	NP	UP	Prog	HNP		NP	UP	Prog	HNP
NP					NP				
UP	−.77				UP	−.65			
Prog	−.51	−.02			Prog	−.43	−.35		
HNP	−.43	−.18	.50		HNP	−.03	−.15	−.05	

Correlations reported here are Pearson product moment correlations; *N*s are all around 400, so that any $r>.10$ is highly significant.

the voters for each party want to see their *own* party capture in Parliament, we find that Nat voters want the NP to gain more seats than UP voters want the UP to have (66 per cent versus 57 per cent, $p<.01$). This supports our previous finding that Nat voters are stronger in their party loyalty. Table 13 gives the results of a correlational analysis of the relationships between percentages of seats desired for each of the parties by respondents in the two major ethnic categories. The highest correlation for both ethnic groups is the predictable negative one between Nat and UP seats desired. The negative correlation between the UP and the Progs for the English only, and between the Nats and the HNP for the Afrikaners only, can be explained by the fact that Afrikaners for the most part choose within the HNP-Nat-UP range, the English within the Nat-UP-Prog range. Thus the Prog-UP choice is meaningful only for the English, the Nat-HNP choice only for the Afrikaners. The strong positive correlation between the Progs and HNP for Afrikaners makes no sense ideologically, but turns out to reflect an underlying trend for high-SES Afrikaners to desire fewer seats for both the Progs and the HNP than do low-SES Afrikaners. We will have more to say about this paradox in the following section on political attitudes, though we can note here that support for the HNP is almost entirely a lower-class phenomenon, cutting across ethnic boundaries to some extent. The negative relationship between SES and HNP seats desired is highly significant for both English ($F=12.1$, $df=2, 139$, $p<.01$) and Afrikaners ($F=5.3$, $df=2, 176$, $p<.01$).

Attitudes and Perceptions

The previous section of this paper was concerned with a behavioral question—who votes for which party? Data on the political attitudes and perceptions of our respondents can in some cases help explain the voting behavior we have observed, and in other cases show evidence of beliefs and perceptions which are *not* expressed behaviorally.

Party Perceptions, Party Vote and Political Ideology. To examine the political attitudes of respondents supporting each party, and perceptions of the political stance taken by each party, respondents were presented with fifteen 6-point Likert-type attitude statements dealing with topics of current political concern in the country such as "A great deal of money should be spent to develop the Bantu homelands" and "Blacks should only be allowed in White areas to work." (For the remaining items see the Methodological Appendix.) To construct a measure of political liberalism, those items which seemed on an *a priori* basis to fall unambiguously along a liberal-conservative continuum were coded so that a high score always indicated a more liberal position. Mean scores over all these statements were then computed for respondents' own positions on the issues and the positions they attributed to each political party.

As Table 14 shows, our respondents as a whole perceived the parties to lie on our liberalism scale in their commonly assumed order of liberalism. This is so despite the fact that the UP, with its amorphous and ideologically inconsistent policies, is not necessarily more liberal than the Nats in reality. It is quite likely that the UP's perceived liberal position vis-à-vis the Nats is a symbolic one, based on the UP's role as the opposition to an admittedly conservative ruling party. Table 14 also suggests one reason for the failure of the HNP's campaign—the electorate's inability to distinguish the HNP's ideological stance from that of the Nats. In general, the orderly arrangement of the parties on this scale helps validate our

Table 14. Mean Perceptions of Party Liberalism for Overall Sample

HNP	NP	UP	Prog
2.7	2.8	3.7	4.2

The higher the number, the more liberal the response.

attitudinal measures. Thus the mean self-attitude score of 3.5 (hardly more conservative than the perceived UP position, and about as close to the Progs on the left as to the Nats on the right) can be given some credence as an indication that the South African electorate, at least in the Cape, is more liberal than the party it helps maintain in power. Less liberal regions might show less of a discrepancy, but the ability of Cape voters to tolerate the discrepancy only illustrates the South African's readiness to vote on other than ideological grounds.

The mean attitude scores by party vote show that this discrepancy extends across the board, with supporters of each party in the Cape being somewhat more liberal than the total sample's perception of that party. (The mean self-attitude liberalism scores for Nat voters is 3.1, for UP voters 3.8, and for Progs 4.8, $p<.001$.) The discrepancy is larger for Nat supporters than for UP supporters ($p<.05$). Supporters of all three parties, however, resolve this discrepancy by perceiving their own party to be more liberal, and thus closer to their own position, than does the total sample. For example, supporters of the NP give that party a score of 3.0, as compared with the total sample's judgment that the party stands at 2.8. At the same time, respondents bring *all* parties closer to their own position, with Nat voters, for example, seeing the Progs as being *less* liberal than any other group does. Nat voters give the most conservative ratings to all parties *except* their own (thereby drastically minimizing the differences between their party and the others), and Prog voters give the most liberal ratings to all parties *including* their own. Perceptual distortion thus takes the form of a general liberal bias together with a standard assimilation effect.

Having found that party supporters differ ideologically in a direction consistent with the conventional images of the parties, we can note that attitudes follow along ethnic lines in a predictable way as well. The mean liberalism score for English-speaking people is 3.9; for Afrikaners, 3.1 ($p<.001$). But which is the better predictor of attitudes—ethnicity or party vote? Table 15 shows the relationship between party vote and political attitudes within each ethnic group. As the F ratios show, *ethnicity* is a more powerful predictor of attitudes than is party vote. This indirectly reaffirms that for the bulk of the electorate, ethnicity has more to do with party choice than do attitudes (see Methodological Appendix).

For the ethnically nonconformist minority of voters (e.g., Afrikaans UP supporters), however, Table 15 shows that the behavioral divergence is associated with a corresponding *attitudinal* divergence from the ethnic mean. This finding is reinforced by a correlational analysis relating political attitudes to percentage of seats desired for each party. The expected correlations (i.e., positive correlations between liberalism and UP seats desired, negative correlations between liberalism and NP seats desired), while significant in almost all cases, are higher for the UP among Afrikaans respondents and for the NP among English respondents. This is because attitudes are more salient when Afrikaners consider the UP and when the English consider the Nats, since these are politically rather than culturally motivated preferences.[16] These correlations, for the two ethnic groups separately, are summarized in Table 16.

Table 15. Self-Attitude Scores by Ethnicity and Voting Intention

Intended Vote	Attitude Scores (Afrikaners)	Attitude Scores (English)
NP	3.0	3.2
	(82)	(30)
UP	3.4	3.9
	(24)	(95)
Prog		4.6
		(13)

Two-way analysis of variance
F (ethnicity) = 60.3, $p<.001$
F (intended vote) = 17.9, $p<.001$
One-way analysis of variance (within ethnic groups)
F (Afrikaners) = 5.3, $p<.05$
F (English) = 14.1, $p<.001$

Returning to Table 15, we learn from one-way analyses of variance that this phenomenon of ideologically based nonconventional voting operates more strongly in the English subgroup than in the Afrikaans. The attitude-party preferred correlations are also considerably more powerful for the English than the Afrikaners (except, as already noted, with the UP). This picture of the

[16] At first sight, this finding would seem to contradict recent characterizations of the "floating voter" as less informed and involved than the stable voter, and even to reaffirm traditional notions of vote changers as rational and discriminating. But as Converse points out, the "floating voter" hypothesis comes from the American *presidential* elections, during which all but a handful of isolated (and hence politically stable) individuals are blanketed with political news. In off-year congressional elections, which do not receive much coverage from the media, we can observe "the least involved being more likely to follow normal behavior patterns than the more involved who do manage to pick up some new information from the weaker flow" (Philip E. Converse, "Information Flow and the Stability of Partisan Attitudes," in Campbell et al., *Elections and the Political Order*, pp. 136–157). South Africa, with no television and a poorly informed electorate, and with its mass of stable voters choosing on the basis of tradition and religious instruction, would seem to fit the American congressional model.

Table 16. Correlations between Liberalism and Percentages of Seats Desired for Each Party by Ethnic Group

	Afrikaners	English
NP	−.23	−.27
UP	.13	.02
Prog	.25	.42
HNP	.08	−.28

Correlations in each row are between liberalism and the party listed in that row.

English as being more deliberate, purposeful voters is further supported by a curious finding about the party perceptions of Afrikaners. NP and UP voters do not differ within either ethnic group in the extent to which they perceive a difference between NP and UP attitudes. English supporters of both parties consistently see the UP as being more liberal than the NP. Afrikaners, however, consistently see *no* attitudinal difference between the two parties. Apparently Afrikaners who vote UP do not do so because they see that party as being more liberal than the Nats, despite the fact that Afrikaans UP voters (like English UP voters) are *themselves* more liberal than Afrikaners who support the Nats.

A related finding is that there is generally greater attitudinal correspondence in Nat voting than in UP voting. For the entire sample, the correlation between liberalism and Nat seats desired is −.38 ($p<.0001$), while the correlation between liberalism and UP seats desired is lower, though still significant, at .20 ($p<.001$). Thus the anomaly of Afrikaners deserting to the UP without distinguishing between its positions and those of the NP reflects not only the Afrikaners' relative inability to tell the difference between the parties, but the entire electorate's inability to discern what the UP stands for.

Responses to open-ended questions asking what respondents liked and disliked about the parties corroborated that those who cast ethnically deviant votes as well as those who changed their votes between 1966 and 1970—in both directions—were acting on the basis of their feelings about the Nats more than their feelings about the UP. Unfortunately, the numbers of respondents in these categories were small to begin with, and those who volunteered responses about parties and issues were so few that we cannot present these findings in statistical form. But they do at least suggest that the NP is a more salient reference object in the voting decision than the UP is, and that voters who desert the Nats are casting protest votes against the government rather than making any attributions whatever about the UP. All this helps explain why the UP does better in actual votes than in the party preferred measure. The Nats do stand for a more coherent set of beliefs and policies than does the UP, even if neither party's support is primarily ideological. And Afrikaans defectors from the governing party, while not necessarily expressing a real preference for the party for which they vote, do see themselves in some sense as moving in a liberal direction. The difference in the mean attitude scores of Afrikaans Nat and UP voters, however moderate, is a real one.

The abstainers in Gardens, whose self-reported party preference for the Nats was discussed in the previous section, are likewise more liberal than actual Nat voters in the district. This group, which is more English than Afrikaans, has a mean attitude score of 3.6, compared with 3.1 for Nat-preferring Nat voters in Gardens and 3.5 for Nat-preferring UP voters. (A *t*-test comparison between the first two groups is significant at the .05 level.) Among Gardens respondents who list the Nats as their preferred party, abstainers—both English and Afrikaans—are more liberal than UP voters, let alone actual Nat voters. This decisively disconfirms Lever's hypothesis that abstainers in 1970 were primarily Afrikaners wavering between the Nats and the more conservative HNP.

Where the attitude-party preference correlations break down altogether is with Afrikaner minor party support. The ideological sophistication of Prog supporters (mainly English) gives that party the highest overall correlation (.44, $p<.0001$), but among Afrikaners the attitudinal extremes of liberalism and conservatism do not find outlets, as they ideally might, in support for the Progs and HNP respectively. In the case of the Progs this is because the ethnic bias makes an allegiance to such a predominantly English (and perhaps Jewish) party psychologically impossible for an Afrikaner. The HNP (for which the Afrikaans attitude-preference correlation is not even significant, at .08) did not succeed in establishing a distinct ideological image. The poor showing of a party so attitudinally congenial to some substantial portion of the Afrikaans electorate testifies to the National Party's successful exploitation of its monopoly of salient communications channels (especially the church), as well as its emotional hold on its constituency.

SES and Attitudes. The complete lack of attitudinal correspondence among Afrikaners in Prog and HNP voting also stems from a lack of political knowledge among lower-class Afrikaners. Among Afrikaans respondents, mean estimates of the HNP's position become increasingly conservative as SES level increases (means are 3.2, 2.9, and

2.2, $p<.001$), while mean estimates of the Prog position become more liberal as SES level increases (means are 3.6, 3.9, and 4.7, $p<.001$). Low-SES Afrikaners do not really know what either of these parties stands for, judging from the fact that they see such a small difference between them. This is why they show a greater preference for the Progs, and less for the HNP, than would logically follow from their conservative beliefs. English respondents, too, see greater ideological differences among the various parties as SES increases.

Respondents' own attitudes also vary significantly with SES. Within both ethnic groups they are more liberal in the higher SES categories, although this is more conclusively true for the English. (For the English, means are 3.2, 3.7, and 4.3, $p<.001$. For the Afrikaners, means are 3.0, 3.0, and 3.4, $p<.01$.) This suggests that political evolution may be occurring at the attitudinal level as a function of social mobility. But this phenomenon has no immediate political significance, since these attitudes have no direct bearing on voting behavior, for which the predictive power of ethnicity is close to total.

Feelings Toward Racial and Ethnic Groups. Given that ethnicity predominates over purely political attitudes in determining party preference, what other attitudes are there which mediate between ethnicity and vote? Can we isolate *any* attitudes which (1) vary significantly by ethnic group, and by party support within each ethnic group, and (2) reveal the psychological mechanisms by which a person's ethnic identity leads to a particular political choice? In this regard, specific issue orientations tell us no more than overall ideological orientations do. The reasons given by respondents for liking or disliking the various parties are largely nonissue-oriented, although the racial issue is mentioned most prominently in about one-quarter of both the like and dislike responses for the Nats. While a large plurality (42 per cent) of respondents regard the racial question as "the most important issue facing South Africa today," this belief does not entail any recognizable voting pattern, and many people who switched to the Nats in this election did so *in spite of* their expressed disapproval of NP racial policies.

On the racial closeness questions,[27] there are no significant differences according to party vote, which lends support to the opinion of many observers that the UP's constituency is no more favorably disposed to the nonwhite races than is the party of *Apartheid*. In line with this (given the relationship between party vote and ethnicity), there are no significant differences in racial closeness by ethnicity. (There is, however, a significant difference by SES, with lower-SES respondents feeling more distant from Africans at the .001 level.) In the 1950s, Pettigrew[28] found a relationship between negative feelings toward nonwhites and authoritarianism in South Africa. He also found that more prejudiced people prefer the National Party (a finding no doubt mediated by the fact that Afrikaners are more prejudiced). But Orpen's[29] more recent work disputes the connection between prejudice and authoritarianism and concludes that racial feelings are politically irrelevant. Orpen argues that since prejudice is a norm of Afrikaans culture, it cannot be understood within that culture as a function of an individual psychological configuration such as authoritarianism. Our data support this interpretation, since we find racial attitudes even less closely connected with political attitudes for the Afrikaners than for the English. (Correlations between liberalism and positive feelings toward the Coloured, Indians, and Africans average .26 for the English, .10 for Afrikaners.) But regardless of ethnicity, attitudes toward the nonwhite races have no more bearing on electoral behavior than do political attitudes.

To find the attitudes that *are* politically salient, we must examine the way white South Africans look at *themselves*, as expressed in the ethnic closeness variable and in that of closeness to whites in general. Table 17 gives the mean closeness ratings which the two ethnic groups assign to themselves and to each other. (Ambiguous Afrikaners have been removed from this tabulation, and will be treated later.) Not only is the Afrikaans self-rating higher than the English self-rating (yielding a t significant at .001), but the Afrikaners hardly separate themselves affectively from the English the way the English do in return. In fact, Afrikaners say they feel closer to the English than the English say they feel toward themselves. These findings, at first sight astonishing, are plausibly explained by the different cultural-historical situations in which the two groups find themselves. English-speaking South Africans

[27] Responses to these questions for the overall sample show a clearcut pattern. Indians are the least liked racial group, followed by Africans and the relatively benign-seeming Coloureds. (Means are 2.7, 3.2, and 4.4 respectively, with a higher score indicating greater closeness.) A probable explanation for the more intense dislike of Indians than blacks is that Indians are economically and socially more challenging to the white population.

[28] Thomas F. Pettigrew, "Personality and Sociocultural Factors in Intergroup Attitudes: A Cross-National Comparison," *Journal of Conflict Resolution*, 2 (March, 1957), 29–42.

[29] Christopher Orpen, "Authoritarianism Within an 'Authoritarian' Culture" (doctoral dissertation, University of Cape Town, 1970), and Christopher Orpen, "The Effect of Cultural Factors on the Relationship Between Prejudice and Personality," *Journal of Psychology*, 78 (May, 1971), 73–79.

Table 17. Mean Feelings of Closeness to Ethnic Groups by Ethnicity

Rating Group	Group Being Rated	
	Afrikaans	English
Afrikaans (168)	9.0	8.6
English (193)	6.3	7.8

Based on a 0 to 10 scale, with higher numbers indicating greater closeness.

have an established worldwide culture to identify with, and are commonly described as smug. Afrikaners, seeing themselves as isolated and perpetually threatened, feel a stronger dedication to their own ethnic group together with a greater acceptance of fellow members of the white South African minority.

In general, but especially for the Afrikaner, there is an emphasis on racial self-preservation which minimizes any potential concern with the ethnic division.[30] This is reflected in the all-important dimension of felt closeness toward "whites" in South Africa. As the ethnic closeness responses would lead us to expect, Afrikaners report feeling closer to South African whites than do the English (8.2 vs. 7.6, $p<.01$). Since there is little difference between the two groups in feelings of closeness to the nonwhite races, Afrikaners also feel significantly closer to whites than nonwhites relative to the English (4.8 vs. 4.0, $p<.01$). Not surprisingly, the Afrikaans word *blanke* (white) connotes far more than its English equivalent, encompassing a notion of civilization that is seen as contrasting with black barbarism.

But the explanatory power of the closeness to "whites" variable extends beyond ethnic differentiation. Among Afrikaners (although not at all among the English), this is the social distance measure which best differentiates Nat and UP voters (8.5 vs. 7.0, $p<.005$). The feeling of solidarity with other South African whites thus emerges as a major cognitive-affective dimension of Afrikaans Nationalist support and a key to the identification of actual and potential defectors from the Nats. The importance of this finding is underscored by the fact that *English* as well as Afrikaans Nat voters feel closer to their own ethnic group than do supporters of the other parties. For Afrikaner ratings of other Afrikaners, means are 9.2 for Nat voters and 8.3 for UP and others ($p<.05$), while for English ratings of other English, means are 8.4 for Nat voters, 7.9 for UP voters, and 6.6 for Prog voters ($p<.05$). For both ethnic groups the feeling of closeness to one's co-ethnics is part of a larger ideological orientation emphasizing in-group solidarity and expressing itself politically as Nationalism. Ethnic and racial solidarity, then, are key links in the attitudinal chain which leads to Nationalist voting.

A measure of the perceptual distortion which goes with these attitudes is that white South Africans overestimate the number of whites and Coloureds in the country and seriously underestimate the number of Africans. Our respondents estimated that Africans comprise 44 per cent of the country's population (the actual figure is 69 per cent). This can be seen as a defense mechanism stemming from fear of the blacks, as well as an evasion of the moral implications of complicity in white minority rule. And it is the Nat supporters whose estimates are the most out of line. They give the highest estimates of both English and Afrikaner populations ($p<.05$); they also give the lowest for Africans ($p<.001$). Afrikaner and English percentage estimates for Africans exactly match those of Nat and UP supporters respectively.

Ambiguous Afrikaners give mean ethnic closeness ratings of 8.0 with Afrikaners and 8.3 with the English. Comparing these figures with Table 17, we find that they are intermediate between the high Afrikaans ratings and the low English ratings. In other words, ambiguous Afrikaners feel closer to *both* ethnic groups than the English do, thus retaining something of their old identity while asserting their new one more forcefully than do those who have always possessed it. As with their political preferences, which are also intermediate between those of the Afrikaners and English, this may be an aggregate phenomenon masking intra-group divergence. A striking finding which enables us to break down the aggregate, and also to suggest a social-psychological analysis of this enigmatic population segment, is the strong negative correlation between the ambiguous Afrikaner's degree of political liberalism and his feeling of closeness to both Afrikaners ($-.35$, $N=44$) and English ($-.22$). It seems that the ambiguous Afrikaners may comprise, at the extremes of the spectrum, two very differently motivated subgroups—those who adopt the English identity as a mark of enlightenment and liberalism, and those who do so from a feeling of status insecurity, which goes with heightened provincialism and racism. We were not, however,

[30] Hendrik W. van der Merwe and J. J. Buitendag, "Political, Ethnic and Structural Differences Among White South Africans," in *ASSA, Sociology Southern Africa 1973* (Durban: University of Natal), pp. 106–121, have observed a unifying trend within the white South African elite over the past decade, with the government de-emphasizing traditional English-Afrikaans differences in the interest of broadening the Afrikaner ethos into a South African national ethos.

easily able to identify these groups in terms of SES.

Summary and Discussion

This study shows that ethnicity is the major determinant of party vote in South Africa. Afrikaners vote for the National Party, English-speaking South Africans back the United Party, and the Progressive Party depends heavily upon Jewish support. Party vote also varies with other demographic categories (SES, religion, place of origin), but this is largely because these factors covary with ethnicity. When ethnicity is held constant, therefore, all these variables lose their predictive power.[31] In other words, while the American model of voting behavior, which stresses sociologically conditioned political predispositions, holds true for South Africa, there is nothing in that country comparable to the Lazarsfeld Index of Political Predispositions, which postulates three *separate* demographic determinants of electoral behavior. Following from the finding of a single powerful voting motivator (i.e., ethnicity), there is little evidence to support the "cross-pressures" hypothesis in South Africa, either with regard to actual voting patterns or information avoidance and other signs of withdrawal.

In a more fundamental way, however, South Africa corroborates hypotheses about electoral behavior which have grown out of American presidential elections, rather than hypotheses based on issue-oriented parliamentary systems, such as Norway's.[32] In South Africa, as in America, party labels are primarily symbolic and evocative, rather than standing for substantive policy disagreements. What they evoke is the different ethnic identities that make up the white electorate, as well as different degrees of attachment to the South African white minority's collective identity. Political attitudes in the ideological (liberal-conservative) sense do not differentiate well between party supporters, especially with ethnicity controlled. Feelings about Africans and other nonwhite races in the country are even less relevant. The attitudes which *do* relate significantly to voting behavior are (1) the feeling of closeness to whites in South Africa, which differentiates both between Afrikaners and English and between Nat and UP voters, and (2) the feeling of closeness to one's own ethnic group, which is greater for Nat voters than for UP voters *within* each ethnic group. These findings speak to the basic social-psychological question which Borre and Katz[33] have raised in their study of a Danish election—the question of what the act of voting means to the voter. In varying degrees, the South African citizen is voting for the white man, or for himself as a white man (and as an Afrikaner or an Englishman). Paradoxically, while an Englishman's vote for the UP is an expression of ethnic identification, those English who are most preoccupied with the issue of ethnic identity vote for the Nats.

Ethnic solidarity, racial solidarity, and Nationalist allegiance are mutually reinforcing notions which form the conceptual basis for political decisions within the Afrikaner ethos, and to the extent that they rub off on the English-speaking, within the South African national ethos as well. The government has used its control over salient communications channels to create a psychological identification between party loyalty and ethnic and national pride. And in a country where the electorate is generally poorly informed, one of the most important channels for the dissemination of political information to the Afrikaans majority is the Dutch Reformed Church. If American voters make up their minds more from ingrained habit and traditional symbolism than from current information received from the news media, the same is even truer of South Africans. This is why the right-wing HNP's challenge to the Nationalists, which generated great concern in the media and among Nationalist leaders themselves, failed so totally. A measure of its failure is that the voters to whom the HNP most sought to appeal—conservative, lower-class Afrikaners—were *least* able to distinguish the HNP's position from that of the Nats. The HNP's fate suggests that a mystical lower-class movement (as opposed to one based solely on pocketbook issues) must offer its followers a whole way of life, as Nazism and Peronism did, rather than appealing through ideological statement.

Since issue orientations and economic status are secondary to more basic questions of self-conception in determining party allegiance, it follows that theories of social mobility, such as Barber's,[34] do not as yet have much relevance to the South African political scene. Higher-SES respondents do have more liberal attitudes, even with ethnicity controlled, but these differences do not translate themselves into votes. Only an unimpressive minority of those Afrikaners who are exposed to more "English" environmental influences vote for the UP. In particular, two groups

[31] The major limitation of Lever's *The South African Voter* is his failure to apply statistical controls for ethnicity, and other factors. See the review of Lever's book by Stanley J. Morse in the *Journal of Modern African Studies*, 12 (1974), 141–143.

[32] For the Norwegian case, see Campbell and Valen, "Party Identification in Norway and the United States."

[33] Borre and Katz, "Party Identification and Its Motivational Base in a Multiparty System."

[34] Barber, *Social Mobility and Voting Behavior.*

which are being looked to as potential vanguards of electoral change—young voters, and newly emerging Afrikaner businessmen in the cities—do not yet show any significant movement in that direction (although there are minor trends for some groups of young people). A third group, however, discovered serendipitously in the course of this study, may turn out to be equally important as a focal point for change: these are the voters whom we have labeled "ambiguous Afrikaners," since they answered the social distance questions as though they were English even though they qualified as Afrikaans by our childhood language criterion.

Both in their attitudes and voting intentions, ambiguous Afrikaners take a position intermediate between the Afrikaner and English poles. This suggests that in a country where ethnic identification dictates a person's vote, a deliberate, self-proclaimed change of ethnic identity may bring about a change in political disposition even though a mere change in economic status does not. Barber's thesis that mobile individuals take political positions intermediate between those characteristic of their status of origin and their present status holds true for the ambiguous Afrikaners, if this group is seen as having moved from an "Afrikaans" to an "English" ethnic status. Thus the Barber hypothesis can be said to be applicable to South Africa, as long as "ethnic mobility" rather than socioeconomic mobility is under consideration.

The presence in such large numbers of ambiguous Afrikaners in our sample calls into question once again the typicality of an election study conducted in the Cape Province of South Africa. The Cape was primarily an English settlement in the nineteenth century, and is now a notably liberal region of the country. It might be thought, therefore, that Afrikaners in the Cape would feel much more tempted (and pressured) to assume an English identity than would Afrikaners elsewhere. However, a more common setting for Afrikaner conversions has probably been the large industrial areas around the Witwatersrand in the predominantly Afrikaner Transvaal Province, where some Afrikaners have taken up a different life-style after moving to the English- and Jewish-dominated commercial center of Johannesburg. As a result, we have no reason to believe that our findings are atypical in this and other respects. The agreement of our data with Lever's[36] Johannesburg data—as far as he takes his analysis—is another sign of their generality.

This survey was conducted just before the Parliamentary election of 1970, in which the Nats suffered minor, though distinct, losses. Analyzing the results, Lever finds a return to the UP on the part of English voters who deserted to the Nats in the 1966 election, when the Nats achieved their greatest majority, along with a shift from a pattern of English abstentions in 1966 to Afrikaans abstentions in 1970. From this he infers that the hope of capitalizing on a growing Nat schism (embodied by the HNP defection) brought English voters back to the polls, and back to the UP, after their having come by 1966 to despair over the prospect of outvoting the Afrikaner majority. At the same time, the appeal of the HNP (along with a series of blunders by government ministers) created an emotional conflict in Afrikaners which led to increased abstentions on their part. Our data do not permit us to evaluate this thesis fully, since we do not have a companion study of the 1966 election (as Lever does), and since we have too few respondents indicating a change of party since 1966 to draw any reliable conclusions about them. But we do find a decisive pattern of abstentions by those who favor the Nats in our swing district (Gardens), and our data confirm Lever's claim that shifting voters were mainly English.

Where we differ from Lever is in interpreting the motivation of Nat-supporting abstainers in 1970, who are also mainly English and, being more liberal than the Nat voters in their district, are unlikely to have been attracted to the HNP. In addition, our study population is considerably more liberal than it perceives the NP to be. It thus seems reasonable to believe that most discontent with the government is not reactionary, and that there is a substantial constituency in South Africa which would support liberal change. Nonetheless, while Afrikaans UP voters are ideologically more liberal than Afrikaans Nat voters, the moderate mean attitudinal divergence between the two groups implies that only some of those with more liberal views actually dissent at the polls. Political loyalty is one of the most deeply entrenched features of cultural identification in South Africa, and one of the last to be discarded.

Another factor impeding the electoral expression of deviant attitudes is that the political parties from which the voter must choose do not offer a range of ideological alternatives. While ethnically nonconforming voters do vote in a direction consistent with their beliefs, these votes cannot really be rationalized on such grounds, given the alignment of the parties. To vote Prog is psychologically impossible for an Afrikaner, while the UP does not have, and is not seen (by Afrikaners, even those voting for the UP) as having, an ideological position distinct from that of the Nats. This is why so much liberal Afrikaans dissent takes the form of abstention or mere protest votes for the UP. A vote for the UP does not imply a

[36] Lever, *The South African Voter*.

real preference for UP control of Parliament as much as a vote for the NP implies a preference for NP control of Parliament. On the other hand, a preference for the NP does not result as predictably in a vote for the NP as a preference for the UP results in a vote for that party. As the governing party, the NP stands for a more coherent set of policies to which the voters can react, while votes for the UP stem from a more diffuse set of political motivations.

Under these conditions, if there is to be a breaking down of the Nationalist ideological consensus, it probably will not be expressed through electoral change within the present party structure. Rather, political change in South Africa must come from movements arising within the Afrikaner Nat majority, possibly within the NP itself. Developments since the 1970 election are in line with this projection from our data. The national-cultural ideology of the NP has all along masked a great diversity of opinion within party ranks, and now this diversity is being more openly expressed, with an increasingly apparent split between the *verligte* (outward-looking, progressive) and *verkrampte* (inward-looking, reactionary) wings of the party.

Meanwhile, recognizing the difficulty in overcoming decades of NP tradition, some *verligte* Afrikaners have been joining liberal English-speaking South Africans in their attempt to take control of the UP as a vehicle for reform, while other Afrikaners are working through a variety of nonpartisan organizations such as the university-based "*Verligte* Action." There is even a new political party, the Democratic party, founded by Theo Gerdener, formerly a liberal NP leader. Most of these efforts have not yielded substantial results. Gerdener's party faces the seemingly insoluble problem of how to draw Afrikaans voters away from the NP, while the agitation within the UP has only exacerbated that party's internal confusion. Policy shifts alone will not change overnight the fact that the UP stands for a cultural and political tradition alien to the Afrikaans majority. And there seems little likelihood of changing the National Party's policies as long as its leaders can count on a substantial constituency which approves of those policies and a larger constituency which supports the party regardless of policy.

Since electoral change is so unlikely, the nonpartisan approach to substantive reform seems most promising, especially when it is carried out through labor and business organizations which have a real place and function in the society. Labor unions, once co-opted by the Nats, have been taking a more independent line, in some cases accepting the unionization of black workers. And business leaders are becoming more insistent about the need to consider worldwide economic and political trends. But even these groups have little direct political power, except insofar as they can bring about changes in public consciousness. For the comfortably well-off National Party, the crucial issue is, perhaps, whether it should strive to expand its majority by recruiting the English-speaking with a more broadly nationalistic appeal, or whether it should emphasize Afrikaans exclusiveness in order to prevent an accelerating erosion of the ethnic consciousness which maintains its current majority.

Postscript

Since the completion of this study, another general election was held in South Africa, in April of this year. With 5 new seats at stake, the NP and the Progs each gained 5 seats while the UP lost 5 seats. This left the new Parliamentary lineup at 123 seats for the NP, 42 for the UP, and 6 for the Progs. In this election, the Nats were seemingly able to reassert their hold on the Afrikaans electorate, in part because of ideological dissension within the UP. But the Prog success was unprecedented, especially when coupled with the capture of 5 seats by a dissident liberal UP faction in the Transvaal province (all around Johannesburg). In addition, the Democratic Party made a good initial showing, with Gerdener coming within 400 votes of beating his UP opponent in Pietermaritzburg. Although all the districts won by the Progs were in relatively well-off English-speaking suburbs of the country's two major cities, the party improved its poll in all four provinces, thus approaching for the first time the status of a national party. The election results support our finding of growing liberal discontent with the two leading parties, and raise the possibility of a liberal coalition embracing the Progs, the left-wing UP bloc, and the Democratic Afrikaners. But for such a coalition to challenge the Nats for national leadership—a prospect which is still far in the future—it will have to give a leading role to the *verligte* Afrikaners whom the Democratic Party is only beginning to mobilize.

Methodological Appendix

A. Statistics

1. *Analysis of variance* is used in this paper in a number of ways, including some where its results can only be considered of heuristic value. For example, on questions comparing the amount of information respondents obtained from one source versus another, within-subject tests would normally be called for. But since we were interested in *patterns* of preference for certain information sources over other sources among groups of respondents, analysis of variance served as a convenient indicator of contrasting trends in the different groups (see also Table 1).

F ratios can be helpful indicators of relative strengths of relationship in cases when this is not a strictly legitimate deduction from a multivariate analysis of variance. For example, in Table 15, the deduction is made that ethnicity is more important than are political attitudes in determining voting, although the dependent variable is actually attitude, while vote is an independent factor. Still, the overwhelming predictive value of ethnicity in the analysis clearly makes it the dominant element in this configuration of variables.

Where analysis of variance is used despite the fact that subjects are not randomly assigned to different categories, analysis proceeds on the basis of evaluating possible explanations for significant variations other than the explanations provided by the predictor factor(s). Incorporating such plausible artifacts as controls, we could more safely accept our original findings as valid when they remained significant through further analysis.

2. *Nominal statistics* used in this paper include the contingency coefficient and lambda, along with chi-square. Lambda tests do not appear in the tables run with controls. Because of the power of ethnicity as a predictor in tables run with ethnicity controls, the majority of responses in all categories of predictor variables fell under the party appropriate to the language group being examined (UP for English, NP for Afrikaners), and thus none of the categories could distinguish between voting responses.

These statistics are appropriate for nominal data, yet we frequently use them where one of the variables is ordinal, or even interval. When the data are ordinal, there is an added benefit in that explanations of trends that occur along distinct directions in the order become much more meaningful and understandable, even though only nominal-level statistics are being employed.

B. Attitude Measures

1. *The items* were as follows (those items followed by R were reversed in coding):

1. The border industries—the industries built near the Bantu reserves—should be developed at a much faster rate.
2. The Blacks and Whites should be encouraged to mix socially.
3. The pass laws should be abolished.
4. A great deal of money should be spent to develop the Bantu homelands.
5. The Bantu should be given complete independence in their homelands.
6. Non-whites should represent their own people in Parliament.
7. People of all races should be considered South African citizens.
8. The Immorality Act should be abolished.
9. Non-whites should be represented in Parliament, but they should be represented by Whites only.
10. Blacks should only be allowed in White areas to work. R
11. Whites should not be allowed any rights in the Black homelands. R
12. No inter-racial sports t countries should be admi Republic, unless we have relations with the country.
13. Blacks should not be all rights in White, urban area
14. In order to protect the cou State Security should be giv
15. English-speaking South A encouraged to adopt the ' life." R

2. *The scale* was based on an liberal-conservative dimension politics. Thus, two items were scale because of their ambigu ment 9 patently cuts both way is also equivocal since the bo vide employment for African policy of *Apartheid.*

Intercorrelating the remainir of which figured in the average find that two others should ha well—Statements 5 and 11. W statements emphasizing Afric the black homelands would be but the lack of any consistent t tions involving these statem people were responding as wel of *Apartheid* they contained. T the correlations for the remai the attitude scale does show sc ity. Only four of these 55 corre only one of which exceeds .05 . of the 51 positive correlatic though not overwhelming. F .05 significance level with an most of these being significant only three correlations exceed exceeding .30. In general, re attitude statement are in line the others, but do not come n variance in the other responses conservative dimension in Sou but it does not totally explai the issues.

3. *Factor analysis* was perfc uncover the salient cognitive lying responses to the attitude components varimax rotatio Kaiser's criterion determining tors to be extracted. Since in S have shown, the crucial atti occur along ethnic lines, we ex structure for each ethnic grou as well as for the overall sampl

Accepting factor loadings o for including an item in a facto yielded essentially the same f the Afrikaans factor structure than the other two, a limit of be set in that analysis and th reduced to .475.) These factors subsume (over all three analy

ization research that the subjects are too young to participate in the political process, and that by the time they come of age the attitudes and values they learned as children may have completely changed.[20] Another reason for choosing recent high school graduates is that these new adults are soon to leave the family permanently, and further family socialization will be minimal.

The data were collected by means of personal interview in Iowa City, Iowa (population 47,000), between June 1 and September 1, 1972.[21] The sampling universe consisted of all graduating seniors from the city's largest high school (who gave Iowa City addresses) and their parents. Eighty per cent of the adolescents and at least one parent in each case were successfully interviewed.[22] The sample, quite obviously, is not randomly drawn from a finite population. But this is of limited importance since the purpose here is not in describing population parameters, but in developing "conditional universal propositions." When the purpose of research is testing conditional universals, a random sample is not necessarily more useful than a nonrandom sample.[23] A defensible and financially more practical procedure for building cumulative knowledge involves testing hypothesized relationships under a variety of conditions. In this paper, hypotheses will be tested using a basically white, urban, middle-class sample. Demonstration of the generalizability of the findings entails replication using, for example, a rural sample, a black sample, or possibly a non-American sample. The hypotheses can, of course, never be confirmed; they can only be disproved.

Measurement

Attitudes were measured by responses to closed-ended, Likert-type items in a personal interview situation. The questionnaires were designed to secure information about the young adult, his parents, his best friend, and his perception of certain attitudes of his parents and best friend. This information consists basically of party identification, attitudes toward three issues, evaluation of eight public figures, and a series of questions relating to adolescent/parent/best friend interactions. This paper will be limited to an analysis of parents and adolescents with respect to party identification, the three issues, and certain interaction variables.

Selection of the issues and their indicators was made only after numerous pretests, first on college students, then on college students and their parents, and finally on a small sample of high school seniors and their parents. The issues finally chosen were: (1) racial integration of the public schools, (2) United States policy toward Communist China, and (3) laws concerning the use of marijuana (these questions are reproduced in the appendix). The attitude toward each issue was measured by a scale consisting of four opinion items, which were scattered throughout the measuring instrument to minimize any tendency to respond to the items in a blanket fashion. Control of response set was effected by wording some of the items negatively and some positively.

The reliability and validity of the scales derived from these questions is, of course, crucial. Fortunately, these estimates can be made with some confidence. Since each scale is used three times (once for the adolescent, once for the adolescent's perception of the parent's attitude, and once for the actual parent attitude), there are three opportunities to check on scale properties.

Reliability (defined as internal consistency) was estimated using Cronbach's alpha.[24] Nine coefficients were computed. All were above .70, which is satisfactory. As for validity, a good argument can be made for the *content* validity of the scales used here. They are composed of specific opinion items measuring attitudes toward concrete relatively well-defined concepts (issues). More important is the assessment of *construct* validity—the extent to which relationships between items are isomorphic with the relationships predicted by the nominal definition of the concept. This form of validity can be estimated by factor analysis, which will indicate those items measuring the same concept, and how successful they are in this measurement.[25]

[20] See David March, "Political Socialization: The Implicit Assumptions Questioned," *British Journal of Political Science*, 1 (October, 1971), 460–464.

[21] Interviewers consisted of eight male political science and sociology graduate students between 25 and 32 years old.

[22] The sample consists of 183 adolescents (54 per cent male, 46 per cent female) and 322 parents (45 per cent male, 55 per cent female). Data on both parents were obtained for 139 of the adolescents. Twenty-six adolescents came from one-parent homes. In the case of the remaining eighteen adolescents, the second parent was not home at the time of the scheduled interview or call-backs.

I shall not, in this report, distinguish between mothers and fathers. Therefore, to make maximum use of the sample size each adolescent is paired with both the mother and the father (when both are present). This procedure renders tests of statistical significance based on F-ratio's inappropriate. See footnote 43 for a further discussion.

[23] David Willer, *Scientific Sociology* (Englewood Cliffs, N.J.: Prentice-Hall, 1967), chapter 6.

[24] George W. Bohrnstedt, "Reliability and Validity Assessment in Attitude Measurement," in *Attitude Measurement*, ed. Gene Summers (Chicago: Rand-McNally, 1970), pp. 87–88.

[25] Fred N. Kerlinger, *Foundations of Behavioral Research* (New York: Holt, Rinehart and Winston, 1964), pp. 453–455.

Table 1. Product-Moment Correlations between Parents and Adolescents on Three Issues and Party Identification

Attitude Objects	Correlation	Means[a]	
		Parents	Adolescents
Racial Integration	.32	8.8	7.4
China Policy	.27	6.4	5.4
Marijuana Laws	.40	9.2	6.9
Party Identification	.48	4.4	3.9

[a] For purposes of issue description, a scale ranging from 1–17 was computed by the additive method. The higher the scale value, the more conservative the respondent. For party identification (measured by the standard SRC seven-point scale), the higher scale values indicate Republican preference.

All twelve opinion items were factored together, once for the adolescents, once for the adolescent's perception of the parent's attitudes, and once for the actual parent attitudes. It was hypothesized that three distinct factors would emerge, with all the racial integration items loading on one factor, all the China policy items on another factor, and all the marijuana items on a third factor. The results supported the hypothesis. The analysis revealed an extremely clean factor structure, with only one of the predicted thirty-six loadings (four items, three factors, three administrations) falling below .50.[26] The actual scale values for each individual respondent were computed with factor scores.[27]

Bivariate and Within-Category Relationships

Bivariate Relationships

The product-moment correlations between parent and adolescent attitudes, presented in Table 1, are as high as or higher than one might expect on the basis of recent literature. The correlations for party identification and racial integration are comparable to those found by Jennings and Niemi (.59 and .34, respectively).[28] These authors also report a number of very low-correlations, but for attitude objects which lacked immediacy at the time of the interview (1965). When immediacy is comparable, the relationships reported here and those reported by Jennings and Niemi are similar.

In terms of group correspondence, the adolescents are in all cases more liberal than the parents but the differences indicate no great generation gap. The standard deviations (data not presented) on each issue for the two groups are almost identical. Absolute congruence between parents and adolescents can best be described as moderately high. This information was computed by subtracting the adolescent's scale score on each issue (computed by the additive method) from the complementary parent's score. These difference scores had a possible range of ± 16, with zero indicating perfect agreement. The means of these difference scores for the race issue, China policy, and the marijuana issue were 1.3, 1.0 and 2.2, respectively.[29] Given this information, the correlation coefficients can be interpreted as indicating a functional relationship between attitudes that are reasonably similar in an absolute sense. There exists no large gap between parent and adolescent attitudes in either group or congruence terms.

Conditions for Parental Influence (Within-Category Relationships)

Having established the magnitude of the parent-adolescent attitude correlations, I shall now attempt to specify the conditions which are conducive to successful transmission. There are two reasons for this analysis. First, it is consequential for determining whether parent-adolescent attitude correlations are due to parental influence or due to other social forces.[30] If attitude correlations between parents and adolescents are spurious, it should make no difference for the magnitude of coefficients if certain specification variables are manipulated. Beyond demonstrating or failing to demonstrate direct parental influence, the analysis is designed to uncover conditional universal propositions concerning parent-child attitude transmission.

[26] The common factor model was used with communalities estimated by squared multiple correlation coefficients. The rotation was oblique. The item falling below .50 was the first racial integration item (see appendix) which loaded at .38.

[27] The factor scores were estimated by multiple regression following the procedures outlined in Harry Harmon, *Modern Factor Analysis* (Chicago: University of Chicago Press, 1968), pp. 350–354.

[28] Jennings and Niemi use the product-moment coefficient for party identification, and tau-*b* for the issues.

[29] Positive scores mean adolescents are more liberal than parents, negative scores mean the reverse. The distribution of these scores approximates a normal curve. The standard deviations for the race issue, China policy and marijuana laws are 4.1 3.7, and 4.3, respectively.

[30] The most plausible hypothesis, alternative to direct parental influence, is that parents and children have similar attitudes because each individual family member is exposed to the same social influences. The example used earlier was social class. Variables of this type may determine the position of the parent and child within their respective groups thus producing a spurious correlation. One test of the spurious relationship hypothesis is controlling for a third variable. Controls were introduced for education, social class, and religion; at no point, for any issue or party identification, was the bivariate correlation reduced by more than .02.

For Hobbes merely to have restated the well known royalist arguments would have meant that he would be speaking only to the already convinced; he would have had no chance to persuade the opposition since it had long before rejected the royalist arguments. Hobbes could have written as an orthodox royalist only at the price of being condemned to ineffectiveness, as just another sectarian, ideological propagandist for the king. In other words it is precisely because Hobbes was a great theorist that he rejected the conventional royalist arguments.

The royalist world-view by the middle of the 17th century not only had been rejected by the king's opponents, but had become quite questionable, if not untenable, from any perspective; it could no longer provide a sound basis for political argumentation. This world-view, which has been appropriately termed the "philosophy of order," envisioned society and indeed the universe at large as a hierarchically ordered structure based God's reason. Looked at vertically the universe was a great chain of being, beginning with the noblest and descending to the meanest things of creation; horizontally it consisted of a number of planes of existence, arranged one above another in order of dignity, but unified by the fact that they "corresponded" in their internally hierarchical structure. In each plane there was a single "primacy" which was supreme over all the other members of that level: the eagle among birds, the lion among animals, gold among the elements, God among the angels, the soul in the human body, etc. Within this world-view the basic form of reasoning was by "correspondencies" or analogies. Such reasoning consisted of reviewing the similarities between things, taking a known fact or situation in one plane and inferring that a corresponding fact or situation existed in another plane. For example because God was sovereign in the universe, and the soul sovereign in the body, therefore there had to be a similar unified and absolute power in the body politic.[15] This style of argument, resting on the philosophy of order, was used by royalists to justify absolute monarchy.

James I, for example, elaborated on analogies between a king in his kingdom and God in the universe, a father in his family, and the head of the human body, in order to support his claim of absolute power.[16] This was not a novel or an outlandish argument, for it had been used frequently during the previous century and was still being used by supporters of the king during the civil war.[17] Even after the defeated king was the captive of the parliamentary forces one of his supporters denied the right of parliament to hold the king, saying that "as in Bodies Natural, so in Politick, King Charles is the essential head of our Politique Body," and so above all other men.[18] While such arguments may appear unconvincing or ridiculous, they were, in fact, considered quite reasonable and forceful within the philosophy of order.

The philosophy of order fell prey to attack from every direction. First the Reformation upset the ordered picture of the universe, replacing God's reason with his will, and offering an egalitarian view of man which clashed with social and political hierarchies. Second the new physics and astronomy, in establishing the heliocentric view of the universe, made untenable the older picture which had provided the basis for argument by correspondence. The hierarchical view of the political order was also being threatened by contemporary social change, for the new social forces and patterns of behavior broke up the rigidly hierarchical social structure, which had "corresponded" to and confirmed the hierarchy of the political order.[19] The emerging commercial class held attitudes and goals which were particularly disruptive of the royalist view of the political order. Thus the combined impact of the Reformation, the revolution in science, and the developing commercialism on the philosophy of order was such that the royalist was left in a much weakened, vulnerable condition.

The royalist historical arguments, which supplemented their philosophical ones, were also being attacked; parliamentary historians offered different versions of both English and ancient history, which supported their political position. While Tudor historians had been able to rely on the legends of King Arthur and a Trojan geneology to support the monarchy, competing historians discovered the "Ancient Constitution" in England's past, which supported Parliament's claims to power.[20] While Parliament's case was never established beyond question, the royalist version was discredited to the point that it was increasingly less convincing as a powerful defense of the monarchy. Thus we can see that Hobbes would have stood on shaky ground had he chosen

[15] See W. H. Greenleaf, *Order, Empiricism, and Politics* (London: Oxford University Press, 1964), pp. 47–53; E. M. W. Tillyard, *The Elizabethan World Picture* (New York: Vintage Books, n.d.).

[16] Francis D. Wormuth, *The Royal Prerogative* (Ithaca: Cornell University Press, 1939), p. 7; Greenleaf, pp. 61–63.

[17] E.g., Robert Grose, *Royalty and Loyalty* (London, 1647).

[18] Quoted in Greenleaf, p. 49.

[19] Greenleaf, pp. 143–146.

[20] *Ibid.*, p. 114–124, 179–187; cf. J. G. A. Pocock, *The Ancient Constitution and The Feudal Law* (Cambridge: Cambridge University Press, 1957); Quentin Skinner, "History and Ideology in The English Revolution," *The Historical Journal*, vol. 8, no. 2 (June, 1965), 151–178.

to defend the king in the conventional manner. Hobbes himself acknowledged the weakness of the royalist case in *De Cive*, where, after referring to several royalist arguments, he dismissed them, saying, "although, I say, these do hold forth monarchy as the more eminent to us, yet because they do it by example and testimonies, and not by solid reason, we will pass them over."[21]

In addition to the anachronistic qualities of the royalist world-view, Hobbes found their specific political arguments to be inadequate to the defense of the king; in fact their arguments virtually justified the Puritan opposition to the king, while weakening the resolve of his defenders. Asking himself "whether the doctrine taught by those divines which adhered to the king, be such . . . as may justify the Presbyterians that incited the people to rebellion," Hobbes answers that such preachers "would go near to be found not guilty" by that doctrine.[22] The royalist doctrine argued that when a king commands something contrary to God's will, his subjects ought to obey God, and give the king only passive obedience. This doctrine encourages rebellion, according to Hobbes, since rebels will always be able to claim that they acted in obedience to God. The notion of passive obedience is itself unworkable since it requires that a subject resist his ungodly king and yet not try to avoid being punished for his resistance—a virtual impossibility according to Hobbes: "Do not we see that all men, when they are led to execution, are both bound and guarded, and would break loose if they could, and get away? Such is their passive obedience."[23] By justifying an impossible passive obedience, the royalist doctrine actually undermined the foundations of the argument against rebellion.

By the time of the civil war, the royalist confusion or misunderstanding of its own position had contributed further to the erosion of the king's authority. The king's advisors shared the "fault, which was generally in the whole nation, which was, that they thought the government of England was not an absolute, but a mixed monarchy; and that if the king should clearly subdue this parliament that his power would be what he pleased, and theirs as little as he pleased: which they counted tyranny." By 1642 even the king himself was defending only a mixed monarchy and rejecting absolute monarchy as tyrannical.[24] Thus Hobbes found himself in the position of having to be, in effect, a better royalist than the established royalists, for their position was both philosophically and historically questionable, and it offered the king only a weak and limited protection.

Hobbes's Multiple Audiences

The problem faced by Hobbes was how to defend the king's position while appealing to, and persuading, a heterogenous opposition. His situation demanded that he go beyond the established patterns of argument (in which each side was speaking past the other) and speak to the various groups on their own terms, invoking, if possible, their own accepted authorities and styles of argument, thus disarming or redirecting their viewpoints to support rather than to attack, the political order. In this situation we would expect that Hobbes would attempt to assimilate the viewpoints or styles of thought of all relevant groups, so that his argument would appear familiar and congenial, and thus forceful, to all sides. There were, as indicated above, three points of view that had done most to undermine the royalist position, and these therefore would be the natural objects of Hobbes's persuasion: the new scientific mentality, the emerging commercial class, and the Puritan dissidents (who were the most serious and immediate political threat, from Hobbes's perspective).[25] Thus we should be able to find in Hobbes arguments or persuasive devices aimed at all three groups.

It is truistic that Hobbes presents his political argument in a thoroughly scientific form. Although scholars disagree about whether there is a logical connection between his scientific propositions and his political theory, no one disputes that Hobbes presents his case in the most scientific terms.[26] And it is equally truistic that there are aspects of Hobbes's doctrine which betray a distinctly bourgeois orientation. Without accepting Macpherson's analysis in its entirety, one can scarcely deny that much in Hobbes's picture of a rightly ordered political system would appeal to a commercial mentality: not only does the Leviathan allow great scope for economic activity, but self-interest as a rationale of individual behavior is relied upon frequently.[27] The scientific and

[21] *De Cive*, p. 116, and also 66; *Leviathan*, pp. 111, 463.

[22] *Behemoth*, ed. Ferdinand Toennies, second edition (London: Cass, 1969) p. 47.

[23] *Ibid.*, p. 50.

[24] *Ibid.*, p. 114; Leviathan, p. 119; Donald W. Hanson, *From Kingdom to Commonwealth* (Cambridge: Harvard University Press, 1970) pp. 248–249; for a discussion of Hobbes's criticism of moderate royalists, see Irene Coltman, pp. 135–192.

[25] *Behemoth*, p. 47.

[26] Leo Strauss, *The Political Philosophy of Hobbes* (Chicago: University of Chicago Press, 1963) argues that the basis of Hobbes's view of human life is nonscientific. R. W. N. Watkins, *Hobbes's System of Ideas* (New York: Barnes and Noble, 1968) argues for the close connection between Hobbes's scientific and political ideas. Strauss, however, says that Hobbes used the language of science to justify his view as the only true and universally valid one.

[27] C. B. Macpherson, *The Political Theory of Possessive Individualism* (Oxford: Oxford University Press,

bourgeois aspects of Hobbes's theory are sufficiently established so that we need not discuss them any further. What has received much less attention, however, is the question of Hobbes's appeal to his third major audience—the Puritans.

The Puritans must have presented an especially difficult case for Hobbes since they were the most politically active group among the royalist opponents.[28] Hobbes's situation required, first, that he persuade them to reject their own political ideas, and subsequently, that he convince them to accept his. Such a change would involve not only a complete reversal of their theoretical position, but also a sacrifice of the actual political standing and power of their leadership (Presbyterian preachers). Effecting such a complete reversal would obviously be difficult under the best conditions, but with respect to political issues, it would be virtually impossible since, in Hobbes's view, most people reason very poorly about politics. The biases and interests of men outweigh their reason, when they consider political ideas.[29] Moreover, according to Hobbes, the Puritans, of all the groups on the political scene, were probably the least given to reason for their religious doctrine emphasized nonrational faith, and downgraded the role of human reason. In fact Hobbes considered the Puritans' belief that they were directly "inspired" by God to be evidence of their "madness."[30] While Hobbes did present rational arguments against both Puritan religious and political doctrines, he could have had little confidence that, by themselves, they would be effective. After all, Hooker many years, earlier had rationally devastated the doctrine of Puritanism, without having any apparent impact upon the Puritans.[31] Thus the only course left open to Hobbes, in appealing to the Puritans, would be to supplement his logical arguments with a mode of persuasion more congenial to the religious mentality.

In dealing with the Puritans it would seem reasonable for Hobbes to have followed his same basic mode of persuasion, that of trying to co-opt the opposition by speaking to them in their own terms, appropriating, if possible, some of their own notions.[32] For such a strategy to succeed, it would have to be applied subtly and indirectly, since any explicit use of Puritan authorities would be likely to have evoked a hostile response. Hence his optimal strategy would be to appropriate not so much their actual language or formal terminology, but rather some of their basic ideas. Moreover explicit use of Puritan language would be detrimental to Hobbes's appeal to his other audiences. This strategy would best apply directly to their religious beliefs and not to their derivative political beliefs.

While this approach may appear to conflict with Hobbes's geometrical-scientific method, in fact, it is a necessary supplement to it. Hobbes's scientific method was seriously limited in its capacity to communicate his doctrine, for, according to Hobbes very few people understood science. Though "the most part of man . . . have the use of reasoning a little way . . . as for *science* . . . they are so far from it, that they know not what it is." Hobbes's use of scripture to support his position is an obvious example of his not limiting himself to his scientific method. He clearly recognized that the acceptance of his doctrine rested, not just on its scientific rigor, but also upon the reader's finding it true to himself. Thus Hobbes openly stated that the only "demonstration" of his doctrine lay in the reader's introspection.[33] Clearly an argument dependent upon introspection for its persuasive force is not strictly scientific in character. In any case, with regard to the Puritans, Hobbes would have had to go be-

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1962); Harold Laski, *The Rise of Liberalism* (New York: Harper and Brothers, 1936), pp. 91–180; Leo Strauss, p. 121; Christopher Hill, *Puritanism and Revolution* (New York: Schoken, 1964), pp. 275–291.

[28] *Behemoth*, pp. 57, 63, 82, 89–90. Clearly it would be easier to persuade those whose ideas were merely disruptive of the established order than those who had deliberately rebelled and developed a justifying political doctrine.

[29] *Leviathan*, pp. 29, 67–68. This problem does not confront Hobbes as severely in dealing with the other two groups since the language of science and self-interest can make its appeal on nonpolitical levels.

[30] *Leviathan*, p. 47; *Behemoth*, p. 24; *E. W.*, IV, 418–419.

[31] Richard Hooker, *The Laws of Ecclesiastical Polity*, preface. From Hobbes's perspective, Hooker's reliance on Aristotle and the philosophy of order was a major defect in his political theory, since it prevented him from affecting his Puritan audience which held different metaphysical assumptions. That is, Hooker would have presented Hobbes with an excellent example of intelligent yet ineffective political theorizing.

[32] Perhaps the outstanding example of Hobbes's co-opting the opposition is his use of the idea of representative government. The doctrine of representative government, popular with many opposition groups, was, of course, rejected by Hobbes: "What bloodshed hath not this erroneous doctrine caused, that kings are not superiors to, but administrators for the Multitude?" (*De Cive*, p. 9). Yet Hobbes took the concept and interpreted it in such a manner as to make it politically innocuous: the representative (sovereign) had total freedom of action, while the represented (the subjects) had all the obligations. Thus Hobbes was able to turn a politically dangerous opposition doctrine into a defense of absolute sovereignty. See Hanna Pitkin, *The Concept of Representation* (Berkeley: University of California Press, 1967), pp. 14–37. Similarly Hobbs co-opted the idea of government based on consent. But in his version, even a government based on conquest, in which the defeated subjects submit out of fear for their lives, is interpreted as an example of voluntary consent. *Leviathan*, pp. 129–133.

[33] *Leviathan*, pp. 29, 6.

yond a purely scientific method since the Puritans were not reasoning people.

In appealing to Puritan beliefs, Hobbes would be following the advice of Aristotle, who in his *Rhetoric* (a work Hobbes much admired and twice translated)[34] suggested that "before some audiences not even the possession of the exactist knowledge will make it easy for what we say to produce conviction. For argument based on knowledge implies instruction, and there are people whom one cannot instruct. Here, then, we must use, as our modes of persuasion and argument, notions possessed by everybody." Clearly, in Hobbes's view, the Puritans were people who could not be instructed about politics, and so could only be approached by the use of notions "generally admitted or believed" by them.[35] Thus if Hobbes did adopt this mode of persuasion with his Puritan audience, we should be able to find in his argument notions or ideas which the Puritans already believed or were predisposed to accept; there should be some similarities between Hobbes's argument, and that with which the Puritans were familiar.

In developing his argument, Hobbes would not have had to concern himself with every Puritan sect, but only with those Puritans who were the most politically significant, i.e., in Hobbes's analysis, the Presbyterians. Since it was the Presbyterians who both incited the people to rebel, and gave rise to the other rebellious, but less important, sects, it would be to the Presbyterians that Hobbes would have to address himself.[36] Of all the English Puritans, the Presbyterians were the most strictly and thoroughly Calvinistic; many had resided in Calvin's Geneva during the period of the Marian exile, and returned imbued with Calvin's doctrine. Hence it was in Calvinism that Hobbes would have to find ideas and arguments on which to base his persuasive case.[37] Thus we should be able to find Hobbes using certain notions "generally admitted or believed" by Calvinists.

Hobbes and Calvin

In the following section we will show that Hobbes appropriated much of the Calvinistic doctrine and constructed an argument that so closely paralleled Calvin's that Presbyterians might be expected to feel quite at home with it.[38] Basically Hobbes constructed a secularized, rationalized version of Calvin's religious argument. Hobbes begins with a description of man's nature that is virtually identical to Calvin's view of man; in both doctrines man exists in a difficult, hostile, insecure condition, continually threatened by death, out of which he can escape only by covenanting into a community ruled by an all-powerful sovereign whose word is law. In other words Hobbes follows Calvin in presenting man in very similar terms, and in offering him the same two alternatives: a life of insecurity, conflict, and death, or a life of security and order in a community, subject to the will of an absolute sovereign ruler.[39]

The most prominent similarity, and the one most often commented on, is that between Calvin's description of "natural" man, and Hobbes's picture of man in the state of nature.[40] Calvin, like

[34] Though Hobbes rejected most of Aristotle, calling him "the worst teacher that ever was," he approved of his *Rhetoric:* "but his Rhetoric and discourse on animals was rare." Quoted in Strauss, *The Political Philosophy of Hobbes*, p. 35. For a discussion of the similarities between Hobbes's work and Aristotle's *Rhetoric* see Strauss, pp. 30–43.

[35] Aristotle, *Basic Works*, ed. Richard McKeon (New York: Random House, 1947) pp. 327–28, 1331. According to Hobbes's own translation, "in rhetoric the principles must be common opinions, such as the judge is already possessed with. . . . the end of rhetoric is victory, which consists in having gotten belief. . . . belief is not gotten only by proofs, but also from manners; the manners of each sort of commonwealth ought to be well understood by him that undertaketh to persuade or dissuade in matter of state." *E.W.*, VI, 426, 436.

[36] Hobbes does refer to other groups, including Catholics, as causes of the civil war, but they play a very minor role in comparison with the Presbyterians. See *Behemoth*, p. 20; Macgillivray, "Hobbes's History of The Civil War," p. 190.

[37] That Hobbes was familiar enough with Calvin to have adopted this approach is very likely; he attended the strongly Puritan Magdalen Hall, Oxford, where students normally read Calvin. For comments on Hobbes's interest in religious issues see Doyle, "The Contemporary Background of Hobbes's State of Nature," *Economica* 7 (December, 1927), 336–355; Willis B. Glover, "God and Thomas Hobbes" in Keith C. Brown, ed., *Hobbes Studies* (Cambridge: Harvard University Press, 1965), p. 142.

[38] In constructing his parallel argument Hobbes was perhaps subtly employing his "good fancy", i.e., the ability to observe "similitudes . . . as are but rarely observed by others," a skill which he recognized was sometimes necessary in order to achieve understanding: "In demonstration, in counsel, and all rigorous search of truth, judgment does all, except sometimes the understanding have need to be opened by some apt similitude; and then there is so much use of fancy" (*Leviathan*, pp. 43–44). Hobbes might have expected that the Puritans' political understanding would be opened by the apt similitude he presented them.

[39] Since this paper is concerned with Hobbes's political theory and not with his statements on religion, it will not deal with Bks. III and IV of *Leviathan*. Hobbes's discussion of religion follows the same pattern as his political theory; it is an argument aimed at the two religious groups most politically disruptive, the Puritans and the Catholics. See *Leviathan*, p. 452; J. G. A. Pocock, "Time, History, and Eschatology in the Thought of Thomas Hobbes" in his *Politics, Language and Time* (New York: Atheneum, 1971), pp. 179, 180–7.

[40] Walzer, *The Revolution of The Saints* (New York: Atheneum, 1968) pp. 26, 34, 41, 42, 47; Sanford Lakoff, *Equality in Political Philosophy* (Cambridge: Harvard University Press, 1964) pp. 70–79; Willis B. Glover,

Hobbes, saw men living in a disagreeable, insecure, warlike condition in which death was an ever present danger: "Human life is beset by innumerable evils, and threatened with a thousand deaths. . . . Now withersoever you turn, all the objects around you are not only unworthy of your confidence, but almost openly menace you, and seem to threaten immediate death. . . . Amidst these difficult times, must not man be most miserable, who is half dead while he lives, and is dispirited and alarmed as though he had a sword perpetually applied to his neck."[41] Calvin also found man to be vain, proud, and desirous of power and preeminence: "I say that the nature of man is such that every man would be a lord and master over his neighbors and no man by his good will would be a subject . . . men are of so perverse and crooked a nature that everyone would scratch out his neighbor's eyes if there were no bridle to hold them in. . . . Subject, then, to so many forms of wickedness, they drag out their lives in ceaseless tumult and disquiet."[42]

Hobbesian man is proud, vain, contentious, desirous of power, and dominated by his passions. Hobbes finds a "natural proclivity of men to hurt each other, which they derive from their passions . . . chiefly from a vain esteem of themselves." They are so inflated with self-love that they refuse to acknowledge their equality, but rather hold themselves superior to others. Hence from birth, men are involved in "a perpetual contention for honour, riches, and authority," and "scramble for everything they covet and would have all the world, if they could, to fear and obey them." Out of desire for gain, reputation, or their own safety, men continually quarrel and threaten each other. Consequently the condition in which such men exist, being without government or order, is a "war of all against all," in which they experience "continual fear, and danger of violent death"; and their lives are "solitary, poor, nasty, brutish, and short."[43]

Calvin further characterizes man as a being who, though not totally without reason, is dominated by his impulses and passions: "man neither rationally chooses . . . nor takes the advice of reason, nor duly exerts his understanding; but without reason, without reflection, follows his natural inclination like the herds of the field." Even when his reason is not dominated by his passions it accomplishes little, for it is debilitated and deformed. Man's understanding can be of two kinds, that of terrestrial matters, including politics and domestic economy, and that of celestial matters, such as God and his kingdom, true righteousness, and the blessedness of a future life. Because of man's sin, his ability to understand the former has been corrupted, while he has been totally deprived of his ability to comprehend the latter (i.e., faith). In other words, by the Fall, man lost the only means by which he can know God (for faith consists in the knowledge of God and divine will) and on which rested his hope for salvation.[44]

In this condition, suggests Calvin, man is not only miserable but is not even on the same level as the rest of brute creation: " . . . men, because by their own fault, they are fallen to that degree of fatuity, that now they approach more nearly to beasts than to true man . . . men entirely destitute of religion not only do not excel the brutes, but are in many respects far more wretched. . . . The worship of God is therefore the only thing which renders men superior to brutes." Thus it seems that even with his reason, man may fall as low as the beasts. Yet impressed upon the mind of man is the idea of the deity, and no matter how much man may deny or ignore it, there remains an innate impression of the divine existence. While this faint impression is not true knowledge or worship of God, it offers the potential for faith, and thus a truly human existence.[45]

Corresponding to Calvin's argument, Hobbes pictures man, in his brutish condition of "mere nature," existing in a manner almost indistinguishable from that of animals. The only ability man has naturally is prudence, which is based on experience. But prudence by itself does not put man above animals, rather it seems that man is worse off: "it is not prudence that distinguisheth man from beast. There are beasts, that at a year old observe more, and pursue that which is for their good, more prudently, than a child can do at ten."[46] Since man is not distinguished from beasts naturally, i.e., from birth, it must be by something he achieves. The unique human achievement is the use of reason.

> Man excelleth beasts only in making rules to himself, that is to say, in remembering and reasoning aright upon that which he remembereth. They which do so, deserve an honour above brute beasts. . . . So that it is not merely the nature of man, that makes him worthier than other living creatures, but the knowledge

"Human Nature and the State in Hobbes," *Journal of the History of Philosophy*, vol. IV, no. 4 (October 1966) 294–311; Christopher Hill, *Society and Puritanism* (New York: Schocken, 1967) p. 242: Francis C. Hood, *The Divine Politics of Thomas Hobbes* (Oxford: Clarendon, 1964), pp. 2–3, 66.

[41] *The Institutes of the Christian Religion*, trans. John Allen (Grand Rapids: W. B. Eerdmans, 1949) I, xvii, 10.

[42] Quoted in Walzer, pp. 31, 33; Calvin, *Works*, vol. 1 (Edinburgh: Calvin Translation Society, 1844) p. 94.

[43] *De Cive*, p. 29; *Leviathan*, p. 460; *E.W.* VII, 73; see also *Leviathan*, pp. 80–82, 111, 118.

[44] *Institutes*, II, ii, 26; II, ii, 12–13; III, ii, 2, 6–7, 14–15.

[45] *Works*, I, 243; *Institutes*, I, iii, 3.

[46] *Leviathan*, p. 16, also p. 43; *De Cive*, p. 13.

that he acquires by meditation and by the right use of reason in making good rules of his future actions.[47]

Although reason is not a natural ability like prudence, Hobbes sees the potential for reason existing in every person. Thus for example, although children are not endowed with reason, they are "called reasonable creatures for the possibility apparent of having the use of reason in the time to come." Unlike prudence, however reason does not develop automatically with growth or experience but requires effort and assistance. According to Hobbes those faculties which are "proper to man only," such as reason and science, are "acquired and increased by study and industry; and of most men learned by instruction and discipline; and proceed all from the invention of words and speech."[48] Since speech is not born with man, but must be invented, there is always the possibility that it will be misused so that men are caused grief, and are unable to communicate or cooperate.[49] Clearly the state of nature is such a situation, since the freedom of each person to define words as he wishes, results in an anarchy of meaning. On the other hand, if speech and reason are used correctly, man's "faculties may be improved to such a height, as to distinguish men from all other living creatures." Hobbes sees the process as follows: ". . . the light of human minds is perspicuous words, but by exact definitions first snuffed, and purged from ambiguity; *reason* is the *pace*; increase of *science*, the *way*; and the benefit of mankind, the *end*."[50] The correct use of reason, which refers to, as already noted, the making of good rules for one's future actions, implies the creation of some system of rules or order in which man can live. Only within a system of order can consistent and intelligible speech be maintained, the growth of reason and science be possible, and mankind benefited. The next step in the argument, for both Hobbes and Calvin, is the establishment of that condition in which man realizes his potential for a truly human existence.

Central to Calvin's argument is his insistence that only by the grace of God could man be saved from his miserable, brutish condition. God's grace is given to man through the covenant with Christ in the church. This "covenent of grace" is totally the work of God, for "grace is not merely offered by the Lord to be either received or rejected according to the free choice of each, but that it is grace which produces both the choice and will in the heart." Even though God accomplishes all by his own power, grace is given in the form of a covenant, which implies two participating and responsible parties. It is "as if God had said, 'see how kindly I indulge thee: for I do not require integrity from thee simply on account of my authority, which I might justly do; but whereas I owe thee nothing, I condescend graciously to engage in a mutual covenant'."[51]

Thus through the concept of a covenant of grace, Calvin is able to combine two things: man's moral obligation to obey God, based on his having agreed to the covenant, and God's absolute sovereignty in his dealing with man. The relationship between God and man is thus one of simple command and obedience. Regenerated by God's grace, men become "serviceable" to God, but they do not at all limit divine sovereignty. In other words, the covenant is a way of activating man into obedience, but not of controlling God.[52]

The emphasis of the covenant is on the community formed by it, not on the individual, for it is the same covenant made with the Jews, which was national and social. While God's grace is given to individuals, a community of the elect is formed through the covenant with Christ. Moreover as the church consisting of the whole body of the elect blurs into the church as a social institution with powers of compulsion, the covenant becomes "a social commitment to obey God's law . . . a social act and subject to social enforcement in God's name."[53] Thus it becomes clear that it is by covenanting into the community of the church that man is saved from his brutishness.

For Hobbes, a system of rules, i.e., civil society, can only be established by individuals covenanting among themselves to set up an absolute sovereign authority to rule over them. Since men are naturally equal and not subject to any authority, it is only on the basis of consent that an artificial system of rules can be established. In addition, only if all rights to act according to private judgment are surrendered to an absolute authority, can the burden of fear, conflict, and brutishness be removed. That is, given the nature of Hobbesian man, the "war of all against all" can only be ended and order established by submitting to a common reason:

> When there is a controversy in an account, the parties must by their own accord, set up, for right reason, the reason of some arbitrator, or judge, to whose sentence they will both stand, or their controversy must either come to blows or be undecided . . . so is it also in all debates of what kind so ever.[54]

[47] *E.W.*, V. 186.
[48] *Leviathan*, pp. 29, 17; also pp. 46, 80.
[49] *Leviathan*, pp. 19, 30, 111.
[50] *Ibid.*, pp. 17, 29–30.
[51] *Institutes*, II, iii, 1; II, iii, 13; II, iii, 7; *Works*, I, 444.
[52] *Works*, I, 443; Walzer, *The Revolution of the Saints*, p. 167.
[53] *Institutes*, II, iii, 1; Walzer, pp. 56–57; Ernest Troeltsch, *The Social Techniques of the Christian Churches*, two vols. (New York: Harper and Row, 1960) II, 586.
[54] *Leviathan*, p. 26.

Thus we can see that by the same means by which men move "up" from the level of beasts to a distinctively human existence, men also move "out" of the dangerous, violent state of nature. That is, men rightly and beneficially use their reason to make rules for their future actions, by covenanting together to create a commonwealth ruled by an absolute sovereign. That man cannot be fully human, "worthier than other living creatures," outside civil society, should be clear in two ways. First what reason men are able to muster in the state of nature would lead them out of it, since "reason suggesteth covenient articles of peace, upon which men may be drawn to agreement," which when joined to the fear of violent death, would lead men to covenant into civil society.[55] Second, since reason and science can only develop on the basis of consistently defined words, and since most men can only acquire the use of reason "by instruction and discipline," civil society must already exist for them to be able to do so. Hence, man who is not recognizably different from other living creatures by nature, becomes so by creating and living in civil society.

While reason (or faith) is the distinctive human quality realized by covenanting, it is not what motivates man to covenant. Calvin, when he is not emphasizing God's power and grace in the making of the covenant, attributes to man the motive of fear. For example, the "pious man" in his natural state is oppressed by "extreme anxiety and dread," and "as he justly dreads fortune, so he ventures securely to commit himself to God." Reflecting "that he is in imminent danger of death," despairing of his strength, "the man terrified with the apprehension of eternal death, which he sees justly impending over him for his unrighteousness, betakes himself entirely to the Divine mercy as to the only port of salvation."[56]

Fear of death is the most appropriate motive for covenanting, since what is offered to man is life. The fall of Adam was a fall from life to death in that the spiritual life of Adam consisted in union with his maker, so alienation from him was the death of his soul. "Under the name of death is comprehended all those miseries in which Adam involved himself by his defection . . . the life of man without God [is] to be wretched and lost, and therefore differing nothing from death." But the life which had been lost by Adam is recoverable in Christ, through the covenant of grace.[57] In other words the covenant with Christ is the means by which the elect leave a condition threatening "eternal death," and enter into a community in which their spiritual lives are secured.

Clearly fear of violent death plays a fundamental role in the actions of Hobbesian men. While reason shows them how to leave the state of nature, it is their fear that pushes them out. In the case of a "commonwealth by institution" men covenant out of fear of one another, while in the case of a "commonwealth by acquisition" they covenant out of fear of their conquerer. "In both cases they do it for fear." The Hobbesian man will agree to the setting up of an absolute sovereign only when fear of violent death shocks him into recognizing the precariousness of his situation, and motivates him to curb his vanity and illusions of superiority and power, so that he takes those steps necessary for his own preservation.[58]

To sum up the arguments, we can see that the structure of Hobbes's argument follows that of Calvin, with the change that religious faith has been replaced by reason. Both begin with "natural" man who, vain, contentious, driven by his passions, exists in a threatening, brutish condition. Left to his own resources, or natural abilities, he is unable to secure his life or his future, and risks sinking to the level of beasts. Yet man has the possibility of escaping his insecure state since he has the potential for that quality (faith, reason) which makes him truly human, able to live above the level of beasts. Driven by the fear of death, man will overcome his pride, recognize the truth of his condition, and covenant into civil society. Thus it is clear that for both Hobbes and Calvin an original brutish condition, man's pride, the fear of death, and the concept of the covenant are basic components of their arguments, which both culminate in man's entering a community. The structure of those communities will be examined below.

The Structure of Community

Calvin holds that the community of the church which the elect enter by covenanting is not the product of the elect, but of God alone. Since the unity of the church consists in the elect's close relationship to God (or more precisely Christ as the mediator between man and God), and since all of man's knowledge of God, as well as man's strength and salvation, are the gifts of God's grace, it is impossible that man be able to determine the form of his church. Each man, on his own, would ignorantly adulterate the "pure religion" and fantasize his own idol instead of God thereby separating himself from God and dissolving the church. Thus "it is not surprising if the Holy Spirit reject, as spurious, every form of worship which is of human contrivance."[59] In other

[55] *Ibid.*, p. 84; see also p. 242.
[56] *Institutes*, I, xvii, 11; II, vii, 3; II, viii, 3.
[57] *Works*, I, 127; *Institutes*, II, i, 5–6; II, vi, 2.
[58] *Leviathan*, pp. 129–130; also *De Cive*, p. 24n; Oakeshott, *Leviathan*, intro. p. xxxvi.
[59] *Institutes*, I, v, 13; I, iv, 1–3. Wolin points out that Calvin insisted that interpretation of scripture be limited

words the community exists only insofar as the members neither act nor judge on their own but submit to divine will and providence.

While the elect accept their community as given to them by God, Hobbesian men, using their reason might appear to create their own commonwealth. Yet this is not so, according to Hobbes, for men use their own reason only so far as to agree upon a sovereign; after that "right reason" is the reason of the sovereign. Ironically men use what reason they have in the state of nature only in order to submit to the reason of another: ". . . in a civil government the reason of the supreme, that is, the civil law, is to be received by each subject for the right." What Hobbes attempts to demonstrate is that it is the conclusion of reason that the individual's right to make crucial moral and political decisions must be given up. That is, the commonwealth cannot survive unless individual reasoning (so far as conduct is concerned) from the moment of covenanting is replaced by the reasoning of the sovereign.[60] In giving up his original reason, Hobbesian man gives up the power to determine the actual form his commonwealth will take.

The sovereign, by being sovereign, necessarily has a free hand in setting up and ruling the commonwealth. Arguing that whoever has the right to an end, has the right to the means to that end, Hobbes points out that since "the sovereign is charged with the end, which is the common peace and defense, he is understood to have power to use such means, as he shall think most fit for his discharge . . . to do whatsoever he shall think necessary to be done. . . "[61] In other words, Hobbes's subjects covenant into they know not what, creating their commonwealth only insofar as they consent to another's creating it. "We make a commonwealth," says Hobbes, by "the act of our submission."[62] Thus it is clear that for Hobbes men must accept their community as given to them, just as much as Calvin's elect accept theirs.

A fundamental aspect of Calvin's thinking is his emphasis on God's omnipotence and sovereignty.[63] Just as God actively regulates the whole universe, relying solely on his own power, so he rules over the community of the elect. Though God often works indirectly, using animate and inanimate objects as his instruments, they do not share in, or detract from, his power. Even angels, which God made the ministers of his power and glory, have no independence of Him, they do not share in his glory or power. Nor is God limited by the laws of nature, for they are only the result of his will. Similarly the moral law which God laid down for man is not binding on God himself, since he is above the law and what he once commanded, he may also reverse.[64]

The Hobbesian sovereign is, in many respects, very similar to Calvin's God. The sovereign, called by Hobbes, a "mortal god," is above the civil law, with unlimited power and authority. Hobbes pictures the sovereign as so great and powerful, that like a god, "the power and honour of subjects vanisheth in the presence of the power soverign." Again like the Calvinistic God, the sovereign simply wills or commands the law, while remaining unhindered by it. Justice is obedience to the law, while the law, defining good and evil, is the public conscience; just as righteousness, for Calvin, is obedience to the word of God. Hobbes, in fact, explicitly compares the sovereign to God in this respect, for just as "God cannot sin, because his doing a thing makes it just, and consequently, no sin," sovereigns "make the things they command just, by commanding them, and those which they forbid, unjust by forbidding them."[65]

For Hobbes the commonwealth is held together by the civil laws, which tie the subjects to the sovereign. Emphasizing the "top down" character of the commonwealth, Hobbes refers to the laws as "artificial chains" which are "fastened at one end, to the lips of that man, or assembly, to whom they have given the sovereign power; and at the other end to their own ears." The equivalent link in Calvin is God's word, which unites the community by tying each member to God. In both cases there is a demand and need for a uniform public truth, set down by the sovereign, without which the community would dissolve.[66] In Calvin's community nothing happens but by God's command or permission, while in the Leviathan, whatever happens is either com-

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to the appropriate officers of the church, who presumably would be among the elect, thereby avoiding the disintegrating effects of private visions in the institutional church as well. Sheldon S. Wolin, *Politics and Vision* p. 180.

[60] *DeCive*, p. 32n; also *Leviathan*, pp. 174, 176. The reason which men have in civil society is the creation of, and dependent on, that society. See Wolin, *Politics and Vision*, p. 257 ff; Blair Campbell, "Prescription and Description in Political Thought, The Case for Hobbes," *American Political Science Review*, 65 (June, 1971), 382–384.

[61] *Leviathan*, pp. 118, 116.

[62] Quoted in Watkins, *Hobbes's System of Ideas*, p. 74. See Harvey C. Mansfield, "Hobbes and the Science of Indirect Government," *American Political Science Review*, 65 (March, 1971), 100–101.

[63] Calvin's use of political language in describing God (e.g., "divine government", "sovereignty", "ruling") allows for, if not encourages, a political interpretation of his doctrine.

[64] *Institutes*, I, xvi, 1–4; I, xvii, 6; I, xiv, 12; *Works*, I, 82–3.

[65] *Leviathan*, pp. 112, 119, 120; *E.W.*, IV, 250; *De Cive*, p. 129.

[66] *Leviathan*, p. 138.

manded or "pretermitted" by the sovereign: a subject's freedom to do as he pleases in a given area of activity is contingent upon the sovereign's not having laid down any rules for that area.[67] Thus for both Hobbes and Calvin the community is structured in such a way that the sovereign stands at the center, uniting the whole, by tying each member to him through his commands.

While knowledge of God's will is essential to the community, it is not sufficient in itself to sustain the community, even with God's paternal love. Fear is equally necessary ". . . for wherever the fear of God does not reign, men rush onward to every kind of wickedness; so that they neither spare human blood, nor restrain themselves from rapine, violence and contumelies . . . it is the fear of God alone which unites us together in the bonds of our common humanity . . . otherwise we should devour each other like wild beasts."[68] In other words the church exists, in part, because the fear of death which motivated man in his "natural" condition has been replaced by the fear of God.

The role of fear is virtually the same in Hobbes's argument: man's condition in the state of nature is one of fear; it is fear that drives man to covenant; and it is essentially fear that holds the commonwealth together. Just as Calvin recognized that love was insufficient to make men behave civilly, Hobbes saw that unless men "be restrained through fear of some coercive power" they "distrust and dread each other," and their passions carry them to "partiality, pride, revenge, and the like." Moreover once men realize that the only alternative to complete submission to the sovereign is to be thrown back into the anarchic state of nature, they will want to behave civilly out of fear of that possibility. That is, it is not so much the difficulty but the danger of rebelling that keeps men civil for Hobbes.[69]

A prominent characteristic of Calvin's God is that he is incomprehensible to man. Although man knows God through faith, man still does not understand God or his providence. "When in defining faith we call it knowledge," Calvin explains, "we mean not thereby a comprehending, such as man use to have of those things that are subject to man's understanding." God rules by his secret and incomprehensible wisdom; thus while faith brings knowledge of God's word, his purposes remain inscrutable. According to Troeltsch, Calvin's God is irrational in the sense that he is not to be measured by the standards of human reason or logic.[70] The incomprehensible, irrational nature of God cannot be a pretext for disregarding or ignoring his word, for that can only lead to death as it did for Adam. The divine word "is the will of God, therefore it must be endured, not only because resistance is unlawful and in vain, but because he wills nothing but what is both just and expedient."[71] God's will is always just and good, regardless of how it appears to man, and must always be obeyed.

While Hobbes does not present his sovereign as incomprehensible, he may be unintelligible to his subjects for there is no requirement that the reason of the sovereign be reasonable in any conventional sense. In setting up the sovereign's reason as the "right reason" the subjects commit themselves to abide by his judgments, and to give up the right to judge the reasonableness of the sovereign's actions. Thus Hobbes's sovereign is inscrutable in the same sense as Troeltsch found Calvin's God irrational: he is not to be measured or scrutinized by the standards or reason of his subjects. The subjects have no alternative but to accept on faith the belief that their sovereign's actions are rational and beneficial.

Although it is likely that the sovereign would be more or less rational in setting up his "system of rules," yet were his word to become irrational or unintelligible, the subjects, still obligated to obey and not criticize, could only "captivate their understanding." Hobbes explains: "We then captivate our understanding and reason, when we forebear contradiction; when we so speak, as by lawful authority we are commanded; and when we live accordingly; which in sum, is trust and faith reposed in him that speaketh, though the mind be incapable of any notion at all from the words spoken."[72] Thus we can see the element of faith in Hobbes's commonwealth. The subjects' reason becomes (at least in the public realm) as dependent upon the sovereign, as is the elect's faith in God, for both rest on acceptance of the sovereign's word. While Hobbes's sovereign may be inscrutable only procedurally (legally), and not ontologically, as is Calvin's God, this distinction matters very little viewed from the perspective of their subjects.

Hobbes's use of Calvinism becomes quite clear in his discussion of the causes of the dissolution of civil society. Since, according to Hobbes, "it belongs to kings to discern between good and evil," it is appropriate that the major cause of disorder is (following the religious analogy) the doctrine "that the knowledge of good and evil belongs to each single man."[73] In stressing the dangers of political opposition, Hobbes refers his

[67] *Ibid.*, pp. 138–139, 143.
[68] *Works*, I, 529.
[69] *De Cive*, p. 11; *Leviathan*, p. 109, also pp. 86, 89, 138; *Behemoth*, p. 144.
[70] *Institutes*, III, ii, 14–15; Troeltsch, II, 585.
[71] *Institutes*, II, i, 4; I, xvii, 2; I, xvii, 8.
[72] *Leviathan*, p. 243, also p. 41.
[73] *De Cive*, pp. 129, 128; also *Leviathan*, p. 211.

Puritan audience to the fact that original sin consisted of a disobedient and presumptuous desire to judge good and evil:

> But private men, while they assume to themselves the knowledge of good and evil, desire to be even as kings, which cannot be with the safety of the commonweal. The most ancient of all God's commands is, (Gen. ii, 7); *Thou shalt not eat of the tree of knowledge of good and evil;* and the most ancient of all diabolical temptations, (Gen. iii, 5): *Ye shall be as gods, knowing good and evil.*[74]

Moreover Hobbes also alludes to a prelapsarian time of innocence when "it was peace and a golden age." In this time, there was no disorder for there was no questioning of the right of rulers to command. Princes exercised their supreme power, "not by arguments, but by punishing the wicked, and protecting the good. Likewise subjects did not measure what was just by the saying and judgments of private men, but by the laws of the realm"; subjects also "reverenced the supreme power, whether residing in one man or in a council, as a certain visible divinity." These "ancients" were able to sustain this idyllic state by keeping "the science of justice wrapped up in fables," not exposed to public disputation. But once "the science of justice" was made available to everyone, princes were exposed to the questioning and judgment of private men, and their subjects joined "themselves with ambitious and hellish spirits, to the utter ruin of their state," falling thereby into the state of nature.[75] In other words Hobbes supported his case for political absolutism and the prohibition of political dissent by drawing on the story of the Fall, specifically on the Puritan "epic of the two Adams": the "old Adam" who fell from God's grace because of his sin, followed by the "new Adam," Christ, with whom the faithful are united in a new community, overcoming that sin.[76] The state of nature, the equivalent of the condition of fallen man, can be escaped only by overcoming that sin (the private judging of good and evil) which originally caused man to fall from the peaceful, orderly state. Clearly Hobbes intended that his own writings, by definitively explaining the "science of justice," would dissuade people from ever again committing that sin, and thereby restore the prince's power and authority and peaceful civil society.

As a consequence of Hobbes's use of Calvinism, we would expect to find some notion of community in his political theory, since Calvin had a strong sense of community. Yet it is a commonplace that Hobbes's extreme individualism precludes the notion of community.[77] It has been said that the unity of Hobbes's commonwealth is nothing more than a rationalistic, legalistic fiction, and that the active support and participation in a community by its members, which is essential to its life, is totally absent from Hobbes's understanding of the political order. On the basis of Hobbes's theory of language, it has been said that Hobbesian men can hardly communicate with each other, let alone participate in any community.[78] Moreover it has also been argued that as a consequence of Hobbes's failure to incorporate any kind of political activity in his system, the sovereign, lacking any popular support, is not the powerful, dominating figure Hobbes claims him to be. In the face of private sources of power, such as money or property, he is a mere "spitfrog."[79] Yet against these criticisms, we can place the fact that Hobbes himself thought he was discussing a real community. He insisted that genuine unity rather than a mere collection of individuals was created by the covenant: "A multitude of men are made *one* person. . . . This is more than consent or concord: it is a real unity of them all in one and the same person. . . . "[80] In the light of Hobbes's insistence on the reality of his community, the failure of Hobbes's readers to find any sense of community in his theory may be due to his having relied on a religious rather than a political model of community. Thus if we looked at the Leviathan as if it were modeled on the Calvinist community of the elect, united in Christ, we might gain a new understanding of its dynamics and unity.

While Hobbes does speak of uniting the strength of the subjects in the will of the sovereign it is never clear how this is accomplished. Rather it seems that the sovereign acts on his own, while the subjects simply "step aside," and that consequently the unity is without substance. Through covenanting, subjects are said to transfer their rights to the sovereign, but this transferring of rights appears to have only a negative meaning since "the conveyance of right consists merely in not resisting."[81] Yet there may still be real unity in the Leviathan if the strength and life of the commonwealth is seen as originating not with the subjects, but with the sovereign. As with the elect united in Christ, all life and strength may

[74] *De Cive*, p. 129.
[75] *De Cive*, p. 9; cf. Lakoff, p. 73.
[76] See William Haller, *The Rise of Puritanism* (New York: Harper and Row, 1957), pp. 152–160.
[77] E.g. Glover, "Human Nature", p. 304.
[78] According to Watkins, *Hobbes's System of Ideas*, p. 101, "Each of us is condemned to solitary confinement within his body. We can transmit and receive signals through the walls of our cells, and we may even work out and execute a common plan. But our minds never meet."
[79] Harrington's term, quoted in Wolin, *Politics and Vision*, p. 285.
[80] *Leviathan*, pp. 107, 112.
[81] *De Cive*, p. 33.

actually flow from the head to the body, not the reverse; the members of the community depend upon the sovereign for their life and motion. "It is true," argues Hobbes, that subjects "cohere together; but they depend only on the sovereign, which is the soul of the commonwealth, which failing; the commonwealth is dissolved into a civil war, no one man so much as cohering to another, for want of a common dependence on a known sovereign; just as the members of the natural body dissolve into earth, for want of a soul to hold them together."[82] Here Hobbes seems very close to Calvin's understanding of the unity of the community of the elect, who are united by their common dependence on God, and " . . . as soon as the Lord takes away his spirit, all things return to their dust and vanish away."[83]

The use of the religious term "soul" to describe the sovereign seems out of place for a materialist like Hobbes. One would expect Hobbes, following his comparison with the natural body, to have called the sovereign the head or heart of the commonwealth, for according to his own theory, the body is mind dependent, and the mind, heart dependent.[84] Moreover the dependence relationship Hobbes employs, which appears to have a religious origin, may explain the lack of participation and activity in his commonwealth. The Calvinistic God, being totally self-sufficient, does not need to mobilize the support of the members of his community; he is lacking in nothing. Similarly Hobbes seems to have envisioned a community which did not need participation or the mobilization of popular strength, since the Leviathan was by himself so powerful that he gave support, indeed life, to his subjects. Perhaps it was because his community followed a religious pattern that Hobbes described the sovereign in such dramatic, awe-inspiring language, and called him a "mortal god"; a community so structured would need a self-sufficient, god-like being at its center in order to survive.

That the subjects do not seem to cooperate or work together in the public sphere is not a problem for the commonwealth; indeed, it is an error, argues Hobbes, to say that the members of a commonwealth "depend one of another."[85] Just as in Calvin's community, the essential relationship is the strong tie between the individual member and the sovereign. While it may be true that the subjects do not really communicate, they are united together, by all being tied to the same sovereign authority. The lack of involvement and activity usually associated with the idea of community thus is due not to Hobbes's lack of any sense of community, but to his relying on a particular conception of community. If Hobbes failed to create real political power and unity, and if his sovereign turned out to be only a "spitfrog," it was because his community required a god to sustain it and Hobbes had only men.

In summary of the above discussion, Hobbes presented man with the same two alternatives as had Calvin: a deadly, brutish, "fallen" condition of disorder, versus an orderly secure, moral community ruled by an absolute sovereign. Relying on fear to motivate man, as did Calvin, Hobbes uses the device of the covenant to establish his civil society. At times Hobbes even compares his covenant to a religious one, stressing that, in his civil society, as "in the commonwealth of the Jews, God himself was made the sovereign, by pact with the people."[86] Hobbes's commonwealth is itself comparable structurally with Calvin's community; in both cases the sovereign stands at the center, sustaining and uniting the members of the community. Hobbes even co-opts religious terminology, calling his commonwealth a "church."[87] Similarly the Hobbesian sovereign is much like Calvin's God, having absolute power and authority, and being the source of all morality. Thus it is clear that both in the process of man's moving from his natural condition into civil society, and in the actual commonwealth set up, Hobbes follows Calvin's religious doctrine.

Conclusion

We can now see how Hobbes structured his defense of the king and order to appeal to what was, politically, his most important audience, the Puritans. By drawing on the logic of Calvinism, Hobbes attempted to persuade his Puritan audience of the need to support, rather than to attack, the political order. Following a pattern of argument with which Puritans were familiar, he hoped to provide for the political order the kind of obedience and loyalty that the Puritans had felt only for their religion. By replacing the church with the political order as the civilizing, humanizing, institution, Hobbes was, in effect, saying to his readers that the dangers of political rebellion and disorder were as great and immediate as they had hitherto believed were only associated with the abandonment of religion: to fall to fearful depths of brutishness, suffering, and death.[88] And just as it would be inconceivable for there to be

[82] *Leviathan*, p. 379, also pp. 5, 218.
[83] *Works*, I, 74.
[84] Watkins, p. 103. Cf. Christopher Hill, "William Harvey and the Idea of Monarchy," *Past and Present*, 27 (April, 1964), 54–72.
[85] *Leviathan*, p. 379.
[86] *Ibid.*, p. 463.
[87] *Ibid.*, pp. 255, 305, 306.
[88] See Ashcraft, "Hobbes's Natural Man," pp. 1089–90, 1109–1111.

moral grounds on which resistance to God could be justified, so Hobbes destroys the possibility of legitimate political opposition by making his "mortal god" the source of all morality and justice. In other words, the political absolutism desired by Hobbes, hitherto defended only anachronistically on the basis of the philosophy of order, Hobbes now justified by working from a Calvinist notion of man to a Calvinist notion of community, an argument which supports the king's authority far more forcefully than a conventional royalist ever could.

Our discussion of Hobbes, beyond providing an interpretation of certain aspects of his theory, should provide support for our general view of political theory. Taking Hobbes as an example, it should be clear that political theorizing can be fruitfully seen as a form of persuasive argument aimed at a specific audience. It is clear that Hobbes, a strong partisan, was trying to convince his countrymen of the folly of their action and of the need to support their king in all matters. Success in this task clearly rested on his ability to communicate to, and persuade his audience. Unlike a lawyer arguing his case in a courtroom, Hobbes had no control over the makeup of his audience; rather it was given to him by the existing political situation. That situation affected the kind of arguments Hobbes employed in two ways. First the fact that the conventional royalist arguments had been discredited, demanded that he (to be effective) justify his royalist position on new grounds, with new arguments. Second, those groups or developments which had done most to undermine the old royalist position determined the audiences to which he had to address himself, and the terms in which he structured his argument. We have seen that Hobbes did present his case on several levels, each designed to appeal to a distinct audience. While the "scientific" Hobbes and the "bourgeois" Hobbes are already well known, this paper has explored Hobbes's "Puritan" argument. (In so doing, I have not presumed, of course, to have exhausted all the levels of Hobbes's argument.) The greatness of Hobbes's work, and what distinguishes it from other political writings, rests on his ability to leave behind the conventional and ineffective arguments of his day, and to create a new world-view, which provides strong support for his political position, and appears powerful and persuasive to a very heterogeneous audience.

This view of political theory has several implications for the study of past political theorists. It should be clear that no part of a work of theory can be ignored or assumed to be irrelevant, if we are to comprehend it in its entirety. If we examine theory only from the perspective of the logician, we may miss some of the ways in which the theorist tried to convey his message, as well as aspects of the message itself. Since a theorist may be addressing himself to more than one audience, his theory may contain more than one argument. By assuming that there is only a single argument, one may mistakenly conclude that various aspects of a work are inconsistent or irrelevant, while they may, in fact, be parts of different arguments. Since a work of political theory may contain several arguments, the attempt to extract a single logic from it would be, in many cases, a mistaken effort. For example, many commentators on Hobbes have argued about his theory of obligation without apparently considering the possibility that Hobbes was presenting several justifications for political obedience. Finally it will be necessary to study the context in which a theorist wrote, for without knowing the issues, arguments, participants, etc. on the political scene, one may not recognize all aspects of a theorist's argument, especially those aspects addressed to an audience with which we today have little in common. Moreover, the significance of an argument for either the theorist or his audience will not be evident without some familiarity with the context in which he wrote. In this study I have tried to show that by taking a broader, historical approach to the study of political theory, one may come to a more comprehensive and dynamic understanding of the activity of political theorizing.

Separate and Unequal: Governmental Inequality in the Metropolis*

RICHARD CHILD HILL
Michigan State University

"The suburb is the Northern way to insure separate and unequal. It has the advantage of being legal. If housing, education, jobs, and matrimony are to remain a charmed circle among formally equal citizens in an era of public goods, there is a powerful logic behind the existing fragmentation and the basis for considerable resistance to the creation of really general governments."[1]

Introduction

Political incorporation by class and status into municipal enclaves is an important institutional mechanism for creating and perpetuating inequality among residents in metropolitan communities in the United States. The salience of governmental inequality has been increasing. In a laissez-faire, capitalist society the distribution of valued commodities among urban residents is largely governed by the distribution of "effective demand" among consumers engaging in price competition in the private market place. With the rapid growth in the public sector of the economy in recent decades, however, there have been vast increases in the consumption of public goods.[2] Concomitant with growth in the public sector, the segregation of public goods consumption through suburbanization has become a central part of the structure of inequality among urban residents.[3] The municipal segregation of class and status groups tends to divorce fiscal resources from public needs in the metropolis. As one student of the problem has recently noted:

> Because the political subdivisions of a metropolitan area are largely autonomous in matters of local finance, differentials in per capita income create inequalities in both fiscal capacity and public service needs between municipalities. And because we entrust local government to effect substantial redistribution of real income through local public services, a serious problem follows from the divorcing of income from need.[4]

This paper presents an exploratory analysis of factors associated with inequality in the distribution of fiscal resources—the capacity to generate public goods—among municipal governments in metropolitan areas in the United States. I commence by outlining a conceptual framework which then serves to organize the analysis of empirical findings. I conclude with a brief discussion of important directions for further research.

Urban Stratification

The student of metropolitan political structure can draw upon a number of conceptual models to organize an empirical investigation. For example, urban-oriented political scientists have viewed the metropolis as a world arena in miniature in which "municipal nation-states" conduct delicate negotiations[5]; as a market in which municipal corporations compete for the allegiance of fickle citizen-consumers[6]; as an ecological coliseum housing recurrent games played by evanascent political teams[7]; as a system of specialized, incorporated groups seeking to promote life-styles and protect values through municipal balkanization[8]; and the list goes on.

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[1] Norton E. Long, "Political Science and the City," in *Urban Research and Policy Planning*, Leo F. Schnore and Henry Fagin, editors (Beverly Hills: Sage Publications, 1967), p. 254.

[2] Total expenditures by all levels of U.S. government rose from 12.8 per cent of GNP in 1945-1950 to 22.4 per cent in 1966–1970. State and local government spending almost doubled, increasing from 5.9 per cent of GNP in 1946–1950 to 11.5 per cent in 1966–1970. See James O'Connor, *The Fiscal Crisis of the State* (New York: St. Martins Press, 1973), p. 97.

[3] Norton Long, "Political Science and the City", pp. 253–255.

[4] Wilbur Thompson, *A Preface to Urban Economics* (Baltimore: The Johns Hopkins University Press, 1965), pp. 105–106.

[5] See Matthew Holden, "The Governance of the Metropolis as a Problem in Diplomacy," *Journal of Politics*, 26 (August, 1964), 627–647; and Philip E. Jacob and James V. Toscano, eds., *The Integration of Political Communities* (Philadelphia: Lippincott, 1963).

[6] See Charles M. Tiebout, "A Pure Theory of Local Expenditures," *The Journal of Political Economy*, 64 (October, 1956), 416–424; Vincent Ostrom, Charles Tiebout and Robert Warren, "The Organization of Government in Metropolitan Areas: A Theoretical Inquiry," *American Political Science Review*, 55 (December, 1961), 831–842; Robert Warren, "A Municipal Services Model of Metropolitan Organization," *Journal of the American Institute of Planners*, 30 (August, 1964), 193–204; and Robert L. Bish, *The Public Economy of Metropolitan Areas* (Chicago: Markham, 1971).

[7] Norton Long, "The Local Community as an Ecology of Games," *American Journal of Sociology*, 44 (November, 1958).

[8] Oliver Williams, "Life Style Values and Political Decentralization in Metropolitan Areas," *Social Science Quarterly*, 48 (December, 1967), 299–310. This

At the cost of contributing another image to this conceptual mélange, I would like to argue the benefits of approaching the metropolitan complex as a system of social stratification. A number of central issues in urban politics center upon inequality among social groups. Basic urban problems emanate from the differential access of urban residents to scarce economic, social, and political resources. In order to deal effectively with these issues it is necessary to conduct research within a conceptual framework which explicitly focuses upon the causes and consequences of inequality in the distribution of the surplus product among social groups in the metropolitan community. This framework should generate testable hypotheses as well as provide insight into potential sources of change in the urban system. Such an approach is particularly useful in an investigation of sources of inequality in the distribution of resources among local governments in metropolitan communities.

As Lenski has recently noted, virtually all the major theorists in the field of social stratification have sought to answer one basic question: who gets what and why?[9] In this study urban stratification refers to the institutionalization of social arrangements that generate and perpetuate intergenerational inequality in the distribution of scarce economic, political, and social resources among collectivities in the metropolis.[10] Institutional arrangements connote the principles, procedures, and policies governing and structuring the relationships between groups in the metropolitan community. An urban stratification system consists of a bounded set of individuals, groups, and organizations whose structured interactions culminate in the allocation and distribution of scarce resources among urban residents.

A basic assumption underlying this investigation is that the distribution of symbolic and material advantages among individuals is largely a function of the distribution of power among social groups. Imbedded in the principles, procedures and policies governing and structuring the relationships between classes, and among status groups in the metropolitan community is a "mobilization of bias": a set of institutional rules of the game variously resting upon authority, influence, and force which benefit some at the expense of others.[11]

Intergroup conflict in the metropolis resides in the attempt of group members to gain access to, or control over, those institutions which govern the distribution of symbolic and material advantages.[12] The social relations among classes[13] and among status groups,[14] imbedded in the means of economic production and exchange in the metropolis, structure differential access to income and economic goods and services. The unequal distribution of income and social status among groups fosters an unequal system of social relationships in the urban housing market and local government institutions resulting in differential individual access to housing, neighborhood and "municipal life style." Residential segregation by class and race shapes interaction patterns, friendship ties, marital selection, and social consciousness. The distribution of income and residential location shapes political relationships between collectivities with discordant interests and creates differential access to public goods and services. Access to public goods and services—in particular, education and cultural facilities—controls access to jobs and income.[15] In this manner an interdependent set of local in-

idea is developed somewhat further in Oliver Williams, *Metropolitan Political Analysis: A Social Access Approach* (New York: The Free Press, 1971).

[9] Gerhard Lenski, *Power and Privilege* (New York: McGraw-Hill, 1966), p. 3.

[10] This is a modified version of the definition of stratification provided by John Pease, William H. Form and Joan Huber Rytina, "Ideological Currents in American Stratification Literature," *The American Sociologist*, 5 (May, 1970), 128. It is worth noting that this definition emphasizes the institutional bases of structured inequality among collectivities in contrast to the more "individualistic" opportunity structure approach characterizing much of the current research on social stratification in the United States.

[11] E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart & Winston, 1960), p. 71.

[12] Frank Parkin, *Class Inequality and Political Order* (New York: Praeger Publishers, 1971), p. 25.

[13] According to Weber we may speak of a *class situation* when: "(1) a number of people have in common a specific causal component of their life chances, in so far as (2) this component is represented exclusively by economic interests in the possession of goods and opportunities for income, and (3) is represented under the conditions of the commondity or labor markets." The term *class*, according to Weber, refers to any group of people in the same class situation. See Max Weber, "Class, Status and Party" in *From Max Weber*, Hans Gerth and C. Wright Mills, eds. (New York: Oxford University Press, 1958), p. 182. Within a mature capitalist society there are essentially two *class-based* sources of inequality. First, inequality is associated with the differential ownership and control of property. Second, within the broad structure of property relations, inequality is associated with the division of labor. Differential possession of services offered in the labor market gives rise to inequality associated with occupational positions.

[14] Following Weber, we view status as a mode of domination predicated upon the distribution of social honor among collectivities distinguished from one another by dress, speech, social conventions, values—in essence, by styles of life. See Max Weber, "Class, Status and Party," pp. 186–194.

[15] See Norton Long, "Political Science and the City" for a lucid discussion of the interlocking components of the structure of urban inequality and the failure of political science to fully address the issues stemming from "social politics" in the metropolis, pp. 254–256.

stitutions gives rise to the structure of inequality in the modern metropolis.

From the perspective of urban stratification the metropolitan city is composed of a set of interlocking institutional arrangements and interacting organized interest groups whose relationships form the structural framework within which metropolitan citizens attempt to improve the quality of their lives. Elucidating the nature of these institutions and organized interests and the substance of their relationships is the primary task of urban stratification theory and research.

The Role of Municipal Government. Residential location is an important resource in the urban stratification system. As Anton and Williams have argued:

> Because social values are unevenly distributed across urban space, the location of a housing unit determines as much about the opportunities available to residents as does the sheer physical quality of the unit itself. Depending upon where it is located, a house or an apartment may "carry" with it more or fewer public services, better or worse schools, more or less access to commercial activities [and employment opportunities], more or less interaction with people who are prized, or people who are shunned.[16]

Advantaged classes and status groups in the metropolitan community seek to maximize control over scarce resources and maintain life-style values through homogeneous and complementary residential groupings. In the context of a fragmented system of governments in the metropolis, municipal government becomes an institutional arrangement for promoting and protecting the unequal distribution of scarce resources. As Wood has noted, modern suburbs often use their political boundaries to "differentiate the character of their residents from their neighbors" and wield their government powers—zoning, residential covenants, taxation, selective industrial development—"to promote conscious segregation."[17] Within the suburbs, segregation by class and status has been enhanced as different builders have produced new one-class communities containing houses falling entirely within a narrow price range. Because of the high cost of providing public services for new residents, many communities have made use of zoning and other land development controls to hold down population growth and to price out lower- and middle-income families whose housing and lots would not yield enough in property taxes to cover their service costs. In addition, racial discrimination on the part of builders, real estate brokers, and mortgage institutions has reinforced economic segregation with direct policies of racial exclusion.[18]

Thus, a decentralized, fragmented metropolitan government pattern facilitates the maintenance and perpetuation of class and status group privilege. The more status-homogeneous the suburb, the easier it tends to be, politically, to maintain the primacy of prized values. Municipalities come to be characterized by specialized service packages and recruit residents according to the dictum, "All those who like the kind of life symbolized by these services come and join us, if you can afford it."[19] Conflict over the production and distribution of values which depend upon location for their realization (e.g., land use, education, housing, recreation, cultural facilities) resides at the center of metropolitan politics.[20]

The Roots of Urban Problems. As Fusfeld has argued, fundamental urban problems are deeply rooted in three long-term trends in the urban stratification system: (1) the persistence of poverty and income inequality in metropolitan areas; (2) the persistence of racial discrimination in the fabric of urban institutions; (3) the continuing decentralization of economic and governmental activities away from the urban core into outlying areas of the metropolitan community.[21]

Each basic source of urban problems reflects an underlying struggle among collectivities in the metropolis. The persistence of widespread poverty and income inequality reflects a division between the poor and the relatively affluent: a struggle between the less and the more privileged classes in the metropolitan economic order. The persistence of institutional racism reflects a struggle among racially demarcated status groups over the distribution of commodities, symbols of status, jobs and valued ways of living in the metropolis. Spurred by increases in population and advances in transportation and communication technology, and shaped by government subsidy, the decentral-

[16] Thomas Anton and Oliver P. Williams, "On Comparing Urban Political Systems: Residential Allocations in London and Stockholm," Paper prepared for delivery at the 1971 Annual Meetings of the American Political Science Association, Chicago, Illinois, September 7–11, p. 1.

[17] Robert C. Wood, "Metropolitan Government: 1975: An Extrapolation of Trends," *American Political Science Review*, 52 (March, 1958), 117.

[18] Advisory Commission on Intergovernmental Relations, *Metropolitan America: Challenge to Federalism* (Washington, D.C.: U.S. Government Printing Office, 1966), p. 4.

[19] Oliver P. Williams, "Life-Style Values and Political Decentralization in Metropolitan Areas," in *Community Politics, A Behavioral Approach*, Charles Bonjean, Terry Clark and Robert Lineberry, eds. (New York: The Free Press, 1971), p. 59.

[20] *Ibid.*

[21] Daniel R. Fusfeld, "The Basic Economics of the Urban and Racial Crisis," *Conference Papers of the Union for Radical Political Economics* (December, 1968), pp. 55–66.

ization of economic and governmental activities has given rise to structured conflicts between inner cities and suburbs over the distribution of public goods and services within the metropolitan political order.

With the progressive decentralization of manufacturing, high-wage industries are deserting central cities and and locating beyond the reach of low-income inner city residents. The central city has increasingly become the center for administration, finance, recreation and other types of services, as well as the location for low-wage industries attracted by surplus labor in crowded slums. Decentralization has also had the effect of reducing the financial resources of central cities as capital has progressively followed high-wage industries and the more affluent population to the suburbs.[22] Thus, as the concentration of the low-income working class and the unemployed in the central city and inner ring suburbs has increased, the fiscal resources necessary to meet their needs have progressively drained to the suburbs and the war chest of the federal government.

The structural contradiction between expanding public needs and declining fiscal resources was greatly intensified with the transformation of southern agriculture during the early and mid-'fifties which dispersed millions of agrarian workers to central cities at a time when mass-production industries were automating and not hiring, economic expansion was slowing down, and high-wage growth industries were decentralizing to "lily-white" sanctuaries at the periphery of the metropolitan areas outside the reach of black and low-income families. In the late 'fifties this wave of migration was followed by a population boom among deprived groups in the central cities of older metropolitan areas which further intensified the problems. A rapid growth in social needs in central cities and inner ring suburbs, accompanied by a corresponding drop in employment opportunities and fiscal resources, produced a deterioration of public services in such crucial areas as education, health, housing, and police and fire protection.[23] This contradiction stimulated the outbreak of ghetto disturbances in the middle and late 'sixties.

In recent decades we have witnessed a greater intermingling of all these conflicts as increasingly nonwhite, impoverished inner cities abrasively bump borders with the more advantaged suburban populations. This struggle seems likely to persist, and may intensify, for while the black migration to the cities has slowed down considerably in recent years, the natural increase in population is continuing. The first generation of children born to the migrants of the early 1950s has been starting the second round of the population explosion in the ghettos and impoverished neighborhoods of the central cities. Residential segregation by race appears to have been increasing,[24] while at the same time "there has been little or no break in the oppressive forces of overcrowded and deteriorating housing, bad education, poor transportation facilities, and inadequate employment opportunities."[25]

Thus, governmental inequality occupies a central position in the urban stratification system. Yet even the most casual survey of census data reveals marked variations in the degree of governmental inequality among metropolitan areas in the United States. The rest of this study is devoted to an empirical analysis of sources of variation in the degree of inequality in the distribution of fiscal resources among municipalities in a sample of metropolitan areas.

Methods and Data

Measuring Governmental Inequality. Comparative research on inequality in the distribution of fiscal resources among governments in metropolitan areas involves three basic issues of measurement. First, how are *units of government* to be defined and aggregated? Second, what is the appropriate *indicator of fiscal resources*? Third, what *measure of inequality* is to be employed? These issues are discussed in turn.

Considering the importance of the issue, there has been relatively little comparative research on governmental inequality among metropolitan areas in the United States. The principal exception is the body of research devoted to explaining socioeconomic and fiscal differences among central cities and suburbs in urban areas.[26] In this

[22] David L. Birch, *The Economic Future of City and Suburb* (New York: Committee for Economic Development, 1970).

[23] Fusfeld provides a striking illustration of this process at work in the inner city of Detroit. In the census area surrounding conflict-ridden Twelfth Street, the number of families and unrelated individuals counted by the census fell by about 4,700 between 1950 and 1965. But the number of children of school age increased by 20,900 in the same period. As Fusfeld notes (pp. 55–56): "Even with all the good will in the world, in the absence of vastly increased resources the school authorities could do little but watch the quality of education deteriorate. Unable to provide the funds needed, school officials have presided over the breakdown of the educational system of the central cities. The people who needed the best got the worst."

[24] See Reynolds Farley and Karl E. Taeuber, "Population Trends and Residential Segregation Since 1960," *Science*, 159 (March 1, 1968), 953–956.

[25] Daniel R. Fusfeld, "The Basic Economics of the Urban and Racial Crisis," p. 56. For evidence on these issues for the late 'sixties see *Report of the National Advisory Commission on Civil Disorders* (New York: E. P. Dutton, 1968).

[26] There are essentially two threads in the research on city-suburb socioeconomic differences. One line of research grows out of the attempts of human ecologists to test and build upon Burgess's concentric zone theory

type of investigation suburban characteristics are averaged together and compared to central city characteristics. Level of governmental inequality is measured by the ratio of central city to suburban characteristics and an attempt is made to explain variations in the city-suburb ratio through the comparative analysis of urban areas. While these studies have advanced our knowledge of factors associated with city-suburb disparities in urban areas, they share a weakness in conceptualization and measurement.[27]

An assumption underlying research on city-suburb inequality is that suburbs are basically similar to one another in socioeconomic and fiscal characteristics; or at least sufficiently similar to justify an aggregated suburban comparison with the central city. Yet urban research has amply demonstrated the inaccuracy of the image of suburbia as uniformly white-collar and high income.[28] There have, of course, always been working-class suburbs. But as industry has been leaving the central city for the suburbs, blue-collar workers have been following and working-class suburbs have been multiplying. In a recent poll, it was found that almost half of all union members live in the suburbs, and that "suburbanites account for about three-fourths of unionists under age 40."[29] As an indicator of the increasing *embourgeoisement* of blue-collar workers, however, this statistic can be highly misleading. Working-class suburbanites tend to live in older, low-to-moderate income residential and industrial suburbs on the borders of central cities.[30] Manufacturing decentralizes as suburbs compete for industrial firms to alleviate property tax burdens. Yet few suburbs will permit public or low-cost housing to be built near the factories they entice to locate within their borders. The decentralization of manufacturing into middle-class municipal enclaves is likely to foster the further development of relatively low-income, low-tax-base suburbs in areas adjoining the new industrial sites. This development, in turn is likely to exacerbate income disparities between municipalities in the metropolis.

As the Advisory Commission on Intergovernmental Relations has recently noted,

> Of growing significance are the fiscal disparities among rich and poor suburban communities in many of the metropolitan areas—disparities that often are even more dramatic than those observed between central cities and suburbia in general. Many of the older suburban communities are taking on the physical, social and economic characteristics of the central city. This type of community is especially vulnerable to fiscal distress because it lacks the diversified tax base that has enabled the central city to absorb some of the impact of extraordinary expenditure demands.[31]

Therefore, while the city-suburban disparity approach to metropolitan governmental inequality appropriately emphasizes the frequent plight of central cities, it tends to gloss over an increasingly class- and status-differentiated suburban governmental structure. It seems appropriate to go beyond the central city-suburb dichotomy when conducting research on governmental inequality. What appears to be required is the development of hypotheses and measurement procedures addressed to inequality in the distribution of resources among all municipalities in the metropolitan area.

Turning to the second issue, there are a number of possible indicators of level of fiscal resources—capability to produce public goods and services—none of which seems fully satisfactory. Sources of municipal revenue vary by regions within the nation, by states within regions, by metropolitan areas within states, and by municipalities within metropolitan areas. Local governments frequently derive the major share of their revenue from taxes on mercantile, manufacturing, residential and personal property. Assessed valuations of property, however, are notoriously un-

of urban growth. See Leo F. Schnore, *The Urban Scene* (New York: The Free Press, 1965); and Leo F. Schnore, *Class and Race in Cities and Suburbs* (Chicago: Markham, 1972). The second line of research focuses more explicitly on intergovernmental relations in the metropolis and grows out of the interest of political scientists and others in the growing fiscal disparities between central cities and suburbs. See Advisory Commission on Intergovernmental Relations, *Metropolitan Social and Economic Disparities: Implications for Intergovernmental Relations in Central Cities and Suburbs* (Washington, D.C.: U.S. Government Printing Office, 1965); Advisory Commission on Intergovernmental Relations, *Fiscal Balance in the American Federal System*, vols. 1 and 2 (Washington, D.C.: Government Printing Office, 1967); Alan K. Campbell and Seymour Sacks, *Metropolitan America: Fiscal Patterns and Governmental Systems* (New York: The Free Press, 1967).

[27] In a series of empirical studies Schnore and his colleagues have found that older, larger, more urbanized areas with high levels of manufacturing activity, high percentages of unsound housing in the central city and low rates of annexation activity tend to have higher status populations in the suburbs relative to the central city. See Leo F. Schnore, *The Urban Scene*.

[28] See Bennett Berger, *Working Class Suburb* (Berkeley: University of California Press, 1960); Leo F. Schnore, "Satellites and Suburbs," *Social Forces*, 36 (December, 1957), 121–129; Advisory Commission on Intergovernmental Relations, *Fiscal Balance in the Federal System*, vol. 2, p. 6.

[29] "Where the Votes Are: Unions' Political Arm Joins Trek to the Suburbs," *Wall Street Journal* (August 29, 1967).

[30] For evidence bearing on this point see Richard Hamilton's insightful discussion of the residential location of blue-collar workers in Milwaukee, Wisconsin, in his recent book, *Class and Politics in the United States* (New York: John Wiley, 1972), chapter three.

[31] Advisory Commission on Intergovernmental Relations, *Fiscal Balance in the American Federal System*, volume 2, p. 6.

reliable indicators of the actual level of fiscal resources available to a municipality. Municipalities also derive revenue from sales taxes on commodities, from payroll and personal income taxes, and from other sources.

In this investigation the median level of family income in a municipality is employed as the indicator of level of fiscal resources or capacity to produce public goods and services. As Riew has noted, "When the local government relies principally on property as its tax base, personal income as a fiscal resource may seem to be unimportant. But it may be an important potential tax base. Furthermore, since all taxes are paid out of income, that is ultimately what measures best the ability to pay taxes."[32] Median family income provides a very rough indication of the fiscal capacity and the level of service needs in the municipality and is a crude indicator of the class composition of the local government. In contrast to most indicators of fiscal resources, median income retains roughly the same meaning across regions, states, metropolitan areas, and municipalities.

While there are some extremely homogeneous suburbs, normally municipal boundaries do not operate as sharp demographic, economic, or social dividing lines. As Williams has noted, any municipality can absorb a degree of internal diversity while maintaining a dominant life style. Scanning the range of mean values of a characteristic among municipalities in a metropolitan area quickly establishes the existence of diversity and inequality.[33] The standard deviation is the measure of inequality in the distribution of family income among municipalities in the metropolitan area adopted in this study.[34]

The standard deviation is a measure of variability about the mean of a given characteristic. In this case the standard deviation in median municipal family income for a metropolitan area indicates the extent to which municipalities in the metropolis deviate in median income from the average for all municipalities in the area. For example, a metropolis with a standard deviation in municipal income of zero would be a metropolitan area in which each component municipality has the same median family income. The larger the standard deviation, the greater the deviation of the wealth of some municipalities from the average income of all municipalities in the metropolitan area. Under the assumption that the distribution of median family income over all municipalities in the metropolitan area is "normal" the standard deviation assumes a specific meaning. One standard deviation from the mean encompasses two-thirds of the municipalities in the metropolitan area—one-third of the municipalities above and one-third of the municipalities below the mean. Therefore, a standard deviation of say, $1000, indicates that roughly two-thirds of the municipalities in that metropolitan area deviate from the average municipal median income by $1000 or less. As the numerical size of the standard deviation increases, the extent to which some municipalities have very high incomes and some have very low incomes, relative to the average, increases. The larger the standard deviation, the greater the governmental inequality in the metropolis.

In summary, the measure of inequality in the distribution of fiscal resources among governments in the metropolis is the *standard deviation in median family income among municipalities in the metropolitan area.* This is a measure of inequality among municipalities treated as corporate units and is not a measure of inequality among the total population of individuals residing within those municipalities in the metropolitan area. It should be further noted that median family income only captures part of the real tax base of municipalities. Because it fails to take account of industrial and commercial property values directly, this measure probably understates the severity of governmental inequality in the metropolis.

The Data. The following procedure was adopted to construct a measure of municipal income inequality in the metropolis. Income data were collected on all municipalities with *populations of 2500 and above* in standard metropolitan statistical areas (SMSAs) in the United States in 1960. The SMSA includes a central city or cities of 50,000 population or more; the county in which the city is located; and adjoining counties which by various social and economic criteria reflect a dependence on the central city or cities.

The population census provides income data only for municipalities of population size of 2500 and above. This was an important constraint on the data collection and a caveat to ponder in interpreting these findings. Under ideal

[32] John Riew, "Fiscal Disparities in the Milwaukee Area," in *Fiscal Balance in the American Federal System*, vol. 2, p. 296.

[33] Williams, "Life Style Values and Political Decentralization", p. 62.

[34] The standard deviation consists of the square root of the average squared deviations from the mean of a frequency distribution. As a measure of inequality it suffers from one basic weakness; it is unusually sensitive to values lying at the extremes of a frequency distribution so that the presence of a few extreme values may have a somewhat disproportionate impact on the size of the standard deviation. See Howard R. Alker, Jr., and Bruce M. Russett, "Indices for Comparing Inequality", in *Comparing Nations*, Howard R. Alker, Jr. and Bruce M. Russett, eds. (New Haven: Yale University Press, 1966), pp. 356–357.

conditions income data should have been collected on all incorporated municipalities for each SMSA. Although one-half of the municipalities in metropolitan areas have populations under 2500, these municipalities account for only 3 per cent of the total metropolitan population.[35] Yet this is an important 3 per cent of the population to the extent that it includes a disproportionate share of the very wealthy and/or the very poor members of the metropolitan area. For the purposes of this analysis, it is assumed that this measure of municipal income inequality based upon municipalities with populations of 2500 or above is an accurate reflection of income inequality among all municipalities in the metropolitan areas in this study.

For each metropolitan area, data on median family income were coded for each component municipality of population size 2500 and above. Then for each metropolitan area containing *three or more* municipalities of 2500 population and larger, a standard deviation was computed from the component municipal income data.[36] Metropolitan areas in the New England states of Maine, New Hampshire, Vermont, Massachusetts, Connecticut, and Rhode Island were not included in this analysis, however. The pattern of local government in New England is one of towns and townships rather than municipalities and counties. The population census provides data only on those New England towns with populations of 10,000 and above. Since the governmental structure and available data for New England metropolitan areas is not strictly comparable with the metropolitan areas in other regions of the country, New England SMASs were omitted from this analysis.

In summary, our population of metropolitan areas consists of SMSAs outside of New England with three or more municipalities containing populations of 2500 and above in 1960. In 1960 there were 66 metropolitan areas with less than the required number of municipalities of appropriate size. Eleven New England SMSAs were omitted from the analysis and eight metropolitan areas were dropped for lack of sufficient data. This left a population of 127 metropolitan areas as a base for analysis.

[35] John C. Bollens and Henry Schmandt, *The Metropolis* (New York: Harper and Row, 1970), p. 104.

[36] The following sources of data were utilized in constructing the standard deviation: (a) the list of names of all municipalities with populations of 2500 and above in metropolitan areas is provided in the *1962 Census of Governments*, Table 14 (Washington, D.C.: Government Printing Office); (b) the income data for each municipality were coded from the *1960 Census of Population, vol. 1: Characteristics of Population*, Tables 32–34 (Washington, D.C.: Government Printing Office).

Because the independent variables analyzed in this study were drawn from a wide variety of sources, there are rather marked fluctuations in the number of cases underlying various analyses of specific empirical relationships. For this reason, this study is best viewed as an exploratory investigation of an important political phenomenon. If the findings from this investigation prove suggestive, more refined research efforts will be in order.

Table 1 presents the mean, standard deviation, and range in governmental income inequality among the 127 metropolitan areas in this study. The standard deviation in municipal income averages $920 among the 127 metropolitan areas in this investigation. The SMSA with the least inequality had a standard deviation in municipal family income of $14. The metropolitan area with the greatest governmental income inequality had a standard deviation of $3128. There is a vast range in inequality in the distribution of income among municipalities in this sample.

What are some of the factors associated with variations in governmental inequality among this sample of metropolitan areas? The remainder of this study is organized according to a sequence of empirical findings presented in the form of empirical propositions, each with a brief rationale. We conclude with a discussion of the relationship between the empirical findings and the conceptual framework prefacing this analysis.

Hypotheses and Findings

(1) *Metropolitan areas in the South tend to have greater inequality among municipalities than metropolitan areas located in other regions of the country.* A number of studies have revealed that economic and social inequality in metropolitan areas varies markedly between the northern and southern United States. If our argument is correct, family income inequality and racial discrimination are two of the principal factors fostering

Table 1: Mean, Standard Deviation and Range in Municipal Income Inequality for 127 SMSAs, 1960

	Municipal Inequality[a]
Mean	$ 920
Standard Deviation	583
Range:	
Minima	14
Maxima	3128
Number of SMSAs	127

[a] Standard deviation in median municipal family income computed over all municipalities with populations of 2500 and above in the metropolitan area.

Table 2. Zero-Order Correlations Between Regional and Demographic Characteristics and Municipal Income Inequality for 127 SMSAs, 1960

	Municipal Inequality[a]
Region:	
1. Metropolitan presence in the South[b]	.132
Demographic Characteristics:	
1. Population size of SMSA	.392
2. Population density of SMSA: number of persons per square mile	.124
3. Per cent of SMSA population residing in urbanized area	.267
4. Age of SMSA: year central city reached 25,000 population	−.230

[a] Standard deviation in median municipal family income computed over all municipalities with populations of 2500 and above in the metropolitan area.

[b] 0=absence, 1=presence, of a metropolitan area in the South.

governmental inequality in the metropolis. Southern metropolitan areas tend to have higher levels of income inequality among families[37] and among racial groups than northern metropolises.[38] Southern metropolitan areas also tend to have more marked residential segregation by race[39] and social class.[40] There is reason to suspect, therefore, that southern metropolitan areas may display higher levels of governmental inequality as well.

The evidence presented in Table 2 gives a small measure of support to these speculations. The zero-order correlation between the dummy variable, absence or presence of a metropolitan area in the South,[41] and municipal income inequality, is $r=.132$. It would seem that governmental inequality is less strongly related to the historical and cultural features imbedded in the region concept than at first surmised.

(2) *Older, larger, and more densely populated metropolitan areas tend to have greater inequality among municipalities.* Williams has suggested that the degree of differentiation among municipalities increases with age and size of the metropolitan area. First, neighborhood and municipal identities become crystallized over time. Moves are made with a greater awareness of the life style of each place. In younger, expanding metropolitan areas, growth may take place so rapidly that the visibility of the life style remains low and the local political structures are not sufficiently stable to employ effective screening policies over entering residents. Second, suburban specialization in a small metropolitan area is likely to be rudimentary. Larger metropolitan areas are likely to contain many gradations of class- and status-homogeneous residential suburbs as well as suburbs with highly specialized economic bases.[42] Ecological research and theory seem to imply much the same hypothesis. From this perspective, increases in population size and density foster increased competition for scarce resources, resulting in increases in the division of labor as individuals seek new means of sustenance. As specialization increases, spatial segregation among individuals with similar social traits becomes a mechanism of social integration in a highly complex urban world.[43]

Data presented in Table 2 indicate that there is a tendency for older ($r=.230$), larger ($r=.392$), more urbanized ($r=.267$) and more densely populated ($r=.124$) metropolitan areas to have higher levels of inequality among municipal governments. On balance the evidence seems to support the ecological hypothesis that population size, age and density provide a demographic potential for increased inequality among governments in the metropolitan community.

(3) *The larger the relative size of the nonwhite population in the metropolitan community, the greater the inequality among governments.* It has been suggested that status rivalry operates along with purchasing power to structure the residential location and segregation of residents in the metropolitan city. Since members of nonwhite minorities have disproportionately lower income,[44] and since they are frequently denied access to neighborhoods and housing they could otherwise afford because of their racial status,[45] we would expect

[37] Richard Child Hill, "Urban Income Inequality" (Ph.D. dissertation, Department of Sociology, University of Wisconsin, Madison, 1973).

[38] Robert Jiobu and Harvey Marshall, "Urban Structure and the Differentiation between Blacks and Whites," *American Sociological Review*, 36 (August, 1971), 638–649.

[39] Karl Taeuber and Alma Taeuber, *Negroes in Cities* (Chicago: Aldine Publishers, 1965), pp. 39–41.

[40] Richard Child Hill, "Residential Segregation in the Metropolis: The Ecology of Class" (unpublished manuscript, Department of Sociology, Michigan State University, East Lansing, 1973).

[41] In this analysis the South includes metropolitan areas in the following states: Texas, Oklahoma, Kansas, Missouri, Arkansas, Louisiana, Alabama, Mississippi, Florida, Georgia, North Carolina, South Carolina, Virginia, West Virginia, Kentucky, and Tennessee.

[42] Williams, "Life Style Values and Political Decentralization," p. 62.

[43] See Emile Durkheim, *The Division of Labor* (Glencoe: The Free Press, 1958); and Louis Wirth, "Urbanism as a Way of Life", *American Journal of Sociology*, 44 (July, 1938), 11.

[44] See U.S. Department of Commerce, *The Social and Economic Status of Negroes in the United States, 1970* (Washington D.C.: Government Printing Office, 1971), pp. 36–37; and Herman Miller, *Rich Man Poor Man* (New York: Thomas Y. Crowell, 1971), pp. 75–79.

[45] See William H. Brown Jr., "Access to Housing: The Role of the Real Estate Industry," *Economic Ge-*

that as the relative size of the nonwhite population increases, governmental inequality in the metropolis will increase.

This hypothesis is supported by the data presented in Table 3. Governmental inequality varies directly with the nonwhite percentage of the metropolitan population ($r=.206$) and the nonwhite percentage of the central city ($r=.352$). It would appear that status group exclusion should be added to the distribution of purchasing power as a basic element of the set of forces fostering inequality in fiscal capacity among municipal governments in the metropolis.

(4) *The greater the inequality in the distribution of income among families in the metropolis, the greater the inequality among municipalities.* As Thompson has argued, the unequal distribution of fiscal capacity among governments is rooted in the unequal distribution of income among families in the metropolis. Family income inequality is translated through the dual mechanisms of residential segregation and political incorporation into inequality in the distribution of wealth, and hence, the capacity to produce public goods and services, among governments in the metropolis.[46] Data provided in Table 3 offer support for this argument.

Among the sixty-three metropolitan areas for which we have combined income and government data,[47] we find that inequality among municipalities is directly related to family income inequality ($r=.234$) in the metropolitan community. At the same time, the size and level of income of the upper-income groups in the metropolis appear to be even more important for government inequality. The larger the percentage of families with incomes of $10,000 a year and above, the greater the inequality among municipalities ($r=.419$). Similarly, as the average median earnings of professional and manager occupations increases, governmental inequality increases ($r=.419$). On the other hand, there is essentially no relationship between the level of poverty, or the median earnings of blue-collar workers, and governmental inequality in the metropolis. It would appear that as the size and wealth of the upper income groups rises, the structural potential for the development of upper-status political enclaves within the metropolitan area increases.

Table 3. Zero-Order Correlations Between Racial Composition, SMSA Income Characteristics, and Municipal Income Inequality in SMSAs, 1960

	Municipal Inequality[a]
Racial Composition:[b]	
1. Per cent of SMSA population, nonwhite	.206
2. Per cent of central city, nonwhite	.352
Income[c]	
1. Median family income	.239
2. Per cent of families with incomes at or below $3,000 a year	−.043
3. Per cent of families with incomes at or above $10,000 a year	.419
Median Earnings:[c]	
1. Professionals and Managers	.419
2. Craftsmen and foremen	.202
3. Operatives	.030
4. Laborers	.085
Income Inequality:[c]	
1. Gini coefficient: family income	.234
2. Racial income inequality[d]	.126

[a] Standard deviation in median municipal family income computed over all municipalities with populations of 2500 and above in the metropolitan area.

[b] $N=127$.

[c] $N=63$.

[d] Median nonwhite family income divided by median white family income. Reflected to read the higher the score, the greater the inequality.

(5) *The greater the residential segregation by social class in the metropolis, the greater the inequality among municipalities.* Income is unequally distributed among social classes. As residential segregation by social class increases, inequality in the distribution of income by neighborhood increases. When class and status homogeneous neighborhoods come to coincide with municipal boundaries, inequality in the distribution of fiscal capacity among municipal governments arises.

(6) *The more segregated the distribution of housing by quality in the metropolis, the greater the inequality among municipalities.* The distribution of social classes by residential area also depends upon the distribution of housing by quality in urban territory. If all housing in the metropolitan area were of the same quality and price, and assuming this price was in reach of even the lowest income groups, the residential distribution of classes would not be mediated by ability to pay for housing. Or, if low, medium, and high priced housing were randomly distributed among neighborhoods in the metropolis, and if the qual-

ography 48 (January, 1972), 66–67; and Rose Helper, *Racial Policies and Practices of Real Estate Brokers* (Minneapolis: University of Minnesota Press, 1969), pp. 277–301.

[46] Thompson, *A Preface to Urban Economics*, pp. 105–106.

[47] In essence these are the metropolitan areas in 1960 with five or more municipalities with populations of 2500 and above, outside of New England, with metropolitan populations in excess of one quarter of a million people.

ity of a family's housing were directly related to level of family purchasing power, we would expect that income would be fairly equally distributed among neighborhoods and local governments in the metropolitan community. Therefore, it seems reasonable to expect that the greater the segregation of housing by quality, the greater the government inequality in the metropolis.

Avery M. Guest, in a recent study of patterns of urban ecological organization, collected 1960 data from the census of housing on a variety of social characteristics of families by census tracts for seventeen metropolitan areas.[48] For each metropolitan area, Guest coded data on the proportion of families with white-collar heads of household and the proportion of sound dwelling units with all plumbing installed in each census tract. From this detailed information on census tracts, he computed the standard deviation in percentage white-collar and percentage of sound housing across census tracts for each metropolitan area. The greater the standard deviation, the greater the segregation. Combining Guest's data with our own yields fourteen metropolitan areas for which joint data are available.

As revealed in Table 4, the residential segregation of white-collar families is directly related to governmental inequality ($r = .356$) as is the segregation of sound housing ($r = .452$) in the metropolitan community. While findings from such a small sample of cases can hardly be more than suggestive, the evidence does support our expectations.[49]

(7) *The larger the number of municipal governments in the metropolitan community, the greater the inequality among municipalities.* Suburban governments tend to function as corporate representatives of the class and status groups interests of their resident populations.[50] Through political devices like zoning ordinances, selective industrial development programs, and building regulations, suburban enclaves that are homogeneous in class and status pose barriers to social and economic "undesirables" and maintain control over the composition of their incorporate fiefdoms.[51] The degree of municipal fragmentation would appear to indicate the governmental potential for fiscal inequality among municipalities in the metropolis.

Data presented in Table 4 support these speculations. The concept of government fragmentation seems to refer to both the absolute number of governments and the density of governments in a metropolitan area.[52] The data indicate that metropolitan areas with large numbers of municipalities tend to have greater inequality among municipalities ($r = .359$).[53] On the other hand, the density of governments, indicated by the number of governments per capita, is inversely related to governmental inequality ($r = -.212$). However, as noted below, when other salient variables are held constant, governmental density is positively related to inequality among municipalities in the metropolis.

Table 4. Zero-Order Correlations Between Class Segregation, Housing Segregation, Number and Density of Governments, and Municipal Income Inequality in SMSAs, 1960

	Municipal Inequality[a]
Class Segregation:[b]	
1. Standard deviation: per cent white collar[c]	.356
Housing Segregation:[b]	
1. Standard deviation: per cent housing sound with all plumbing[c]	.452
Government Fragmentation:[d]	
1. Number of municipalities in SMSA	.359
2. Number of municipalities per capita in SMSA	−.212

[a] Standard deviation in median municipal family income computed over all municipalities with populations of 2500 and above in the metropolitan area.
[b] $N = 17$.
[c] Computed over all census tracts in the SMSA.
[d] $N = 127$.

A Multivariate Analysis. Thus far we have surveyed a number of empirical hypotheses and partial explanations of variations in municipal in-

[48] See Avery M. Guest, "Retesting the Burgess Zonal Hypothesis: The Location of White Collar Workers," *American Journal of Sociology*, 76 (May, 1971), 1094–1108.

[49] In research not reported here I found that as white-collar groups and sound housing becomes more decentralized—as larger proportions of white-collar groups and sound housing become located at increasing distances from the center city—inequality among municipalities tends to increase.

[50] Williams, "Life Style Values and Political Decentralization," pp. 58–59.

[51] Morton Grodzins, *The Metropolitan Area as a Racial Problem* (Pittsburgh: University of Pittsburgh Press, 1958), pp. 7–9.

[52] Number of governments and density of governments are quite different properties of the government structure of metropolitan areas. The concept of government fragmentation requires elaboration and refinement to take account of it's several component dimensions.

[53] The same pattern of correlations exists for other forms of government in the metropolis. Number of school districts ($r = .261$), number of special districts ($r = .171$) and number of governments of all types ($r = .228$) are all positively associated with inequality among municipalities in the metropolitan community.

equality in fiscal capacity among metropolitan areas. A number of questions naturally arise. To what extent is the impact of any particular independent variable independent of its relationship to other independent variables under discussion? What is the combined predictive power of the independent variables? Multiple correlation and regression analysis help provide answers to these questions. Joint data are available on all of the variables, except class and housing segregation, for sixty-three metropolitan areas—the data base for the regression analysis.

Results of the regression analysis are presented in Table 5. With the exception of region and number and density of municipalities, the variables in the regression model retain the predicted relationships to municipal inequality. The relationship between metropolitan presence in the South and government inequality reverses direction when other salient variables are held constant. Number of municipalities has little predictive power independent of other variables in the model, and governmental density becomes positively associated with inequality among municipalities ($rp = .317$).

The relative size of the nonwhite population has the strongest independent influence on government inequality ($rp = .511$), followed by per cent of families with incomes of $10,000 a year and above ($rp = .420$), number of municipalities per capita ($rp = .317$) and family income inequality ($rp = .277$). Overall, the variables in this model account for a not insignificant 58 per cent of the total variation in inequality among municipalities in the metropolis.

Summary and Conclusions

The political incorporation and municipal segregation of classes and status groups in the metropolis tend to divorce fiscal resources from public needs and serve to create and perpetuate inequality among urban residents in the United States. Williams has suggested that much of urban politics consists of conflict over the production and distribution of scarce resources, like education and housing, which depend upon location for their realization.[84] And, as Long has noted, if the increased demand among urban residents for public goods and services continues in the future, as it has in the past two decades, the governmental allocation of values may become as important as the private marketplace.[85] Thus, inequality in the distribution of fiscal resources among municipal governments in metropolitan areas occupies an increasingly important role in the urban stratification system.

Considering the importance of the issue, there has been relatively little comparative research on governmental inequality among metropolitan areas in the United States. Past research on governmental inequality has largely been devoted to the investigation of city-suburban differences and has directed little empirical attention to an increasingly class and status differentiated suburban government structure. With this in mind, this study has measured inequality in the distribution of fiscal resources (capacity to generate public goods and services) among governments by the standard deviation in median family income among municipalities in the metropolitan area.

This exploratory investigation of data collected for a large number of metropolitan areas in 1960 revealed a number of variables associated with

[84] Williams, "Life Style Values and Political Decentralization," p. 58.

[85] Long "Political Science and the City," pp. 255–256.

Table 5. Multiple Regression of Municipal Income Inequality on Salient Independent Variables for 63 Metropolitan Areas, 1960

	Municipal Inequality[a]		
	Partial Correlation	Beta Weight[b]	Level of Significance[c]
1. South	−.268	−.313	<.044
2. Population size of SMSA	.146	.167	<.280
3. Per cent of families with incomes at or above $10,000 a year	.420	.466	<.001
4. Gini coefficient: family income	.277	.289	<.037
5. Per cent of SMSA population, nonwhite	.511	.601	<.001
6. Number of municipalities	.054	.068	<.690
7. Number of municipalities per capita	.317	.342	<.016
$R = .761$ $R^2 = .579$ $N = 63$			

[a] Standard deviation in median municipal family income computed over all municipalities with populations of 2500 and above in the metropolitan area.

[b] Standardized regression coefficient.

[c] Level of statistical significance of the regression coefficient using the *t*-test, two-tailed.

inequality among municipalities in the metropolis. The empirical findings appear to be consistent with the urban stratification framework outlined in the introductory discussion. First of all, the data support the thesis that governmental inequality is rooted in income inequality among families in the metropolis. Family income inequality is translated through the dual mechanisms of residential segregation and political incorporation into inequality in the distribution of fiscal resources among municipal governments in the metropolis. Thus we find that inequality among municipalities varies directly with level of family income inequality, the residential segregation of social classes, housing segregation by quality, and government fragmentation in the metropolis. Our second thesis—that racial discrimination is a basic component of the urban stratification system and underlies governmental inequality—is consistent with the data. Inequality in the distribution of fiscal resources among municipalities varies directly with level of nonwhite concentration in the metropolis indicating status group exclusion operating in the urban housing arena and in the policies of local governments. Finally, consistent with Williams's ideas,[56] inequality tends to be higher in older, larger and more densely populated metropolitan areas which indicate a demographic potential for municipal differentiation in urban space.

By way of conclusion, a number of caveats are in order. First of all, because the independent variables analyzed in this study were drawn from a wide variety of primary and secondary sources, there are marked fluctuations in the number of cases underlying various analyses of specific empirical relationships. As a result, it is not possible to generalize from these findings to all metropolitan areas in the United States. Second, the measure of level of fiscal resource and median municipal family income, only captures a part of the phenomenon in question, and needs to be supplemented with other indicators of the capacity to produce public goods and services. Finally, the standard deviation as a measure of inequality is open to the criticism of being highly sensitive to values lying at the extremes of a frequency distribution. Future research needs to consider alternative measurement procedures. On the positive side, it is worth noting that the empirical results revealed in this study are largely consistent with theoretical expectations. Further research is required to establish the generality of the findings and the accuracy of the interpretations offered here.

[56] Williams, "Life Style Values and Political Decentralization," p. 62.

Conflict, Power and Relative Deprivation*

WALTER KORPI
University of Stockholm

During recent decades, power and differences in power resources between parties have often been neglected in the study of conflict.[1] In a review of twelve years of conflict research, Converse[2] observes "that power frequently is handled as a factor to be dropped out by assuming that two conflicting parties are roughly equal in 'power.'" Instead, explanations of conflict in terms of motivation, i.e., in terms of the subjective states and desires of the parties in conflict, have gained considerable vogue among social scientists.[3] According to this theoretical approach, conflicts like revolutions and collective violence are primarily responses to an intolerable gap between normative expectations and actual achievements created by cumulated experiences of hardship in a collectivity, i.e., as a result of an increase in relative deprivation.[4]

This expectation achievement approach to conflict theory is obviously insufficient to the extent that it puts exclusive emphasis on the motivation or wish for improvement among the potential participants as the central factor in conflict and overlooks the actual possibilities for achieving the desired change, the latter being determined primarily by the differences in power resources between the parties concerned. In many situations where the desire for change in a collectivity increases with increasing relative deprivation or hardship, the actual possibilities for alleviating the situation may at the same time be decreasing as a result of the changing balance of power among the parties.

The expectation achievement approach to conflict has recently been attacked on theoretical as well as on empirical grounds. Its critics have proposed power resources, the mobilization of power resources, and the struggle for power as the central features in an alternative approach to conflict theory and in explanations of conflicts like revolutions and collective violence.[5] In this alternative political process approach, manifest conflicts like collective violence or revolutions are seen as closely related to and produced by the central political process, in which a collectivity within a population makes claims for certain privileges and this claim is resisted by another collectivity. The process of acquiring control over power resources is seen as a necessary condition for the capacity to contend for privileges. Conflicts are assumed to be most frequent in periods

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[1] For exceptions see Hubert M. Blalock, *Toward a Theory of Minority Group Relations* (New York: Wiley, 1967); Lewis Coser, *The Functions of Social Conflict* (London: Routledge & Kegan Paul, 1956) and *Continuities in the Study of Social Conflict* (New York: Free Press, 1967); Ralf Dahrendorf, *Class and Class Conflict in Industrial Society* (Stanford: Stanford University Press, 1959); William A. Gamson, *Power and Discontent* (Homewood, Ill.: The Dorsey Press, 1968); Allen D. Grimshaw, "Interpreting Collective Violence: An Argument for the Importance of Social Structure", *The Annals of the American Academy of Political and Social Science*, 391 (September, 1970), 9–20; John Rex, *Key Problems of Sociological Theory* (London: Routledge & Kegan Paul, 1961); and Herman Schmid "Peace Research and Politics," *The Journal of Peace Research*, 5, No. 3 (1968).

[2] Elizabeth Converse, p. 489 in "The War of All Against All: A Review of the Journal of Conflict Resolution, 1957–1968", *Journal of Conflict Resolution*, 12 (December, 1968), 471–532.

[3] Another theoretical approach has been based on the concept of "strain," see Neil J. Smelser, *Theory of Collective Behavior* (New York: Free Press, 1963). For critiques of this approach cf. Elliot Currie and Jerome H. Skolnick, "A Critical Note on Conceptions of Collective Violence", *The Annals of the American Academy of Political and Social Science*, 391 (September, 1970), 34–45, and Anthony Oberschall, *Social Conflict and Social Movements* (Englewood Cliffs: Prentice-Hall, 1973), pp. 19–24.

[4] Sources on this approach include James C. Davies, "Toward a Theory of Revolution", *American Sociological Review*, 27 (February, 1962), 5–18; "The J-curve of Rising and Declining Satisfactions as a Cause of Some Great Revolutions and a Contained Rebellion" and Ivo K. Feierabend, Rosalind L. Feierabend and Betty A. Nesvold, "Social Change and Political Violence: Cross-National Patterns" in *Violence in America: Historical and Comparative Perspectives*, ed. Hugh D. Graham and Ted R. Gurr (Washington: U.S. Government Printing Office, 1969); Ted R. Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970).

[5] Presentations and discussions of this theoretical approach can be found in David Snyder and Charles Tilly, "Hardship and Collective Violence in France, 1830 to 1960", *American Sociological Review*, 37 (October, 1972), 520–532; Charles Tilly "Does Modernization Breed Revolution?", *Comparative Politics*, 5 (April, 1973), 425–447; Anthony Oberschall, *Social Conflict and Social Movements* and Abdul Q. Lodhi and Charles Tilly, "Urbanization, Crime and Collective Violence in 19th-Century France", *American Journal of Sociology*, 79 (September, 1973), 296–318.

when parties acquire or lose control over power resources.

The proponents of the political process approach tend summarily to reject motivational concepts like relative deprivation in theories of conflicts as being theoretically unfruitful or empirically unsupported.[6] I am in basic agreement with the proponents of the political process model of conflict on the primacy of power resources and the mobilization of power resources in the explanation of conflicts like collective violence and revolutions. Yet, it seems worthwhile to analyze the role of relative deprivation in the development of conflict. This is because any theory of conflict that focuses on mobilization of power resources will at least implicitly have to come to grips with motivational concepts like relative deprivation, since motivational factors are generally accorded a central place in theories of mobilization.[7] An analysis of the role of relative deprivation in the development of conflict can also help to clarify in what respects the expectation achievement approach to conflict is inadequate. More fruitful theories of conflict can be constructed if the notion of relative deprivation is incorporated into models of conflict, where differences in power resources and mobilization of power resources play the central role.

The purpose of the present paper is to develop a model of conflict which incorporates the central concepts from both the expectation achievement approach and the political process approach. In this model, which might be called a power balance model of conflict, relative deprivation as well as differences in power resources between the parties are thus included. The difference in power resources between parties is used as the central independent variable. Utility of reaching the goal, expectancy of success and relative deprivation are introduced as intervening variables to relate the effects from changes in the balance of power between the parties to the probability of manifest conflict between them.

Background, Concepts and Some Assumptions

The conflict situations that are of central concern here are those which in one way or another relate to the struggle over how the surplus in society is to be distributed among different parties, i.e., how goods and services which are not necessary for keeping producers alive and productive are to be distributed in the society.[8] The basic view taken here is that conflicts of this type are essentially bargaining situations.[9] In such bargaining situations the strategy of conflict adopted by the parties generally involves the use of rewards as well as of punishments over a period of time. This implies that conflict should be analyzed as interaction over time. Both short-term and long-term effects of changes in the balance of power between the parties need to be considered.

The term conflict here refers to manifest conflict, i.e., to interaction between parties with incompatible interests[10] which primarily entails punishments. For conflict to occur, *both* parties have to resort to the use of punishments. If one party turns the other cheek or returns good for bad, no conflict becomes manifest in the present sense of the word. Conflict need not, however, involve violence. The parties or actors entering into conflict can be individuals or collectivities. Actors are assumed to be rational and to be satisficing rather than maximizing.[11]

For simplicity the analysis here is limited to conflict situations with only two parties. This is of course a simplification since conflicts between two parties often are affected by third parties and other environmental factors. The introduction of third parties would, however, bring up two so far unreconciled theoretical traditions in conflict theory. According to the "crisscross" or "cross-cutting cleavage" theories, introduction of a third party may decrease the probability of conflict.[12] Another contemporary theoretical tradition, "coalition theory," deals with the process by which three or more actors combine into two opposing camps in the conflict situation.[13] Since for the present purposes it is not necessary to attempt to connect them, third parties will be left outside the discussion.[14]

[6] See Oberschall, pp. 37–38 and Snyder and Tilly.

[7] See John W. Atkinson, *An Introduction to Motivation* (Princeton: Van Nostrand, 1964) and Blalock, *Toward a Theory of Minority Group Relations*, chap. 4. One should also remember that the classical concept related to mobilization in conflict, the class consciousness of the working class, includes relative deprivation as an integral part. See John C. Leggett, *Class, Race and Labor* (New York: Oxford University Press, 1968), pp. 34–42.

[8] See Gerhard E. Lenski, *Power and Privilege* (New York: McGraw-Hill, 1966), chap. 3.

[9] For similar conceptions see Thomas C. Schelling, *The Strategy of Conflict* (London: Oxford University Press, 1960), pp. 4–5; Rex, *Key Problems of Sociological Theory*, p. 123 and Martin Patchen, "Models of Cooperation and Conflict: A Critical Review", *The Journal of Conflict Resolution*, 14 (September, 1970), 389–408.

[10] See Robert Axelrod, *Conflict of Interest* (Chicago: Markham, 1970); Lars Bergström, "What is a Conflict of Interest," *Journal of Peace Research* 7, No. 3 (1970), 197–218; Clinton F. Fink, "Some Conceptual Difficulties in the Theory of Social Conflict", *Journal of Conflict Resolution*, 12 (Dec., 1968), 412–460.

[11] See Herbert A. Simon, *Models of Man* (New York: Wiley, 1957), pp. 196–206.

[12] For treatments and contributions to this theory see Coser, *The Functions of Social Conflict;* James S. Coleman, *Community Conflict* (Glencoe, Ill.: Free Press, 1957); Dahrendorf, *Class and Class Conflict in Industrial Society;* and Johan Galtung, "Rank and Social

Viewing conflicts as a part of bargaining situations suggests the possibility of carrying out the analysis within the framework of an exchange approach and of utilizing some of the concepts from exchange theory. The parties entering into exchange or conflict must, however, not be assumed to have equal access to power resources, an assumption which is often made in exchange analysis.[15] Instead the difference in power resources is used as the central independent variable in the present analysis. Power resources are the properties of an actor which provide the ability to reward or punish another actor.[16]

Exchange between two actors can be characterized by a going rate of exchange according to which the transactions of the parties are executed.[17] Each actor is also assumed to evaluate the outcomes (the going rate of exchange) in the relationship according to his normative expectations (level of aspiration), which indicates what outcomes in the relationship he evaluates positively and considers to be fair and just. An actor's aspiration level in a particular exchange relationship is assumed to be determined by the outcomes known to him by personal or by vicarious experiences in similar relationships.[18] The difference between the level of aspiration and the going rate of exchange of an actor defines the degree of relative deprivation he experiences in the relationship.[19]

We will assume further that in the long run the going rate of exchange is determined primarily by the difference or balance in power resources between the parties. This implies that also the availability of power resources other than those directly concerned in the exchange can affect exchange rates.[20] The going rate of exchange is usually arrived at through some kind of explicit or implicit bargaining between the parties. If bargaining fails to bring the going rates close enough to the aspiration level of an actor, exchange may still take place, but the result might also be a termination of the relationship or a conflict.[21] In the latter case the use of punishments can be viewed as an attempt by one of the parties to change the going rate of exchange. Conflict is thus the continuation of bargaining by other means. As in the political process approach, conflict here becomes a natural byproduct of the bargaining processes and of attempts to alter exchange rates.

Since the outcomes in exchange relationships known to an actor through personal or vicarious experiences have resulted from the prevailing balance of power in these relationships, we can assume that through a process of learning the actors will also have their aspiration levels strongly influenced by the perceived difference in power resources between them. In the long run the weaker actor will, through internal psychological processes, tend to adjust his aspiration level towards the going rates of exchange in the relationship.[22] We can also assume that the stronger actor will use his power to "engineer consent" and to influence the weaker party to accept as legitimate the going rate of exchange

Integration" in *Sociological Theories in Progress*, ed. Joseph Berger, Morris Zelditch, Jr. and Bo Anderson (Boston: Houghton Mifflin, 1966).

[13] See Theodore Caplow, *Two Against One: Coalitions in Triads* (Englewood Cliffs: Prentice-Hall, 1968) and William A. Gamson, "A Theory of Coalition Formation", *American Sociological Review*, 36 (June 1961), 373–382.

[14] Some points of departure for such a connection and comparison can be suggested, however. The basic idea in the cross-cutting cleavage theories is that if the possible lines of cleavage in a society are not superimposed but rather cut across parties, the probability of manifest conflict will decrease and, "society is sewn together by its inner conflicts." This theory thus apparently assumes that each party is internally heterogeneous and has internally opposed interests. Therefore mobilization of power resources incurs costs which makes coalition formation and conflict less likely. In coalition theory on the other hand, it is apparently assumed that parties are homogenous with respect to interests. Mobilization of power resources is therefore easy and does not incur high costs, something which facilitates coalition formation and conflict.

[15] For an exception see Edmund Dahlström, "Exchange, Influence and Power", *Acta Sociologica*, 9, No. 3–4 (1966), 237–284. Even in exchange analyses where inequalities in power resources are recognized, the consequences of such inequalities are often not assumed to be crucial for the result of the exchange, see George C. Homans, *Social Behavior: Its Elementary Forms* (New York: Harcourt, Brace & World, 1961), pp. 77–78 and Peter M. Blau, *Power and Exchange in Social Life* (New York: Wiley, 1964), pp. 157–158.

[16] See Blalock, *Toward a Theory of Minority Group Relations*, pp. 118–119.

[17] See Blau, pp. 151 ff.

[18] For parallel assumptions see John W. Thibaut and Harold H. Kelley, *The Social Psychology of Groups* (New York: Wiley, 1959), pp. 81–83.

[19] This paper adheres to the sociological convention of defining relative deprivation in terms of the perceived discrepancy between on the one hand the actual position or state of the actor with respect to some good or value dimension, and on the other hand the level of aspiration or the normative expectations of the actor with respect to the dimension, i.e., what he believes he is justifiably entitled to. Gurr (pp. 23–30) defines the concept somewhat differently to include both present and future connotations. It would appear, however, that for the present discussion this difference in definitions is not crucial.

[20] This assumption is similar to the proposition by Lenski, *Power and Privilege*, p. 44, that "power will determine the distribution of nearly all of the surplus possessed by the society."

[21] Albert O. Hirschman discusses similar issues in his *Exit, Voice, and Loyalty* (Cambridge, Mass.: Harvard University Press, 1970).

[22] See Thibaut and Kelley, *The Social Psychology of Groups*, chap. 6.

in the relationship.[23] In the short run however, aspiration levels may often deviate from the going rates of exchange. It is also important to remember, as for example Dahrendorf[24] has pointed out, that if there is a latent conflict of interests between actors, the legitimacy of authority must always be precarious and authority potentially illegitimate.

The term mobilization of power resources has two distinct meanings which are often not separated. Mobilization has thus been used to refer to the process whereby a party acquires control over power resources which it did not previously have,[25] to the process whereby a party invests or uses the power resources already under its control,[26] or to both these processes.[27] In the present context it is important to keep these aspects separate. Here the term mobilization is used to refer only to the process of investing or using power resources already under one's control.[28] The equally important process by which a party acquires control over new resources or loses control over some of its resources, affects the difference in power resources or the balance of power between the actors concerned, i.e., the basic independent variable in the present model. The consequences of acquiring or losing control over power resources, that is, the changes in the difference in power resources or in the balance of power between the parties concerned, is thus a central aspect both in the political process approach and in the balance of power model of conflict. The balance of power model does, however, attempt to refine the political process model of conflict by specifying the mechanisms through which changes in the power balance between parties affect the probability of conflict between the parties.

A Power Balance Model of Conflict

In order to facilitate the presentation and understanding of the power balance model discussed here, the variables assumed to affect the probability of mobilization of an actor's power resources are outlined in Figure 1. The causal relationships assumed to exist among these variables are indicated by arrows in the figure. In the following presentation and discussion of these hypothetical causal relationships, each hypothesis is numbered serially in the order of presentation and the corresponding arrow can be located in the figure by this number. Plus signs (+) on the arrows indicate positive relationships between the variables, whereas minus signs (−) indicate negative relationships.

As stated above, the basic independent variable here is the actual difference in power resources between the parties. We assume that before taking action, a rational actor will attempt to assess the difference in power resources between himself and his opponent. Let us further introduce a probabilistic element into the perception of the difference in power resources, but make the assumption that on the whole the actual difference in power resources tends to be correctly perceived by the parties.[29] Thus when the balance of power between two actors, A and B, changes to the benefit of actor A, the probability that A will perceive himself as the stronger party will increase monotonically (*Hypothesis 1*).

Following Atkinson[30] and Blalock[31], we will assume here that the degree to which power resources are mobilized is a *multiplicative* function of the perceived probability of achieving the goal (expectancy of success) and of the utility to the actor of achieving the goal. Now if actor A perceives his power resources as larger than those of his opponent, actor B, his expectancy of success will naturally be higher than if he perceives himself to be the weaker party. We can thus assume a positive relationship between the difference in power resources an actor perceives between himself and his adversary and his expectancy of success (*Hypothesis 2*). *Ceteris paribus*, the higher an actor's expectancy of success, the higher is the probability of mobilization (*Hypothesis 3*), and, further, the higher his utility of reaching the goal, the higher is the probability of mobilization (*Hypothesis 4*).

The utility to an actor of reaching the goal is here assumed to be a function of his expected costs in reaching the goal and of the degree of his relative deprivation with respect to the goal. In a situation of potential conflict the anticipated costs of reaching the goal will to a large extent

[23] Rex, *Key Problems in Sociological Theory*, pp. 181–182.

[24] Dahrendorf, *Class and Class Conflict in Industrial Society*, p. 176.

[25] Gamson, *Power and Discontent*, p. 98, Snyder and Tilly, "Hardship and Collective Violence in France, 1830 to 1960", p. 526 and Tilly, "Does Modernization Breed Revolution?" p. 437.

[26] Blalock, *"Toward a Theory of Minority Group Relations,"* p. 110.

[27] Oberschall, *Social Conflict and Social Movements*, p. 28.

[28] The use of power resources by collectives has also been termed "collective action"; see Mancur Olson, Jr., *The Logic of Collective Action; Public Goods and Theory of Groups* (Cambridge, Mass.: Harvard University Press, 1965) and James S. Coleman, "Foundations for a Theory of Collective Decisions" in *Social Choice*, ed. Bernhardt Lieberman (New York: Gordon & Breach, 1971).

[29] Such probabilistic relationships allowing for influences from variables not included in the model are also assumed among the other variables in the model.

[30] Atkinson, *An Introduction to Motivation*, p. 242.

[31] Blalock, *Toward a Theory of Minority Group Relations*, p. 127.

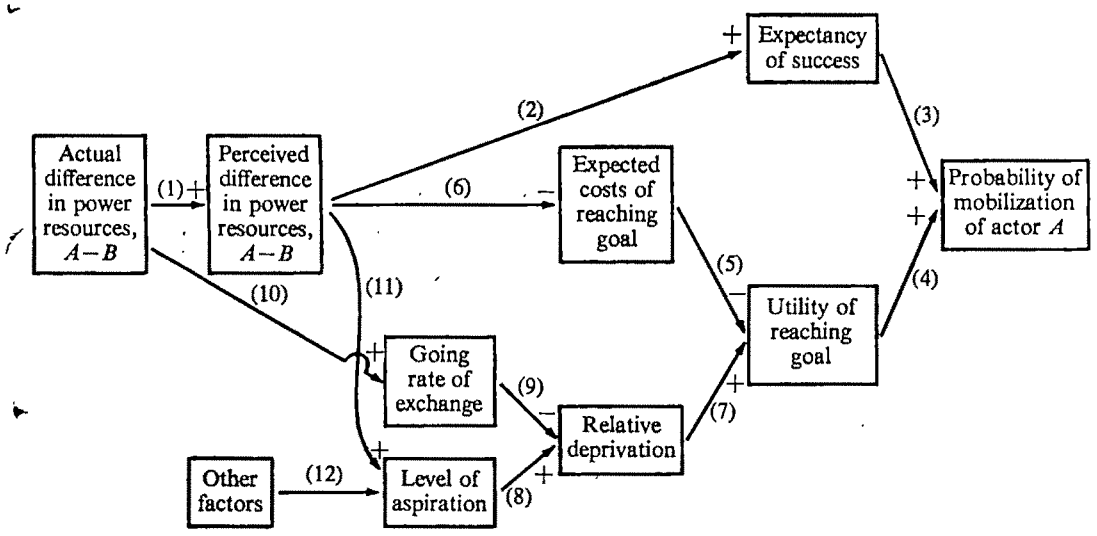


Figure 1. Causal relationships between main variables assumed to affect probability of mobilization of actor A in a situation of potential conflict with actor B.

depend on how the other party is expected to react. The relevant costs of power here are the costs to actor A of attempting to influence actor B by pressure resources and the costs to B of attempting to resist A's influence attempts, both defined in terms of the value of the opportunities foregone in choosing to attack the other party or to resist his influence.[32] The higher the expected costs to an actor are, the lower his utility[33] is assumed to be (*Hypothesis 5*). Expected costs for reaching the goal are assumed to be negatively related to the perceived difference in power resources. Thus the larger the difference to his advantage an actor perceives, the smaller are his expected costs (*Hypothesis 6*).

The relative deprivation of an actor was defined above as the difference between the going rate of exchange and the actor's level of aspiration. The higher an actor's relative deprivation with respect to the goal, the higher is his utility of reaching the goal assumed to be (*Hypothesis 7*). *Ceteris paribus*, the higher an actor's level of aspiration, the higher is his relative deprivation (*Hypothesis 8*) but the higher or better his going rate of exchange, the lower is his realtive deprivation (*Hypothesis 9*).

We have assumed above that the actual difference in power resources between actors will in the long run determine the going rate of exchange. Thus the more advantageous to an actor the balance of power is, the better is his going rate of exchange in the relationship (*Hypothesis 10*). Now we have assumed that the actual difference in power resources between two actors determines both the perceived differences in power resources and the going rate of exchange in the relationship, and that the level of aspiration depends primarily on previous experiences of outcomes in similar exchange relationships. Therefore we can expect that through a process of learning, an actor's level of aspiration will to a large extent be influenced by the perceived difference in power resources in his present exchange relationship (see above).

Thus the more an actor perceives that the balance of power is to his favor, the higher is normally his level of aspiration in the relationship (*Hypothesis 11*), since by personal and by vicarious experiences he has learned to expect that in similar situations the stronger actor will strike

[32] See John C. Harsanyi, "Measurement of Social Power, Opportunity Costs, and the Theory of Two-person Bargaining Games", *Behavioral Science*, 7 (Jan., 1962), 67–80; and also A. Baldwin, "The Costs of Power", *Journal of Conflict Resolution*, 15 (June 1972), 145–155.

[33] Atkinson, p. 242, assumes utility or value of achieving the goal to be a multiplicative function of an actor's motivation (need) to achieve the goal and of incentives (objective rewards or punishments associated with the goal). The costs associated with reaching the goal should be deducted from its objective incentive value to find the "net" value of reaching the goal. Utility is thus here regraded as "net" utility derived from different types of outcomes as, for example, costs of reaching the goal and improved exchange rates.

the better bargain and will tend to get "the lion's share" of the goods to be distributed.[34] This conclusion is generally overlooked in discussions of factors determining levels of aspiration, where usually other factors are seen as the most important ones.[35] Of course we must also here assume that such other factors may increase or decrease an actor's level of aspiration (*Hypothesis 12*).

The above discussion leads to the conclusion that the more advantageous to him an actor perceives the balance of power in the relationship to be, the higher is his utility of reaching the goal. Since actors are here assumed to be satisficing rather than maximizing (see above), the effects on utility of further improvements in the balance of power of an already stronger actor will however gradually decrease. Now a change in the actual difference in power resources between two actors, which is to the favour of A but to the disadvantage of B, will increase the expectancy of success as well as the utility of actor A. From this follows that, *ceteris paribus*, such a change will increase the probability that actor A mobilizes power resources (but decrease the corresponding probability of actor B).

We have assumed that the probability of manifest conflict betweeen the two parties depends on the probability that they both mobilize power resources at the same time. For manifest conflict to occur, one of the parties has to initiate conflict through a punishing move against the other party, who has to retaliate. When the difference in power resources between the actors is very large, the probability that the stronger party makes a punishing move against the weaker is high, as is the probability that the stronger party will defend himself against an attack from the weaker party. Since, however, the probability that the weaker party will retaliate to the attack from the stronger party is low as is the probability that the weaker party himself will attack, the probability for manifest conflict remains low. When the difference in power resources between the actors begins to decrease, the probability of mobilization of the stronger party decreases while the probability of mobilization of the weaker party increases. This will increase the probability for manifest conflict.

The above discussion therefore indicates that we can expect a *curvilinear relationship between on the one hand the difference in power resources between the parties and on the other hand the probability of manifest conflict between them.* The probability of manifest conflict is thus low when parties have greatly unequal power resources but will normally increase when the difference in power resources begins to decrease. In contrast to other models of conflict, which usually assume maximum probability of conflict at the point of parity in power resources between the parties,[36] a bimodal distribution of the probability of manifest conflict on the difference in power resources between the two actors is predicted here. The probability of manifest conflict is thus assumed to be low when the power resources of the parties are greatly unequal, to increase up to a point with decreasing differences in power resources but to drop around the point of parity in power resources.[37] An increase in the difference in power resources between actors with unequal power resources is assumed generally to decrease the probability of conflict.

Once the parties have mobilized their power resources and entered into conflict, the outcome of the conflict may affect not only the perception of the difference in power resources between the parties but also the actual balance of power. In this way feedback mechanisms are introduced into the model.

Time, Relative Deprivation and the Rate of Change in Power Resources.

The effects of the time dimension are incorporated into the power balance model through an assumption that a change in the balance of power between actors affects the going rate of exchange and the aspiration levels of the actors with different degrees of delay. We assume that the aspiration level of an actor is slow as it moves downward but that it moves upward more easily. When the balance in power resources between two actors changes, the most immediate effect of this change is assumed to be an increase in the aspiration level of the actor who benefits from the change. If the new balance of power remains stable, the going rate of exchange in the relationship will tend to change accordingly as a result of bargaining or conflict or both. With time the aspiration level of the losing actor will then also approach the new going rate of exchange in the relationship.

[34] To the extent that power resources overlap with investments, this assumption is a parallel to the "rule of distributive justice" proposed by Homans, *Social Behavior: Its Elementary Forms*, chap. 12, but has different implications.

[35] See Gurr, *Why Men Rebel*, chap. 4.

[36] *Ibid.*, pp. 234 and 277.

[37] The prediction of some degree of decrease in the probability of manifest conflict around the point of parity in power resources derives from an assumption that changes in the difference in power resources have somewhat different effects on the probability of initiation or attack and on the probability of retaliation or defense in a conflict situation. This aspect of the power balance model of conflict is not of immediate significance for the present discussion and is developed in another paper by the present author, Walter Korpi, "Conflict and the Balance of Power", *Acta Sociologica*, 17, No. 2 (1974), 99–114.

We have assumed above that changes in the difference in power resources between two parties will affect the aspiration level of the gaining party before the going rate of exchange becomes modified. This indicates that not only the actual size of the difference in power resources but also the rate of change in power resources may have effects on relative deprivation. If the originally weaker actor experiences a rapid improvement in his power position, he will feel greater relative deprivation than after a slower increase, which gives time for a gradual readjustment of the going rate of exchange.

The above assumptions imply that increases as well as decreases in the difference in power resources between the parties will increase the relative deprivation of the actors in the period immediately following the change. If the new balance of power becomes stable, however, relative deprivation will decrease with time. This makes for an intricate relationship between relative deprivation and the probability of conflict, which we shall now consider.

Relative Deprivation and the Probability of Conflict

Relative deprivation has been assumed to increase according to three basic patterns. Decremental deprivation occurs in a situation where men's expectations remain stable but where they receive less and less. If men's expectations rise while their achievements remain stable, aspirational deprivation is created. The third type is progressive deprivation, which occurs when a relatively steady improvement in men's social and economic conditions generates increasing expectations but is followed by a sharp reversal in the trend of improvement.[38]

Decremental deprivation is usually the result of an increase in the difference in power resources between the actors. An increasing difference in power resources will change the going rate of exchange to the disadvantage of the weaker actor. Since aspiration levels are assumed to be slow in moving downward, a decrease in achievements will lead to decremental deprivation for the weaker actor. The probability for conflict can, however, be expected to decrease in such a situation since the deteriorating balance of power will decrease the expectancy of success of the weaker party and increase the expected costs of reaching the goal, which will decrease the probability of mobilization.

A common cause of hardships experienced by many groups in society is probably that their command over power resources relative to that of other groups in society is slipping. In spite of their high motivation to achieve improvements and a high degree of relative deprivation, collectivities losing power are less likely to enter into conflict. The assumption that mobilization is a multiplicative function of utility and expectancy of success implies that if expectancy of success is low, a high utility or desire for change will normally not lead to a high probability of mobilization.[39]

The power balance model of conflict allows for increases in relative deprivation of an actor even when the difference in power resources and the going rate of exchange remain stable, i.e. for aspirational deprivation. Aspirational deprivation will increase the utility of mobilization of an actor. Since the balance of power between the actors can be assumed to be stable, expectancy of success will not increase, however. Also expected costs for reaching the goal will remain unaffected. Because of the multiplicative relationship assumed between expectancy of success and utility, aspirational deprivation will have relatively small effects on the probability of mobilization of an actor and thus on the probability of conflict. Progressive deprivation has been assumed to occur in connection with a rapid improvement in the social and economic conditions of the lower social strata.[40] To the extent that this improvement is the result of a more equal distribution of the surplus in society, we can assume that the power differences between higher and lower strata have decreased.[41] Progressive deprivation thus generally occurs in situations where the difference in power resources between actors is decreasing.

A decrease of this kind in a large difference in power resources between two parties is expected to increase the probability of conflict. This increase in the probability of conflict is primarily the result of the assumptions made in the present model of conflict that the probability of conflict

[38] Gurr, pp. 46–58. More complex varieties of these basic patterns are discussed by Feierabend, Feierabend and Nesvold in "Social Change and Political Violence: Cross-National Patterns."

[39] A very high utility can of course sometimes offset the effects of decreasing expectancy of success. Utility, being a multiplicative function of motivation and incentives (Atkinson, *An Introduction to Motivation*, p. 242), can be very high in situations which are of vital concern for the actor and where incentives thus are exceedingly high. For example, where extermination is threatened, mobilization can be expected in spite of very low prospects for success.

[40] Davies, "Toward a Theory of Revolution."

[41] Improvements in the social and economic conditions of the lower strata in the society can occur also when the going rates of exchange remain stable if the total surplus in the society increases. The improvements achieved by the lower strata in the industrial societies during the last hundred years is presumably an effect of improved exchange rates as well as of increasing surplus.

between two parties is higher if the difference in power resources between them is small than if the difference is large. The rate of change in power resources may however also have some effect on the probability of conflict. A rapid improvement in the power position of the weaker party will increase his level of relative deprivation more than a gradual improvement will; it will thus bring about some additional increase in his utility of reaching the goal and to some extent also in the probability of mobilization. When a formerly large difference in power resources between two parties begins to decrease, the probability of conflict is therefore expected to be especially high. In contrast to the other forms of relative deprivation, progressive deprivation is thus assumed to be associated with, albeit not the major cause of, an increase in the probability of conflict.

If the new balance of power between parties with formerly large differences in power resources becomes stable, the probability of conflict can be assumed to decrease with time, since through bargaining or conflict the going rate of exchange will change in favor of the party gaining in power, and since the aspiration level of the losing party will gradually move towards the new going rate of exchange. The probability of conflict, however, will remain higher than it was when the more unequal balance of power prevailed between the parties.

Discussion and Conclusions

In contrast to the expectation achievement approach to conflict, the power balance model of conflict outlined here indicates that we can not expect a generally positive correlation between relative deprivation and conflict. Let us compare the present model in this respect with two well-known examples of the expectation achievement theories of conflict, those proposed by Davies and by Gurr.

The basic proposition stated by Gurr[42] is that variation in relative deprivation strongly affects the potential for collective violence and thereby also the potential and magnitude of political violence. Increases in relative deprivation will thus, *ceteris paribus*, be associated with increases in conflict.[43] Davies's theory deals only with progressive deprivation and states that it is the decisive factor behind the outbreak of revolutions.

Gurr's theoretical tenet that increases in relative deprivation will lead to increases in the probability of conflict, does not make a distinction between different types of relative deprivation. Apparently Gurr assumes that all three types of relative deprivation have roughly the same effects on the probability of conflict. In this respect his theory is inadequate. As the analysis above indicates, only increases in progressive deprivation will be associated with a strong increase in the probability of conflict. Increasing aspirational deprivation will only add a little to the probability of conflict, while increasing decremental deprivation tends to be associated with a decreasing probability of conflict. These diverse trends, when taken together, would make us expect that the overall correlation between relative deprivation and probability of conflict will be insignificant.

The difference between the predictions made by Gurr and those derived from the present model is the result of the different role given to the balance of power between the actors in these two theoretical models. In the power balance model outlined here, the difference in power resources is assumed to affect an actor's achievement as well as his aspiration level and thereby also his degree of relative derpivation. Gurr introduces the balance in pressure resources between the parties at a later stage in the causal chain only to account for the extent to which relative deprivation finds expression in political violence.[44]

Like Davies's theory of revolution, the power balance model predicts that progressive deprivation will be associated with a high probability of conflict. This correlation, however, is not primarily a result of a rapid increase in the relative deprivation of the weaker party but rather an effect of the decrease in the power differences between the parties.

One of the basic similarities between the power balance and the political process models of conflict is that both see conflicts as natural byproducts of the normal political or bargaining processes. Another basic similarity is the central role accorded to the changes in power differences between actors. In the political process model of conflict, the acquisition or loss of power resources by the parties—mobilization and demobilization—are seen as the central variables in the development of conflict,[45] since they affect the capacity of the parties to make claims for privileges and advantages which, if resisted, lead to conflicts. The power balance model of conflict takes the difference in power resources as the central independent variable.

It appears, however, that in the political process model of conflict, acquisition and loss of power resources are assumed to have roughly the same effects on the probability of manifest conflict. Thus Tilly[46] states that conflicts such as col-

[42] Gurr, *Why Men Rebel*, p. 320.

[43] The magnitude of political violence is also assumed to be affected by legitimacy, coercive potential and social-structural facilitation (Gurr, chaps. 6–9).

[44] *Ibid.*, chap. 8.

[45] Snyder and Tilly, "Hardship and Collective Violence in France, 1830 to 1969," p. 527, and Tilly "Does Modernization Breed Revolution?", p. 437.

[46] Tilly, pp. 438–439.

lective violence tend to be especially frequent not only when groups are gaining in power relative to others but also when they are losing power resources. Such a prediction does not follow from the power balance model of conflict, which makes a distinction between situations where the difference in power resources increases and where it decreases.

It is true as Tilly[47] observes that both parties gaining and parties losing power resources relative to others will "have a special propensity to articulate strongly moral definitions of their situations" and thus to suffer from relative deprivation. According to the present analysis, however, we would not expect parties losing power resources to contend for privileges as often as parties gaining in power. In a situation where an already weaker party is losing power resources the probability for conflict is therefore normally expected to be lower than in a situation where the weaker party is gaining power resources. In this central respect, then, the power balance model can modify the predictions of the political process model. Also when it comes to the change in the probability of conflict over time, the power balance model can help to clarify the political process approach by specifying what mechanisms will modify the propensity of the actors to contend for privileges.

Testing the Models: Industrial Conflict as an Illustrative Case

Needless to say, the empirical testing of theories of conflict like the power balance model and the political process model presents formidable problems. The scope of such an undertaking as well as its possibilities and difficulties are illustrated by the massive efforts of the Michigan group.[48] One of the intervening variables in the power balance model, the concept of utility, is difficult to measure indeed and is used here as a heuristic to arrive at predictions concerning the nature of the relationship between changes in the power balance between parties and the probability of manifest conflict.

In the empirical testing of models of conflict like the ones discussed here, over-time data are of course preferable to crossectional data, but are harder to come by. The Michigan group has shown how over-time indicators on collective violence and the causal factors in the expectation achievement and in the political process models can be produced and utilized. Time-series of official statistics in different countries should provide some possibilities. One can note, for example, that the size of the national budget used by Snyder and Tilly as an indicator of the repressive capacity of the government can also be taken as an indicator of the balance of power between the governmental authorities and other groups contending for power. Snyder and Tilly find that this variable is to some extent related to the incidence of collective violence in France from 1830 to 1960, a result which agrees with the power balance model of conflict.

For other examples of the possibilities of confronting some hypothesis in the power balance model of conflict with empirical data, we can turn to studies of industrial relations. Changes in the level of unemployment can be taken as an indicator of changes in the balance of power between the two parties on the labor market, i.e., employers and labor. When unemployment decreases, the power position of labor can generally be expected to improve, while it can be expected to decline in periods of increasing unemployment. The wage rate can be taken as an indicator of the going rate of exchange on the labor market. The power balance model of conflict predicts a direct relationship between the balance of power and the going rate of exchange (*Hypothesis 10*). This hypothesis receives strong support from the well-known Phillips curve, which refers to the negative relationship between level of unemployment and the rate of change in wages. Wages have thus been found to increase considerably faster when unemployment is low than when it is high, and the change in the level of unemployment appears to be the best predictor of the rate of change in wages.[49]

The central prediction of the power balance model of conflict is that between parties with unequal power resources, decreases in the difference in power resources will increase the probability of conflict, while increases in the power difference will decrease the probability of conflict. Data on industrial conflict are consonant with this prediction. In several countries it has thus been observed that the frequency of strikes shows a cyclical variation, which tends to be positively related to the trade cycle and negatively to changes in the level of unemployment.[50] Thus when the trade cycle shows an upward swing and unemployment decreases, the frequency of strikes tends to increase, while the reverse tends to occur when the trade cycle declines and unemployment

[47] *Ibid.*, p. 438.

[48] See Snyder and Tilly: also Lodhi and Tilly, "Urbanization and Crime in 19th-Century France."

[49] The original findings in A. W. Phillips, "The Relation between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861–1957," *Economica*, 25 (November, 1958), 283–299, have since been replicated in many studies. See, for example, Lars Jacobsson and Assar Lindbeck, "Labor Market Conditions and Inflation—Swedish Experiences 1955–67," *The Swedish Journal of Economics*, 81, No. 2 (1969), 64–103.

[50] See K. G. J. C. Knowles, *Strikes—A Study in Industrial Conflict* (Oxford: Basil Blackwell, 1952), 143–150; Albert Rees, "Industrial Conflict and Business

increases. These observations are thus consonant with predictions made according to the power balance model of conflict.

While the power balance model of conflict predicts that the probability of conflict is higher when the difference in power resources between parties is decreasing than when it is increasing, in the political process approach these situations appear to be viewed as equally conflict-prone. Rees[51] and O'Brien[52] report empirical data from the United States for the period 1915–1961 on the relation between the cycle of general business activity and the frequency of strikes beginning each month in the consecutive stages of the business cycle, which provide information of relevance for this issue. The general finding of Rees and O'Brien is that the frequency of strikes reaches its peak before the peak in general business activity is arrived at and remains relatively high also in the period immediately following the peak in business activity. In these studies the frequency of strikes is unfortunately not related to the changes in unemployment. Unemployment is highly correlated, however, with general business activity although unemployment tends to continue to decrease also somewhat after the peak in general business activity is reached.[53] These findings would thus appear to give support to the power balance model of conflict over the political process model on the issues whether declines and gains in power resources have similar effects on the probability of conflict.

Fluctuations," *Journal of Political Economy*, 60 (October, 1952), 371–382; Walter Korpi, *Vilda Strejker inom Metall- och Verkstadsindustrin* [Wild-cat Strikes in the Metal Working Industry], (Stockholm: Department of Sociology, University of Stockholm, 1968); Douglas A. Hibbs, Jr., "Industrial Conflict in Advanced Industrial Societies" (Cambridge, Mass.: Massachusetts Institute of Technology, 1973. Mimeo.); F. Stephen O'Brien, "Industrial Conflict and Business Fluctuations: A Comment," *Journal of Political Economy*, 73 (December, 1965), 650–654.

[51] Rees, "Industrial Conflict and Business Fluctuations."

[52] O'Brien, "Industrial Conflict and Business Fluctuations: A Comment."

[53] Rees; O'Brien.

The Influence of Parents on the Political Attitudes of Adolescents

KENT L. TEDIN
University of Houston

Introduction[1]

A common assumption made by students of socialization is that the family is the major source of cultural transmission in society. The child is said to mirror his parents on a wide variety of social and political values. Ernest Campbell, writing in the 1969 edition of the *Handbook of Socialization*, claims that "we would be exceedingly surprised were we to discover, in research on any factor whatsoever, that a knowledge of the parents' position or score on a factor did not predict positively to the score of the adolescent."[2] This general conception is reflected in works concerned specifically with political socialization. Roberta Sigel asserts, "The similarity of views between parents and children on such issues as foreign affairs, civil rights, economics and many other vital topics of the day is truly phenomenal."[3] Herbert Hyman, in a general review of the empirical research related to the transmission of political attitudes (pre-1960), finds this research to ". . . establish very clearly a family correspondence in views that are relevant to matters of political orientation. Over a great many such correlations from different studies, the median value approximates .50."[4]

Recently, however, the effectiveness of the family, notably the parents, in the transmission of certain political values has been called into question. R. W. Connell, referring explicitly to Hyman's review, asserts that almost all investigations which report high correlation coefficients between parent-child attitudes have a "self-selection" bias in their sampling procedure. This bias, he argues, artificially inflates relationships, leading to an overestimate of the influence parents have on the political attitudes of their children.[5] Post-1960 empirical evidence lends support to this contention. Based on the University of Chicago socialization study, Robert Hess and Judith Torney assert that the family has little, if any ability to transmit idiosyncratic attitudes. Rather, its influence is limited to partisanship and to reinforcing those attitudes consensually held.[6] The same inference can be drawn from the family socialization project carried out by the University of Michigan's Survey Research Center. In the currently most influential intergenerational study, "The Transmission of Political Values from Parent to Child," M. Kent Jennings and Richard Niemi report relatively low correlations between attitudes held by parents and those held by their adolescent children. Over a range of issues, excluding party identification, the highest coefficient of association was found to be .34, and most coefficients were considerably lower.[7]

But even in this critical light, it is apparent that parents do influence the attitudes of their children. Negative correlations, in any study, are almost nonexistent. It seems probable that bivariate (uncontrolled) correlations between parent-child attitudes in any reasonably stratified sample, drawn in the early 1970s, are going to be positive and moderately low. Perhaps in a different time period (for example, the Depression, when many of the investigations reviewed by Hyman were conducted) correlations, even in the case of unbiased samples, would have been positive and moderately high.

But if concern is with building cumulative knowledge, the population parameter is only of passing interest. The real interest lies in specifying conditional universal propositions. Regardless of the issue, time-frame, and population, under what conditions do parents influence the political

[1] Financial support for this project was provided by a National Science Foundation Grant for Doctoral Dissertation Research made to Donald J. McCrone (Grant #GS 34218), and by a research grant from the Graduate College of the University of Iowa. I would like to thank Professor Donald McCrone, for his sound substantive advice, and George Woodworth, Assistant Professor of Statistics, University of Iowa, for his advice on the general linear hypothesis. Thanks also to John C. Wahlke for a number of helpful suggestions, and to Susan Brantley and John Tatlock for many stimulating discussions relevant to this research.

[2] Ernest Campbell, "Adolescent Socialization," in *Handbook of Socialization*, ed. David A. Goslin (Chicago: Rand McNally, 1969), p. 827.

[3] Roberta Sigel, *Learning About Politics* (New York: Random House, 1970), p. 104.

[4] Herbert Hyman, *Political Socialization* (Glencoe, Illinois: The Free Press, 1959), p. 72.

[5] R. W. Connell, "Political Socialization and the American Family," *Public Opinion Quarterly*, 36 (Fall, 1972), 326–327.

[6] Robert Hess and Judith Torney, *The Development of Political Attitudes in Children* (Chicago: Anchor Press, 1967), p. 113.

[7] M. Kent Jennings and Richard Niemi, "The Transmission of Political Values from Parent to Child," *American Political Science Review*, 62 (March, 1968), 172–176.

attitudes of their children? What, if any, are the general principles involved?

Criteria for Studying the Transmission of Political Attitudes and Values

Studies of family attitude transmission should clearly demonstrate at least three points: (1) the nature and extent of the attitude correspondence between parents and children,[8] (2) the degree to which this correspondence is a function of parental transmission as distinguished from other social forces, and (3) the factors that affect the success of transmission.

The Nature of Parent-Child Attitude Correspondence

There are three possible types of attitude correspondence between parents and children, which we may call "relative pair," "absolute pair," and "group" correspondence.[9] Relative pair correspondence, usually indicated by some type of correlational statistic, is the measure used in most studies. All correlations, whether based on intervals or ranks, determine the extent to which one distribution is a function of another. If a correlation coefficient is high, it means, for example, that the most conservative fathers will usually have the most conservative sons. It does not mean that the absolute scores of the sons on a scale of conservatism are at all close the absolute scores of their fathers. Correlation coefficients are not measures of absolute agreement. On a scale ranging from one to fifty, all the sons could be in the one-to-twenty range and all the fathers in the thirty-to-fifty range, and there could still be an extremely high correlation between their attitudes.

Absolute pair correspondence is the extent to which the scale scores of the sons are exactly the same as the scale scores of their fathers, e.g., twenty-five per cent of the father-son dyads have a score of two on the scale or item in question. This correspondence is usually measured, as just indicated, by percentage agreement.

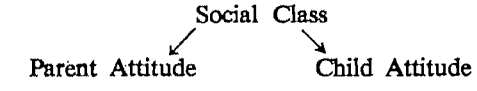
Group correspondence is the extent to which the opinion distributions of each generation are similar, regardless of the pair relationships between parents and their own children. For example, fifty per cent of a student sample and forty-five per cent of a parent sample might favor socialized medicine, a case of close group correspondence. But this says nothing about the correspondence between parents and their *own* children. Group correspondence is usually measured by means and standard deviations, or percentages as in the example.

Distinctions among the types of correspondence are not always properly made in the literature. Hyman, for example, interprets at least one case of group correspondence as relative pair correspondence.[10] While pair comparisons are most often made with correlation coefficients, terms like "transmission," "similarity," or "congruence" are often used simultaneously in ways that imply absolute pair correspondence. In the following discussion the term "congruence" will be reserved for absolute pair correspondence. Group correspondence will be used as defined above. The terms "transmission" and "influence" will indicate relative pair correspondence.

Direct Parental Influence vs. Other Social Influences

Direct parental influence is only one possible explanation for parent-child attitude correspondence. An alternative explanation holds that parents and children have similar attitudes because each individual family member is exposed to the same social influences. Both group and pair correspondence could be spurious. Another alternative holds that the political attitudes of children are influenced by parents indirectly.[11] Tests for these possibilities have been extremely infrequent in past research.

Spurious Relative Pair Correspondence. The problem of spurious relative pair correspondence will be illustrated by assessing the possibility that it is social class which is causing parent-child attitudes to covary. It can be argued that by belonging to the same social class, parents and children are exposed to similar media, have friends and acquaintances with like attitudes, have the same "class interests," etc. This possibility can be diagrammed as follows:



Social class is leading parents and children to have similar attitudes in an absolute sense. This may have little meaning for correlation analysis,

[8] The discussion here will be limited to the *nature* of attitude correspondence.

[9] Connell discusses relative pair and group correspondence, but neglects the third possibility—absolute pair correspondence. Connell, pp. 324–325.

[10] Connell, p. 325.

[11] For example, many investigators believe that strict child-rearing patterns lead to authoritarianism in child. This authoritarianism could, in turn, "structure" policy attitudes. For a discussion of indirect socialization see Richard Dawson and Kenneth Prewitt, *Political Socialization* (Boston: Little, Brown, 1969), pp. 65–73; for a critique of the structuring principle see Donald D. Searing, Joel J. Schwartz and Alden Lind, "The Structuring Principle: Political Socialization and Belief Systems," *American Political Science Review*, 67 (June, 1973), 415–432.

particularly if the "third" variable is constant or has little variance. Arguments which claim that correlations are artificially high because the sample is "all of one social class," or "all from one atypical community" are almost certain to be false.[12] In the case of the example, social class would have to be fairly heterogeneous before it would affect the parent-child correlations. The logic would be something like: the lower the family's social class, the higher both parent and child would score on some scale relative to their own group. The spurious relationship hypothesis is usually tested by partialing for the third variable.[13]

Spurious Group Correspondence. In many circumstances group correspondence will be affected by common environmental variables. For example, religion may affect both parent and child attitudes toward legalized abortion. If the environmental factor varies, the possibility of a common effect on the two groups can be tested with a difference-in-means statistic. Often, however, there is no variance on these factors. An example would be a common community effect when the sample is drawn entirely from one community. There is no way to reject the hypothesis that group means are similar because parents and children live in the same place. But if the research concern is with parental influence, commonly affected mean values may be of little interest, as long as there is ample variance in parent-child attitudes. The research question remains: "To what extent are deviations from the child mean scores influenced by deviations from the parent mean scores?"

Direct Parental Influence and the Magnitude of Correlation Coefficients. While the procedures just outlined may be used to reject the spurious alternative to parental influence, another method is better for testing the extent to which correlations between parents' and children's attitudes support the direct parental influence hypothesis. The method involves varying certain conditions which might be expected to affect the strength correlations. One condition might be issue salience to the parent. If an issue is salient to a parent, we would expect attitude transmission to be more successful than if the parent is not concerned with the issue. Another condition might be the accuracy of the child's perception of the parent's attitude. If parents are directly influencing their children, an important prerequisite would seem to be some knowledge of the parent's attitude by the child. But if correlations are spurious, or influence is indirect, it should make no difference whether or not an issue is salient to a parent or whether the child knows or does not know the parent's attitude. Variance in these conditions should not affect the magnitude of correlation coefficients.[14]

Specifying Conditions of Parental Influence

Correlation coefficients indicating the success of attitude transmission vary greatly from issue to issue and from one study to another. However, attempts to explain these variations, i.e., to specify the conditions of influence, have been notable only for their lack of success. Jennings and Niemi, for example, did not find a single variable having any significant impact on successful transmission.

Some have suggested that the content of an attitude, the attitude object, is consequential for the success of transmission.[15] More plausible, in my view, is a failure to specify all conditions relevant to successful transmission. What might these conditions be? Some insight can be gained by analyzing those attitudes successfully transmitted on a consistent basis. Of particular interest is party identification. The influence of parents on the partisanship of their children has been well documented. This attitudinal predisposition is surrounded by a somewhat unusual set of circumstances. Party identification is subject to considerable discussion in many families at regular intervals, i.e., elections. In addition, many parents convey to their children quite early that "we" are Democrats or "we" are Republicans.[16] Party identification is different, then, from most other attitudes in its *visibility* to the child and its *salience* to the parents. The important point to note is that these are population parameters. They are not inherent characteristics of the attitude object.[17] The influence of these variables can be moved to the table marginals and controlled.

[12] A constant will not usually inflate correlations. One instance, however, in which it would inflate correlations is when the constant causes a large percentage of both parent and child scores to fall into one rank or interval. It is important, therefore, to check the distribution of the attitude scores for both parents and children.

[13] A reduction due to partialing does not, of course, necessarily mean a spurious relationship exists. A reduction also occurs when a developmental sequence is present.

[14] This methodology is discussed by Frank F. Furstenberg, "The Transmission of Attitudes in the Family" (Ph.D. Dissertation, Department of Sociology, Columbia University, 1967), chapter 3.

[15] Stephen Wieting, "Family Factors and the Religious Belief Systems of Adolescents" (Ph.D. Dissertation, Department of Sociology, University of Minnesota, 1971), p. 87.

[16] Fred Greenstein, *Children and Politics* (New Haven: Yale University Press, 1965), pp. 71–72.

[17] There are, however, differences between party identification and issues which are inherent in the attitude

Perceptual Accuracy. At some point in any study of social influence it would seem imperative to determine if the person who is believed to be influenced is aware of the attitudes and values of the person or group believed to be exerting influence. This point is often overlooked in studies of social influence. Parents may be exerting considerable influence on the attitudes of youth, but it may be of the subjective sort. That is, children are identifying with what they think or perceive are their parent's attitudes.[18]

Perceptual accuracy can strongly influence successful transmission. In Furstenberg's study of mobility aspirations, the parent-child correlation for the group in which student respondents were uncertain of (didn't know or misperceived) parental attitudes was actually negative (tau-b-.183). But when perception was accurate, the coefficient was a positive .480.[19] There is a strong possibility that parent-child attitude correlations, for all issues, are highly dependent on the marginal distribution of perceptual accuracy for the attitude in question.

Issue Salience. The finding of Jennings and Niemi that the level of political interest had little or no effect on the success of value transmission surprised many observers. This failing, and the failure of other specification variables, seems rooted in a research assumption which has seldom been questioned—the idea that some families are more successful than others in transmitting attitudes. Investigators have ignored the cognitive and emotive factors surrounding specific issues and have instead focused on all-encompassing variables like parent-child affect, parental authoritarianism and, in the same intellectual tradition, political interest. Successful transmission, rather than essentially being a family characteristic, may essentially be issue specific. That parents are "politically interested" does not mean they necessarily have interest in any particular given issue. The reverse is also possible; parents may find some political issues quite important without having any general interest in politics. To the extent that the general level of political interest and the salience of any one issue diverge, the effect of political interest on the successful transmission of the attitude toward that issue is likely to be reduced.

Family Interaction. Most family interaction variables, such as parent-child affective relations, have been found to have either a slight positive effect or no effect on successful parental attitude transmission. Family interaction, not being specific to issues, may be too general to have much impact. It is conceivable that the influence of family variables is severely attenuated by a child's inaccurate perception of the parental attitude. All the family conditions necessary for parental influence could be present, and the child may in fact want to identify with the parent attitude. When questioned, he may give a response he believes similar to the attitude of the parent. But if the parental attitude is misperceived, the child's response is treated as an "error." According to theory, he should have shared the parent attitude, but did not. There is no reason to believe that there is any consequential relationship between variables like parent-child affect, parental authoritarianism, and accurate perception. It comes as no surprise, therefore, that when positive effects are reported for a family variable, the attitude object is almost invariably one that is highly visible, like party identification.

The study on which the following discussion is based was designed to investigate the transmission of attitudes from parents to children in light of the above considerations. I shall proceed by first examining the extent and character of the attitude similarity between parents and children in terms of relative pair, absolute pair and group correspondence. Next will be an analysis of the extent to which covariation between parent and child attitudes (relative pair correspondence) is due to direct parental influence as opposed to other factors. Finally, I shall attempt to elaborate, as generally as possible, the conditions requisite to successful transmission.

The Sample and Measurement

The Sample

The sample for this analysis consists of 322 parent-child dyads. The "children" are recent high school graduates. Recent graduates were selected because at the time of the interview most were fully enfranchised members of the political community, and almost all would be eligible to vote in the 1972 presidential election. This procedure avoids the common criticism of social-

objects. For example, "independent" for party identification is not equivalent to "undecided" in the case of issue attitudes. This point will be discussed again later in the paper.

[18] Richard Niemi reports that adolescents in the Michigan socialization study perceived themselves as attitudinally closer to parents by an average of .10 (tau-b) than they were in actuality. He suggests that this misperception serves an important psychological function for the youthful respondents by reducing whatever tension may arise from asymmetry in parent-adolescent political attitudes. Richard G. Niemi, "Collecting Information about the Family: A Problem in Survey Methodology," in *Socialization to Politics*, ed. Jack Dennis (New York: John Wiley and Sons, 1973), p. 480.

[19] Furstenberg, p. 172.

the mediocre not in transcending political bias (i.e., its commitment to certain political values), but rather in its communicative power and range of appeal. Indeed a mediocre argument is perceived as sectarian or ideological because it lacks those very qualities: it seems persuasive or cogent only to a few people, only from within a single viewpoint. The arguments or the picture of the world presented by a great theorist may be so impressive, appeal to so many, that the theory remains interesting and challenging well after the political crisis that occasioned it is forgotten.

One advantage of examining political theory in terms of its communicative power is that it allows us to view a theory as a complex and subtle work, involving many methods of persuasion, a persuasive scheme not limited to the strictly logical (demonstrative) level, to which the analytic school restricts its attention. From the latter perspective many facets of a theory may be regarded as unessential, or even irrelevant to the argument.[11] The approach used here, in contrast, does not dismiss any aspect of a work of political theory; it presumes that every facet of a work has some purpose, is designed to appeal to some particular mentality. If this view of political theory is valid, it should be possible to look at the work of a "great" theorist and find there various styles of argument and dimensions of persuasion by which he tried to appeal to his contemporaries.

In the following sections we will examine the political theory of Hobbes from this perspective. Hobbes is a particularly appropriate subject for assessing the merits of this approach since he is unquestionably a great political theorist; great in terms both of the durability of interest in his work and of his impact on his contemporaries.[12] If we are able to show that Hobbes's theory does, in fact, conform to the pattern of argument we have suggested, we will have begun to account for his great impact on his contemporaries, as well as providing support for our general view of political theory as argument aimed at persuading a specific audience.

[11] E.g., F. S. McNeilly, *The Anatomy of Leviathan* (London: Macmillan, 1968), views the argument of *Leviathan* in such narrow terms that several prominent aspects of the book are dismissed as irrelevant: ". . . although the notion of a universal fear of death is constantly mentioned, it does not perform any actual, still less any indispensible, part in the argument" (p. 250). Hobbes's "pessimistic view of human nature" and his idea of the desire for precedence are similarly treated.

[12] Cf. Richard Ashcraft, "Hobbes's Natural Man: A study in Ideology Formation," *Journal of Politics*, vol. 33, no. 4 (November, 1971), 1077–79; Samuel I. Mintz, *The Hunting of Leviathan* (Cambridge: Cambridge University Press, 1962), pp. viii, 147; Quentin Skinner, "The Context of Hobbes's Theory of Political Obligation," in Cranston and Peters, *Hobbes and Rousseau*, pp. 109–119.

The Ideological Setting

In order to understand to which of his contemporaries Hobbes was addressing himself, and of what he was trying to persuade them, we must look at the political position Hobbes himself held. He is usually seen as an authoritarian advocate of law and order: the author of a doctrine so extreme that neither royalists nor parliamentarians would accept it. Because neither side in the civil war would acknowledge Hobbes as a friend it has often been assumed that he was, in fact, aligned with neither side. Yet the explanation that Hobbes gave his own activity is very different; it puts him firmly in the royalists' camp: he wrote *De Cive* so that "all nations" should know what the rebels were doing in England, and so detest them; in that work he endeavored "to gain a belief in men that monarchy is the most commodious government," a point which he confessed he had not "demonstrated, but only probably stated." "*Leviathan* was written in defense of the king's power, temporal and spiritual, [and] in the behalf of those many and faithful servants and subjects of his majesty, that had taken his part in the war, or otherwise done their utmost endeavor to defend his majesty's right and person against the rebels. . . . That book now fights for all kings and for all those under any title who exercise the rights of kings."[13] Hobbes's royalist credentials receive additional confirmation from his history of the civil war, *Behemoth*, which is clearly and strongly royalist in its sympathies. Finally, in his autobiography, Hobbes characterized himself as "one who strove with his pen to defend kingly power."[14]

Even if we accept Hobbes as a defender of the king, the question still remains why he was such an unorthodox royalist? Why did he not articulate a doctrine that the king's supporters would embrace? The answer lies in the very crisis that was the object of Hobbes's concern. Clearly the society was divided into conflicting ideological camps well before the civil war actually broke out.

[13] *The English Works of Thomas Hobbes of Malmesbury*, ed. William Molesworth (London: J. Bohm, 1839–45), IV, 415, 407, 420–21. (Hereafter cited as *E.W.*); *De Cive or The Citizen*, ed. Sterling P. Lamprecht (New York: Appleton-Century-Crofts, 1949), p. 17; "Autobiography" in *Rationalist Annual* (1958), p. 27.

[14] "Autobiography" p. 28; cf. *Leviathan*, ed. Michael Oakeshott (London: Basil Blackwell, 1955) pp. 122–125; Royce Macgillivray, "Hobbes's History of the Civil War," *Journal of the History of Ideas*, vol. 31, no. 2 (April-June, 1970), 179–198. Very little is known of Hobbes's life, but those few facts that are known support the idea that he was a royalist. Hobbes spent most of his adult life as a member of the aristocratic and royalist family of the Cavendishes. Prior to the Civil War he was a member of a circle of royalist intellectuals. (See Irene Coltman, *Private Men and Public Causes* (London: Faber, 1962), pp. 135–192). Later, in exile, he tutored the future Charles II.

good lawyer does not simply present the facts, for often the facts are not in question, but "marshals them so as to emphasize those which favor his client. . . . He chooses language which does not merely state, but impress: he uses voice, gesture, facial expression, all the devices of eloquence and style in order to influence the decision of the jury in favor of his client."[6]

This view is not suggesting that fraudulent advocacy be taken as the model of moral or political discourse, for lawyers are not all frauds, nor need they be. The point is that we defend our values much as lawyers do, who use their material to impress and convince, but not to prove. The difference in the kinds of arguments employed corresponds to the difference in purpose: "the function of scientific judgments is to alter one's expectations; that of ethical judgments, by contrast, is to alter one's feelings and behavior."[7] In other words, the validity of a political or ethical statement is inseparable from its communicative power;[8] an assertion which convinces no one has no value in political discourse. Thus while assertions of values cannot be true or false in any objective sense, decisions and choices can be better or worse, defenses can be powerful and persuasive, or weak and unconvincing. It is in terms of these judgments that we can develop evaluative criteria for political theory.

Once it is recognized that political theory involves making arguments much as a lawyer might, it becomes relevant to know to whom an argument is addressed, what kind of jury is it that must be convinced. Since a major function of ethical and political judgments is to alter feelings and behavior, we must necessarily seek to understand such argument in relation to its audience. To assume that all argument is addressed to some abstact "universal man" would be to misunderstand the dynamics of political argumentation. For example if we wish to convince someone of the value of democracy, we might present him with an attractive picture of a free and democratic society, and if he already values independence, experimentation, mutual trust, etc., he may agree that these values are realized in such a society and so may accept democracy. But if he holds other values (that is, if we have misjudged our audience), he may call independence, insolence; experimentation rash meddling; and our argument will have failed.[9] Thus knowing the character of the audience addressed can serve as a useful guide in an examination of a work of political theory.

It is a commonplace that great political theory has often been written during periods of great political upheaval, in which consensus regarding the modes of legitimizing and understanding the political order has broken down. In a crisis involving conflicting groups and ideologies one can no longer base ones political position on traditional assumptions, for they have been called into question by the crisis. In such a situation the effective theorist must go beyond a narrow defense of a particular position (since the grounds for such a defense might have dissolved) in order to present a more comprehensive picture of the political world. That is, since one can only defend a particular value within a shared context of belief, the theorist attempts to present a new or reformulated world-view to replace the now discredited old one, through which the political order can again be made intelligible. The resulting world-view can vary immensely in philosophical grandeur (compare Hegel and Machiavelli) but it must result in a comprehensive and satisfactory picture of the world. It is precisely through the construction of such a new scheme of intelligibility that the aspiring theorist expresses his persuasive power.

The task confronting the political theorist is to build a case for his political and moral position that will appeal to the broadest possible spectrum of politically relevant groups. Unlike the lawyer in the courtroom he has no voice in the selection of his audience. The theorist must deal with the political situation as it is. Thus he must present as many arguments for his positions as there are relevant audiences. J. S. Mill, for example, presented two different arguments for liberty, each aimed at a different set of readers.[10] In fact, one mark of a great work of political theory may be its ability to appeal to a great many different people, including those with opposing political ideas. Thus a good political theory differs from

to make and assess arguments do not conform to the canons of the science of logic because formal logic has become a theoretical study unrelated to practical activity.

[6] MacDonald, "Natural Rights," p. 53.

[7] Stephen Toulmin, *An Examination of The Place of Reason in Ethics* (Cambridge: Cambridge University Press, 1968) p. 130.

[8] Sheldon S. Wolin, *Politics and Vision* (Boston: Little, Brown & Co., 1960), p. 27: "the validity of an idea cannot be divorced from its effectiveness as a form of communication."

[9] MacDonald, p. 54.

[10] "The presence of an individualist and a social justification of liberty in the essay *On Liberty* is not the product of a divided mind or confusion or lack of the necessary courage, but rather of design. There are two justifications because there are two audiences." Richard B. Friedman, "A New Exploration of Mill's Essay *On Liberty*," *Political Studies*, vol. 14, no. 3 (September, 1966), 303–304. Leo Strauss has suggested that Rousseau, in *A Discourse on The Arts and Sciences*, presents two distinct arguments addressed to two different audiences. "On The Intention of Rousseau" in *Hobbes and Rousseau*, ed. Maurice Cranston and Richard S. Peters (Garden City, N.Y.: Anchor Books, 1972) pp. 263–64.

Hobbes and His Audience: The Dynamics of Theorizing*

MARK GAVRE
University of California, Los Angeles

Political theory has for many years been subject to severe criticism for its failure to substantiate the claims it makes for itself. It has been attacked by both philosophers and scientists, and denied any standing in either discipline. Its claims for truth have been unmasked as mere assertions of ideology or individual biases. Its arguments have been exposed as either manipulation by "persuasive definition," or dismissed as guilty of the "naturalistic fallacy."[1] It appears that political ideas, no longer philosophically respectable nor scientifically tenable, have been relegated to the intellectual limbo of "personal preferences." While this line of attack is most often associated with T. D. Weldon's *Vocabulary of Politics* (which may be dated in many respects), it has, in fact, continued to be quite strongly held by many analytic philosophers and social scientists.[2] Thus it would seem that the contemporary student of political theory is in need of a new defense or justification, if his discipline is to be saved from virtual dismissal or severe curtailment.

While no attempt will be made here to refute the many attacks on political theory, it will be suggested that a different and more fruitful understanding of political theory is possible, and that many of the failures and errors attributed to political theory are, in fact, consequences of a rigid and mistaken conceptualization of the activity of theorizing about politics. Moreover since the great works in the tradition of political theory still remain challenging, forceful, and often persuasive, even to analytic philosophers, it is worth our while to try to understand the nature of that attraction, a task which the analytic school has not well performed. The first part of this paper will present an approach to understanding political theory; the second will discuss Hobbes as an example of such a challenging theorist.

* I am deeply indebted to Richard Ashcraft and Blair Campbell for their very helpful criticism and suggestions.

[1] See, for example, Charles L. Stevenson, *Ethics and Language* (New Haven: Yale University Press, 1944); Richard S. Peters, *Hobbes* (Baltimore: Penguin, 1967) pp. 166–167.

[2] According to Anthony Quinton, *Political Philosophy* (Oxford: Oxford University Press, 1967), "the works that make up the great tradition of political thought are methodologically very impure. They are only to a small . . . extent works of philosophy in the strict sense." Statements of values he classifies as "affirmations of ideology" not "analysis" (pp. 1,3); cf. Brian Barry, *Political Argument* (London: Routledge, 1965) pp. 37, 290n.

The argument of T. D. Weldon has received such wide circulation, that it is often assumed that his is the only view to be found among contemporary analytic philosophers. But in fact there have been a number of philosophers who are concerned with ethical questions and whose work provides the basis for an understanding of political theory very different from that of Weldon.[3] What these philosophers have in common is the use of the jurisprudential analogy; that is, ethical or political argument is seen as very similar to the type of argument employed by lawyers in a courtroom. For Margaret MacDonald, ethical statements are neither statements about facts nor emotive statements; they are rather like records of decisions or positions taken, which we support or defend, much as a lawyer would defend a client. They are like decisions which are rationally, not arbitrarily, made, in that we do try to defend them; similarly they are unlike mere expressions of emotion, for we do not feel called upon to defend a cry or a shout. We do not prove (inductively or deductively) an ethical statement, as we do a scientific proposition, for a decision can never be true or false, though it may be forcefully or poorly defended. While we try to "prove" the law of gravity, we do not refer to J. S. Mill's "proof" of liberty, but to his "magnificent defence" of it.[4]

That assertions of values are not supported by inductive or deductive methods should not be taken to mean that they are illogical or irrational, but only that another sort of reasoning process is involved. According to Wisdom, "The process of [legal] argument is not a chain of demonstrative reasoning. It is a presenting and representing of those features of the case which severally cooperate in favor of the conclusion, in favor of saying what the reasoner wishes said. . . . It has its own sort of logic and its own sort of end—the solution of the question at issue is a decision. . . "[5] A

[3] See Margaret MacDonald, "Natural Rights" in *Philosophy, Politics and Society*, First Series, ed. Peter Laslett (Oxford: Basil Blackwell, 1956); John Wisdom, *Paradox and Discovery* (Berkeley: University of California Press, 1970), and *Philosophy and Psycho-Analysis* (Oxford: Oxford University Press, 1953); Stephen Toulmin, *Uses of Argument* (Cambridge: Cambridge University Press, 1958).

[4] MacDonald, "Natural Rights," pp. 52–55; cf. B. Barry, pp. 53–54.

[5] *Philosophy and Psycho-Analysis*, p. 153; Toulmin, p. 2, suggests that the methods we use in everyday life

(see Note B-1 above) are (a) "social *Apartheid*" (8, 3, 2, 7), (b) "economic *Apartheid*" (4, 1), (c) "Afrikaans way of life" (12, 15, 14, 9), and (d) "political separatism" (13, 11). As a check of just how similar the English and Afrikaans factors are, interclass and product moment correlation coefficients comparing the two structures both measure .86. Three of the factors, however (b, c, d), are inverted, as indicated by extreme negative correlations. In other words, the two ethnic groups show different affective reactions to almost identically organized cognitions.

Perceptual Accuracy. Accurate perception of the parental attitude by the child has been shown by Furstenberg to have a very significant effect on the success of attitude transmission—at least in the case of mobility aspirations. The same should be true of political attitudes, with those parental attitudes most accurately perceived by adolescents being those which are the most successfully transmitted.

Hypothesis 1: The more accurate the adolescent's perception of the parent's attitude, the more influence the parent will have on the adolescent's attitude.

Perceptual accuracy for party identification was operationalized by asking each adolescent to report the party identification of his parents. This report was collapsed into three categories (Democrat, Independent and Republican) and compared with the actual parent party identification (also collapsed into three categories). If the two were congruent, perception was classed as accurate. If they were not congruent or the adolescent responded "I don't know," perception was classed as uncertain. The situation for the issues is a bit more complicated. These attitudes were measured with four opinion indicators. Adolescents were asked to report parent opinions for all four items using a five point agree-disagree scale. A sixth option was an "I don't know" response. These items and the corresponding parent items were collapsed into three categories—agree, undecided, disagree—and a comparison was made to determine the accuracy of perception. It is possible for adolescent respondents to manifest uncertainty on all four indicators at one extreme, or to perceive the parent's opinions correctly on all four items at the other. The result is a scale ranging from zero to four, the higher score indicating the greater accuracy.

Some analysts assume that party identification is an attitude intrinsically different from those toward issues or public figures, believing it is this difference which accounts for its successful transmission. Connell, for example, sees a difference in social patterning—the objects of opinion being institutions, and the choice among them being a relatively simply dichotomy. Jennings and Niemi, in discussing the successful transmission of party identification, note the low correlations observed in other domains of political life and assert that there is no ground for any "general thesis" explaining attitude transmission.[21] It was observed earlier that there are inherent differences between party identification and other attitudes. But there are also noninherent differences which are controllable. One of these is perceptual accuracy. Children are simply more likely, for reasons already discussed, to perceive parental party identification more accurately than they perceive attitudes toward current issues. Table 2 shows that while 72 per cent of the adolescents have a correct perception of their parent's party identification, not more than 12 per cent have a completely accurate perception of their parent's attitude toward any one issue. Even when the two categories indicating the most accurate perception are collapsed together, not more than 36 per cent could be classed as having an accurate perception of any one parental attitude.

These observations lead to the second hypothesis:

Hypothesis 2: Parents will have equal influence on the party identification and issue attitudes of adolescents when perceptual accuracy is held constant.

The correlations necessary to test hypothesis 1 and hypothesis 2 are presented in Table 3.

The effects of perceptual accuracy on parental influence are very strong and offer substantial support for hypothesis 1.[22] The data, in addition, support the direct parental influence explanation for parent-adolescent attitude correlations. For all four attitude objects there is a linear increase in parental influence as perception becomes more accurate. As predicted, adolescents uncertain of parental attitudes are subject to almost no parental influence.

Hypothesis 2 also receives support from the data presented in Table 3. Within categories of perceptual accuracy, the coefficients for the issues compare quite favorably with those for party identification. This correspondence suggests the operation of a general process, with the principles involved in the transmission of attitudes toward issues being the same as those involved in the transmission of party identification. Note also that the attitude toward marijuana laws does not deviate from the overall pattern. If there exists, as claimed by some sociologists, a youth culture exerting countervailing influence to that of parents, it should at this point be manifested.

Issue and Partisan Salience. Jennings and Niemi found that political interest had no effect on the success of attitude transmission. The same pattern, using the same operational indicator, is

[21] Connell, pp. 329–330, Jennings and Niemi, p. 169.

[22] Perceptual accuracy was collapsed into three categories to insure that each category had an adequate sample size. The linear patterns was, however, even more pronounced in the five-category case, although the N's were small. In the five-category breakdown, the correlations in the category exhibiting the greatest perceptual accuracy for the race issue, China policy, and the marijuana issue were .66, .72, and .61, respectively (based on 30–35 cases).

Table 2. Distribution of Perceptual Accuracy for the Issues and Party Identification

Attitude Objects	Perceptual Accuracy					
	Zero	One	Two	Three	Four	Total[a]
Racial Integration	11%	23%	30%	24%	11%	100% (N=318)
China Policy	24%	19%	20%	24%	12%	100% (N=318)
Marijuana Laws	15%	23%	28%	24%	09%	100% (N=317)
Party Identification[b]	28%	72%	—	—	—	100% (N=319)

[a] May not total to 100%, because of rounding.
[b] Perceptual accuracy for party identification is a dichotomy. The adolescent either knows (one) or does not know (zero) the parent's attitude.

found in the data employed here.[33] The parent-adolescent correlations within categories of political interest appear to be randomly distributed (data not presented).

The next step is to substitute issue and partisan salience for political interest. Salience of party identification to the parent was operationalized by breaking parental partisanship into three categories—independents, weak partisans, and strong partisans. A scale of parental issue salience was constructed using three items: (1) how important is the issue to the parent, (2) how often do the parent and adolescent talk about the issue, and (3) would it make any difference to the parent if the adolescent adopted an attitude on the issue different from that of the parent. The scale ranged from one to seventeen with the median scores for the race, China, and marijuana issues being 7.8, 6.8, and 11.4, respectively. It is obvious that the marijuana issue is considerably more salient to parents than are the other two issues. Since the distribution of perceptual accuracy did not differ on the three issues, we immediately suspect that the reason for the higher parent-adolescent correlation on the marijuana issue, as opposed to the other two issues, is the greater issue salience to parents.

Hypothesis 3: The greater the issue and partisan salience to the parent, the more influence the parent will have on the adolescent attitude.

Table 4 indicates clear support for the hypothesis, although the relationship in the "strong partisan" category for party identification is somewhat greater than the "high salience" category for the issues. It must be noted, however, that salience for the issues and salience for party identification are not exactly comparable. The term "Independent" for party identification does not have the same connotations as the term "low" for issue salience. It is possible for independents to be highly concerned and interested in partisan politics, while "low" issue salience necessarily entails low "interest" in that issue. Finally, Table 4 like Table 3, offers strong support for the "direct parental influence" explanation for parent-adolescent attitude covariation. While we cannot rule out all explanations alternative to parental influence, the direct influence hypothesis seems at this point the most plausible.[34]

[33] The question is: "How often do you discuss politics with your son or daughter?" (1) never, (2) a few times a year, (3) several times a month, (4) several times a week.

[34] It is possible that adolescents are influencing parents. But if this were the case, correlations should not be so strongly affected by adolescent perceptual uncertainty, unless there is a functional relationship be-

Table 3. Parent-Adolescent Product/Moment Correlations Within-Categories of Perceptual Accuracy

Attitude Objects	Perceptual Accuracy[a]		
	Uncertain	Medium Accurate	Accurate
Racial Integration	−.15 (107)	.30 (96)	.56 (115)
China Policy	−.08 (139)	.21 (65)	.51 (116)
Marijuana Laws	.12 (122)	.25 (90)	.60 (106)
Party Identification[b]	.01 (90)	—	.57 (229)

[a] Based on Table 2 (for the issues), the scores of zero and one for perceptual accuracy were combined for the uncertain category; the score of two equals the medium accurate category, and the scores of three and four were combined for the accurate category.
[b] Perceptual accuracy for party identification is a dichotomy.

Table 4. Parent-Adolescent Issue Attitude and Partisan Correlations Within-Categories of Issue and Partisan Salience to the Parent

Attitude Object	Parental Issue Salience		
	Low	Medium	High
Racial Integration	.00 (64)	.27 (127)	.48 (125)
China Policy	.03 (73)	.25 (142)	.44 (100)
Marijuana Laws	−.21 (35)	.35 (71)	.48 (211)
	Parental Partisan Salience		
	Independent[a]	Weak Partisan	Strong Partisan
Party Identification	.28 (163)	.31 (73)	.68 (83)

[a] Includes both "pure" Independents and Independents who "lean" toward either the Republicans or the Democrats.

Family Interaction. Family interaction variables were criticized earlier as being too general to have much impact on successful transmission.

Hypothesis 4: Conditions of family interaction will not affect the success of attitude transmission.

I have chosen for analysis two of the most frequently used family variables—family authoritarian ideology and parent-child affective relations. Many students of the family believe that authoritarian as opposed to permissive child-rearing will affect the success of attitude transmission. Empirically, Jennings and Niemi, as well as Middleton and Putney, report a curvilinear relationship between parental authoritarianism and successful transmission.[35] Other investigators, however, have reported no relationship.[36] The measure of family authoritarian ideology used here is a five-item scale taken from Ernhart and Loevinger.[37] The data support hypothesis 4. The within-category correlations display no patterned distribution, either linear or curvilinear.[38]

It is also commonly thought that warm parent-adolescent interpersonal relations should lead to a desire by the adolescent to adopt the values and attitudes of the parent. Jennings and Niemi found parent-adolescent affective ties had no influence on the success of transmission. Middleton and Putney, on the other hand, report a positive effect.[39] The parental "attractiveness" scale employed here is operationalized by items in-

tween adolescent uncertainty over parental attitudes and parental uncertainty over adolescent attitudes. Any conclusive test would require longitudinal data. Another nonrejectable alternative is the possibility that parents and peers have attitudes which are strongly related. If this were the case, it could be argued that the adolescent's attitude is due to peer influence.

[35] Jennings and Niemi, p. 181, and Russell Middleton and Snell Putney, "Political Expression of Adolescent Rebellion," *American Journal of Sociology*, 68 (March, 1963), 532–533.

[36] Philip Nogee and Martin Levin, "Some Determinants of Political Attitudes among College Voters," *Public Opinion Quarterly*, 22 (Winter, 1958–1959), 460.

[37] Claire B. Ernhart and Jane Loevinger, *Authoritarian Family Ideology: A Measure, Its Correlates, and Its Robustness* (Austin, Texas: Society for Multivariate Experimental Psychology, 1969).

[38] A four- as opposed to three-category breakdown is reported because any curvilinear pattern is more likely to be evident in the former. The results with three categories reflect the same pattern.

[39] Jennings and Niemi, p. 181, and Middleton and Putney, pp. 532–533.

Table 5. Parent-Adolescent Product/Moment Correlations Within-Categories of Authoritarianism for the Issues and Party Identification

Attitude Objects	Parental Authoritarianism			
	High	Med-High	Med-Low	Low
Racial Integration	.32 (62)	.31 (85)	.24 (93)	.38 (66)
China Policy	.16	.23	.31	.35
Marijuana Laws	.55	.32	.32	.36
Party Identification	.31	.58	.40	.46

Table 6. Parent-Adolescent Correlations Within-Categories of Parental Attractiveness for the Issues and Party Identification

	Parental Attractiveness		
Attitude Object	Low	Medium	High
Racial Integration	.17 (76)	.36 (167)	.42 (67)
China Policy	.14	.26	.41
Marijuana Laws	.46	.37	.46
Party Identification	.24	.58	.52

quiring into how "close" the adolescent feels to the parent, and how much the adolescent "admires" the parent.

It is apparent that, contrary to hypothesis 4, attractiveness does in some instances have an effect on successful transmission. But the pattern is unstable. The transmission of attitudes toward marijuana laws seems impervious to changes in attraction. On the other hand, there is a moderately strong effect on the other two issues and party identification (although there is a mild reversal in the case of the latter).

Multivariate Models

Statistical Methodology

Both issue salience[40] and perceptual accuracy, taken by themselves, have effects on successful transmission as strong or stronger than any variable reported in past research. In this section it shall be determined how these variables, along with "attractiveness," operate together. This will require a multivariate statistical model. The model employed is a test of the general linear hypothesis for multiple covariance. The specific form of the model used is somewhat complex going a step beyond the analysis of covariance. Conceptually, the dependent variable is parental influence. Operationally, this is not a single variable, but a relationship between two variables. Its indicator is the regression of the adolescent attitude on the parent attitude. Analysis of covariance would test the effect of a categorical variable on the regression intercept. First order statistical interaction, or nonadditivity, exists if the slope of the relationship between parent and adolescent attitudes is dependent on a second (or *n*th) independent variable. Clearly, the hypotheses predict this interaction. That is, within categories of a second independent variable—for example, issue salience—the slope of the parent-adolescent attitude relationship should increase as issues become more salient to parents. Consequently, the first-order interaction will be built into the statistical model. This model is the summary analog of comparing relationships within categories of a specification variable or variables—the method used to test hypotheses in the previous section.[41] The procedure might be thought of as a two-way analysis of variance, but instead of means, the cell entries are regression coefficients.[42] As in all tests of the general linear hypothesis, a maximally predictive equation is fitted to the data, and against it are tested less complex equations. If the F-ratio resulting from a comparison of these models is statistically significant, it means the additional terms in the more complex model make a difference.[43] The procedure, in addition, yields multiple correlation and partial regression coefficients.

Issue Salience and Perceptual Accuracy

The effect of issue salience and perceptual accuracy on parental influence are combined in a number of models, presented in Figure 1, which are consistent with my conceptual assumptions. I am assuming parental influence to be the de-

[40] From this point forward, the term "issue salience" will be used to refer to partisan salience as well as issue salience.

[41] The general form of model is:

$$Y = a_0 + a_i + b_0X_j + b_jX_j + e,$$

where a_0 represents the grand mean, a_i represents the deviation of the grand mean from the kth category, b_0X_j is the regression of Y on the interval independent variable which is common to all categories, b_jX_j represents the deviation of the optimum regression function in the kth category from the common slope. The question of interest is whether or not the common regression is parallel to the within-category regression. If they *are* parallel, it means that categorizing the parent attitude (by, for example, issue salience) has no effect. If the common regression slope and the slope computed within categories *are not* parallel, it means the regression within categories provides the line of best fit. The sums of squares due to fitting the line within the categories will thus be greater than the sums of squares due to the common regression. The explained variance and the correlation coefficient will increase because of this nonparallelism. See John E. Overall, "Multiple Covariance Analysis by the General Least Squares Regression Method," *Behavioral Science*, 17 (May, 1972), 313–320.

[42] Most readers are no doubt aware that two-way analysis of variance computed by standard methods requires equal cell frequencies. Unbiased estimates are usually obtained, when cell frequencies are not equal, by using the weighted squares of means approximation. The multiple covariance linear model is the mathematical equivalent of this approximation. James Fennessey, "The General Linear Model: A New Perspective on Some Familiar Topics," *American Journal of Sociology*, 74 (July, 1968), 4.

[43] Tests of statistical significance based on *F*-ratio's are not appropriate for these data because the least squares assumption of "independence" in independent random samples is not met (183 adolescents are paired with 322 parents). This means that the degrees of freedom for the residual sums of squares cannot be estimated in an unbiased manner. Consequently, the mean square residual (the denominator in the *F*-ratio) cannot be computed. The sums of squares, the cross-products, and the degrees of freedom for the numerator are, however, unaffected.

pendent variable, and that issue salience is antecedent to perceptual accuracy. These assumptions are not unreasonable. It is difficult to imagine parental influence as a determinant of the other two variables.[44] Also, conditions in which perceptual accuracy on the part of the adolescent might lead to issue salience for the parent are difficult to fathom.

One intuitively suspects that perceptual accuracy is largely a function of issue salience. If an issue is important to the parent, it will be discussed or commented on within the confines of the family, and other family members will become aware of that particular parental attitude.

[44] If, however, parental influence were indirect (for example, overprotected adolescents being conservative on China policy) it is possible that this indirect influence could lead to adolescent perceptual accuracy. Adolescents, because they are influenced, could be motivated to learn the parent's attitude. But if parental influence is direct, as is argued for the attitudes studied here, it is very unlikely that parental influence could be antecedent to adolescent perceptual accuracy.

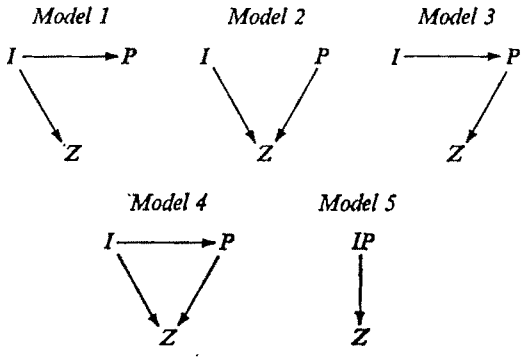


Figure 1. Models of Interrelationships Among Issue Salience, Perceptual Accuracy and Parental Influence

I = Parental Issue Salience
P = Adolescent Perceptual Accuracy
Z = Parental Influence on the Adolescent Attitude

Model 1: This model posits I with a direct effect on Z, and a spurious relationship between P and Z. The two latter covary only because of the common influence of I.

Model 2: Model two posits no relationship between I and P, but with each having an independent, additive effect on Z.

Model 3: The developmental sequence. I leads to P which in turn leads to Z.

Model 4: The developmental sequence, but with I having an independent effect on Z in addition to its effect through P.

Model 5: I and P act on Z in a nonadditive manner.

Table 7. Product-Moment Correlations Between Parental Issue Salience and Adolescent Perceptual Accuracy for the Issues and Party Identification

Attitude Object	Correlation Between Issue Salience and Perceptual Accuracy
Racial Integration	.25
China Policy	.22
Marijuana Laws	.25
Party Identification	.23

Hypothesis 5: The greater the issue salience to the parent, the more accurate the adolescent perception of the parental attitude.

The data only marginally support hypothesis 5. There is unquestionably a relationship, but it is obvious that factors other than issue salience are responsible for a considerable amount of the variance in the perceptual accuracy of the adolescent. Nevertheless, while the correlations between issue salience and perceptual accuracy can best be described as modest, they are consistent across all three issues and party identification, and are strong enough to warrant rejection of Model Two.

The multiple covariance model will now be used to analyze the independent and combined effects of issue salience (I) and perceptual accuracy (P) on parental influence (Z), where $Z = a_0 + b_0X$ (the regression of the adolescent attitude on the parent attitude).

$$\overset{3}{} \quad \overset{3}{} \quad \overset{9}{}$$

Hypothesis 6: $Y = a_0 + a_i + a_p + a_{ip} + b_0X$

$$\overset{3}{} \quad \overset{3}{} \quad \overset{9}{}$$

$$+ (b_i + b_p + b_{ip})X + e.$$[45]

With the additive effects of issue salience and perceptual accuracy held constant, parental influence is affected by a nonadditive combination of issue salience and perceptual accuracy.

$$\overset{3}{} \quad \overset{3}{} \qquad \overset{3}{} \quad \overset{3}{}$$

Hypothesis 7: $Y = a_0 + a_i + a_p + b_0X + (b_i + b_p)X + e.$

With issue salience held constant, parental influence is affected by perceptual accuracy.

$$\overset{3}{} \quad \overset{3}{} \qquad \overset{3}{} \quad \overset{3}{}$$

Hypothesis 8: $Y = a_0 + a_p + a_i + b_0X + (b_p + b_i)X + e.$

With perceptual accuracy held constant, parental influence is affected by issue salience.

[45] In these equations the a's are the intercepts and the b's are the regressions. The a's are necessary for unbiased estimates of the within-category slopes (their effects on Y are partialed out). The numerals above the a's and b's are the number of categories used in constructing the dummies and the first-order interaction terms (although perceptual accuracy for party identification has two categories rather than three).

Table 8. Effects of Issue Salience and Perceptual Accuracy on Successful Parent-Adolescent Attitude Transmission

	Explained Variance (Explained S.S./d.f.)	Multiple Correlation.
Racial Attitude		
Parent Attitude	228.0	.32
Test of Hypothesis 7: Effect of Perceptual Accuracy, Control Issue Salience		
Issue Salience	37.5	.38
Perceptual Accuracy, Control Issue Salience	99.5	.48
Test of Hypothesis 8: Effect of Issue Salience, Control Perceptual Accuracy		
Perceptual Accuracy	113.0	.44
Issue Salience, Control Perceptual Accuracy	24.0	.48
Interaction	15.0	.51
China Attitude		
Parent Attitude	97.0	.27
Test of Hypothesis 7: Effect of Perceptual Accuracy, Control Issue Salience		
Issue Salience	21.5	.32
Perceptual Accuracy, Control Issue Salience	67.5	.45
Test of Hypothesis 8: Effect of Issue Salience, Control Perceptual Accuracy		
Perceptual Accuracy	73.5	.41
Issue Salience, Control Perceptual Accuracy	15.5	.45
Interaction	7.0	.46
Marijuana Attitude		
Parent Attitude	411.0	.40
Test of Hypothesis 7: Effect of Perceptual Accuracy, Control Issue Salience		
Issue Salience	48.0	.44
Perceptual Accuracy, Control Issue Salience	47.0	.48
Test of Hypothesis 8: Effect of Issue Salience, Control Perceptual Accuracy		
Perceptual Accuracy	62.0	.45
Issue Salience, Control Perceptual Accuracy	33.0	.48
Interaction	8.0	.49
Party Identification		
Parent Attitude	165.0	.48
Test of Hypothesis 7: Effect of Perceptual Accuracy, Control Issue Salience		
Issue Salience	9.5	.50
Perceptual Accuracy, Control Issue Salience	27.0	.54
Test of Hypothesis 8: Effect of Issue Salience, Control Perceptual Accuracy		
Perceptual Accuracy	32.0	.52
Issue Salience	7.0	.54
Interaction	0.5	.54

The information necessary to test these hypotheses and reject the remaining models is presented in Table 8.[46]

Hypothesis 6 and the accompanying Model Five can first be rejected. This model posits interaction, but it is apparent from Table 8 that the effects of *I* and *P* are strongly additive. When these effects are controlled, the increase in the multiple correlation coefficient due to the second-order interaction is quite small.

Examining the variance explained by issue salience and perceptual accuracy, it is obvious that the latter has the far greater impact on parental influence. Model One would be consistent with the data only if a control for issue salience very sharply reduced the effect of perceptual accuracy. One can see this is definitely not the case. With issue salience controlled, perceptual accuracy retains a strong impact.[47] The arrow between *P* and *Z* cannot be removed, and Model One must be rejected (hypothesis 7 is supported).

Remaining is Model Three, a developmental sequence, and Model Four, the same but with *I* having an independent effect in addition to being part of a developmental sequence. Both issue salience and perceptual accuracy, taken by themselves, have a strong influence on successful transmission. Controlling for perceptual accuracy reduces by 26–36 per cent the variance explained by issue salience. This is evidence for the operation of a developmental sequence, as depicted in Model Three.

Finally, it can be asked whether issue salience has an independent as well as a developmental

[46] The information presented in Table 8 will be used to test hypotheses 6, 7, and 8, and to partition the explained variance between issue salience and perceptual accuracy. Total explained variance for the partitioning analysis is that variance explained by issue salience and perceptual accuracy. It is important to note that by the terms "issue salience" and "perceptual accuracy" is meant the parental attitude categorized by these two variables, or the effect of the two variables on the parent-adolescent regression (parental influence).

[47] Using the race issue as an example, entering perceptual accuracy immediately after the parent attitude increases the correlation coefficient from .32 to .44. Entering perceptual accuracy after the parent attitude and issue salience (issue salience controlled) increases the correlation from .38 to .48.

effect. If controlling for *P* substantially wipes out the variance explained by *I*, we know there can be no arrow between *I* and *Z*. The data show that with *P* controlled, *I* accounts for at least seventeen and as much as thirty-five per cent of the explained variance. While not high, it seems to me that this impact, being present for all three issues and party identification, is large enough to attribute an independent effect to issue salience (hypothesis 8 is supported).[48] It is concluded, therefore, that Model Four best fits the data. In this model, issue salience, which is conceptualized as being antecedent to perceptual accuracy, accounts for approximately twenty-five per cent of the explained variance (through both independent and developmental effects) and perceptual accuracy accounts for the remaining approximately seventy-five per cent (although the split for the marijuana issue is about fifty-fifty).[49] With all the terms in the maximally predictive equation, the multiple correlation coefficients are quite similar for all attitudes.

Direct Effects of the Parental Attitude. A comparison can now be made of the individual, direct effect that parental party identification and parental attitudes on race, China, and marijuana have on the same adolescent attitudes when issue salience and perceptual accuracy are held constant. This requires computation of the partial regression coefficient. Reported in Table 9 are the unstandardized coefficients (b's) which, according to Blalock, are generalizable across populations.[50] Since the issues and party identification are measured with different scales, the standardized coefficients (beta weights) are also reported. These latter coefficients, which will be interpreted, represent the change that would occur in the adolescent attitude if the parent attitude were to change by one standard unit, with issue salience and perceptual accuracy controlled. The beta weights (see table 9) for the effect of the parent attitude on the adolescent attitude, with issue salience and perceptual accuracy held constant, are similar enough for all issues and party identification to warrant the assertion of a rough general explanation for the successful transmission of political attitudes from parent to child.

[48] In the case of the race issue, entering issue salience immediately after the parent attitude increases the correlation from .32 to .38. Entering issue salience after the parent attitude and perceptual accuracy (perceptual accuracy controlled) raises the correlation from .44 to .48.

[49] The variables are entered in the order called for by the equation for hypothesis seven.

[50] Hubert M. Blalock, Jr., "Causal Inferences, Closed Populations, and Measures of Association," *American Political Science Review*, 60 (March, 1967), 130–136.

Table 9. B's and Beta Weights for the Effect of the Parent Attitude on the Adolescent Attitude with Issue Salience and Perceptual Accuracy Controlled[a]

	Beta	*B*	Std. Error
Racial Integration	.52	.52	.10
China Policy	.46	.45	.09
Marijuana Laws	.55	.58	.09
Party Identification	.54	.45	.04

[a] Additive effects only.

Table 10. Multiple Correlation with Attractiveness Added to the Equation for Hypothesis Seven

Attitude Object	Multiple Correlation
Racial Integration	.49 (+.01)[a]
China Policy	.47 (+.02)
Marijuana Laws	(Not computed due to lack of linear progression within categories)
Party Identification	.57 (+.03)

[a] Increase in the multiple correlation coefficient when attractiveness is added to the equation for hypothesis 7.

Parental Attractiveness

Finally, parental attractiveness can be added to the equation for hypothesis seven.[51] Operating by itself, attractiveness was found to have moderately strong effects on two issues and party identification. Because of its inconsistent impact and because its proves to be only weakly related to either perceptual accuracy or issue salience,[52] attractiveness was excluded from the model until the combined effects of issue salience and perceptual accuracy had been completed. Nevertheless, when added to the equation, it may explain enough additional variance to warrant substantive interpretation.

$$\textit{Hypothesis 9: } Y = a_3 + a_i + a_p + a + b_t X + (b_i + b_p + b_a) X + e.$$

With *I* and *P* held constant, parental influence is affected by attractiveness.

The data reveal that, with a reservation, hypothe-

[51] If the variable "attractiveness" is added to the equations for either hypothesis 7 or hypothesis 8, the results will be statistically identical.

[52] The correlations between attractiveness and perceptual accuracy for the attitudes toward racial integration, China policy, marijuana laws and party identification are .08, .09, —.05 and .05, respectively; for attractiveness and issue salience they are .01, .00, .12, and —.05, respectively.

sis nine can be rejected. Attractiveness has no impact on the successful transmission of the attitude toward marijuana laws. The impact it has on attitudes toward the race issue, China policy and party identification[53]—while small—prevents dismissing this variable or others like it (nonissue specific) as being completely inconsequential.[54]

Conclusion

In recent years, students of political socialization have downplayed the family as a source of attitudes toward public policy issues. This conclusion is not unwarranted given the data. But it would seem not to be an inherent characteristic of parent-child attitude transmission. Rather, quite the opposite appears to be the case. Parents have an inherent potential for successful transmission. Whether this potential is realized depends basically on the distribution within the population of (1) issue salience to the parent, and (2) adolescent perceptual accuracy of the parent attitude. These variables are *specific* to the individual attitude objects, as opposed to variables of the family interaction variety or "political interest" which are *general* to all attitude objects. Selected for analysis were attitude objects different enough to allow a fairly stringent test of any general thesis. One was party identification, to which some investigators have attributed unique transmissible characteristics. The others were substantive issues. One of the issues (marijuana laws) was deliberately chosen with the idea that it would be susceptible to countervailing peer-group influence. But when issue salience and perceptual accuracy are held constant in a multiple covariance equation, the beta weights indicating the success of transmission are very similar for all the attitudes that were studied. It may be tentatively concluded, therefore, that the influence of parents on the party identification and public policy attitudes of their children can be explained by a general equation; and that the level of parental influence, at any one point in time, will be highly dependent on the distribution of issue salience and perceptual accuracy for the particular attitude object in question.

[53] It should be noted from Table 6 that there is a reversal for the within-category progression for party identification. The statistical model will make no adjustment for this fact. If we assume conceptually that the progression must be linear, then the model overestimates the impact of attractiveness on the transmission of party identification.

[54] It should be kept in mind that what is being discussed is the independent effect of attractiveness on successful transmission *after* the effects of issue salience and perceptual accuracy have been partialed out.

Appendix

Opinion Items Measuring Attitudes Toward Issues

Racial Integration of the Public Schools

1. It is desirable to have racially integrated schools in the United States.
2. The Government should stay out of the question of whether blacks and whites go to the same school.
3. In places where both blacks and whites live, the Government should make sure that there are both black and white children in all the public schools.
4. Busing should be used to integrate the public schools.

United States Policy toward Communist China

1. The United States is getting too friendly with Communist China.
2. The United States Government should have more firmly opposed letting Communist China into the United Nations.
3. The United States should send ambassadors to Communist China and give the Chinese diplomatic recognition.
4. The United States should engage in greatly expanded trade with Communist China.

Laws Regulating the Use of Marijuana

1. The Government is too easy on those caught with possession of marijuana.
2. The laws dealing with those caught with possession of the marijuana are too tough.
3. Anyone caught with possession of marijuana should spend some time in jail.
4. The use of marijuana should be made legal.

Majority Party Leadership and the Committee System in the House of Representatives*

LOUIS P. WESTEFIELD
Southern Illinois University, Edwardsville

Students of congressional behavior are interested in party leadership, leadership management strategies, and the committee system.[1] Such themes are the concerns of this paper. Party leadership strategies and the committee system of the House are examined in concert to provide a very elementary, yet explicit, theory about one strategy of the majority party leaders with respect to a simple but largely ignored phenomenon, change in size of the standing committees of the House.[2]

The theory deals with the use of the committee system as a resource by the party leaders. Briefly, the argument is that the leaders pursue a strategy of accommodation. That is, the leaders attempt to accommodate member demands for committee positions in order to gain leverage with the members. To gain leverage the leaders must guarantee a steady supply of resources—i.e., party positions on committees.

The theory builds on several relatively clear concepts such as compliance, quality of assignment, expectation, and scarcity. The resulting argument orders and provides an interpretation for some elementary observations of leadership behavior. Furthermore, it is shown that distinguishing between the short-run and the long-run is useful theoretically, i.e., short-run decisions have long-run consequences.

Theory

Consider the majority party leaders.[3] What part do they play in the establishment of the sizes of the standing committees? Formally the task of making committee assignments falls to the respective party committee-on-committees. Since the Democrats, by virtue of their majority status, organized most of the Congresses in the period we shall examine, the responsibility for majority party assignments has most often fallen upon the Democratic members of the Ways and Means Committee.[4] In practice the literature on committee assignments suggests that the position of the party leaders determines the outcomes (the assignments) should they, the leaders, choose to exercise their influence.[5] Their position is prominent particularly in assignments to the better committees. Very little, however, is known about the process of determining party ratios on committees and committee size.[6] Therefore, we are forced to infer from the literature on committee assignments that the majority party leaders also make the decisions about the sizes of the committees. That is, leaders of both parties may negotiate about the establishment of party ratios and size,

* I would like to express my gratitude to James W. Davis, Adam Przeworski, Robert Salisbury, Kenneth Shepsle, and John Sprague for their advice and helpful comments. In addition, I owe a very special and substantial debt to John Sprague who gave freely of his time and insight throughout this project. And while none of these individuals may wish to be associated with the arguments and interpretations of this paper, much of what may be useful is the result of their efforts. Finally, I would like to thank the two anonymous referees for their suggestions.

[1] See, for example, Randall B. Ripley, *Party Leaders in The House of Representatives* (Washington: Brookings Institution, 1967); Charles O. Jones, *The Minority Party in Congress* (Boston: Little, Brown, 1970); Randall B. Ripley, *Majority Party Leadership in Congress* (Boston: Little, Brown, 1969); and Lewis A. Froman, Jr. and Randall B. Ripley, "Conditions for Party Leadership: The Case of the House Democrats," *American Political Science Review*, 59 (March, 1965), 52–63.

[2] Gawthrop is one of the few scholars to point out this phenomenon. However, his primary concern was to describe the changing patterns of multiple committee assignments in the House. See Louis C. Gawthrop, "Changing Membership Patterns in House Committees," *American Political Science Review*, 60 (June, 1966), 366–373.

[3] For the purposes of this paper, "majority party leaders" will mean the Speaker, the Majority Leader, and the Majority Whip.

[4] Their decisions are voted on by the Democratic Caucus. This final approval, however, is most often a *pro forma* action. See David W. Rohde and Kenneth A. Shepsle, "The Committee Assignment Process: A Case Study of Social Choice" (paper presented at the 67th annual meeting of the American Political Science Association, Chicago, 1971).

[5] Nicholas A. Masters, "Committee Assignments in the House of Representatives," *American Political Science Review*, 55 (June, 1961), 345–357; Ripley, *Party Leaders in the House of Representatives*, pp. 57–61; Charles L. Clapp, *The Congressman: His Work As He Sees It* (Washington: The Brookings Institution, 1963), pp. 183–212; and John F. Manley, *The Politics of Finance: The House Committee on Ways and Means* (Boston: Little, Brown, 1970), p. 24.

[6] On the temporal sequence of the assignment process, see Kenneth A. Shepsle, "A Model of the Congressional Committee Assignment Process: Constrained Maximization in an Institutional Setting" (paper presented at the 69th annual meeting of the American Political Science Association, New Orleans, 1973).

but the positions of the majority party leaders are pre-eminent and decisive.[7] Consequently, assume: The majority party leaders are decisive in decisions about committee size.

This assumption is a necessary condition for the remainder of the argument. To be sure, the Legislative Reorganization Act of 1946 provides guidance about matters of committee size and the party ratios on committees. But the sizes of committees may be altered and still satisfy the condition that a division on committees should reflect the party ratio in the chamber. Furthermore, the argument will be evaluated in terms of a long time series, a substantial part of which predates the 1946 reorganization.

Our theory has congressmen in it, and we must specify what they want, i.e., provide a motivation for the actors. Since we are dealing with majority party strategy, the universe is the members of the majority party in the House. We partition these members into two groups, the party leaders and the rank-and-file members. What is it, then, that these actors want? We shall impute the following motivation to the followers:

The members want "good" assignments. Because the demand for "good" assignments most often exceeds the supply, these resources are scarce. The members want these scarce resources.

This assumption provides a motivation for the members which establishes an economic condition: "good" positions are scarce; the demand for these positions exceeds the supply. The status of this assumption, and of the other assumptions of the theory as well, is that of a statement asserted to be true for the purposes of the argument. However, these assumptions of motivation are plausible and correspond to what is known from the literature.[8] (The crucial shortage of desirable positions will be considered more fully later in the text.)

With respect to this first assumption, Masters's study of the committee assignment process can aid intuition. In discussing the Democratic Committee-on-committees, he observes that committee positions are indeed scarce: "The volume of work before the Committee varies, depending chiefly on the changes resulting from the preceding election. Almost always, however, there are more applications than vacancies; in the 86th Congress 124 applications were made for 75 places to be filled."[9] Thus, there is evidence for assigning to committee positions an economic quality, namely scarcity.

We also furnish the majority party leaders with a motivation:

The majority party leaders want compliant behavior on the part of the members. They perceive that by accommodating the followers (dispensing "good" assignments) they, the leaders, can reward past loyalty or encourage such behavior in the future.

We elect to deal with party leaders in terms of their perceptions because we are not concerned at this point whether, in fact, this strategy does lead to compliant behavior on the part of the members. Nor are we concerned with the strategies the members might pursue to gain desirable assignments. True, a motivation is provided for the followers, but the theory is one of strategies of the party leaders. Therefore, we merely assume the leaders believe in such a strategy and act on the basis of this belief. (It should be emphasized that this argument treats the goal of the party leaders as general control over the members or at least over a large number of members and not policy specific control.) Once more Masters's study is instructive, for it asserts that bargaining and rewards for compliance characterize the assignment process: "Party leaders, working in conjunction with their committee-on-committees, use assignments to major committees to bargain with the leaders of party groups or factions, in order to preserve and fortify their leadership positions and conciliate potential rivals, as well as to reward members who have cooperated."[10] Randall Ripley, sorting out the Democratic committee assignments in the 88th Congress, reaches much the same conclusion.[11] Thus, this motivational assumption also is plausible and it accords with what is known from the literature.

One qualitative distinction has been posited for the political actors and a motivation provided for each—the leaders and the followers. In this argument committee positions are given the status of a currency, a basis of exchange between the leaders and followers. The leaders desire compliant behavior on the part of the members. By dispensing committee assignments the leaders be-

[7] Democratic vacancies on Ways and Means are filled by the Democratic Caucus. Ways and Means has been included as a committee to be observed because we assume the pre-eminent position of the leaders also applies to this committee.

[8] The intent is not to raise the issue of the "realism" of the assumptions. Two classic statements by Friedman and Koopmans take opposite positions on this issue. See Milton Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), pp. 3–43; and Tjalling C. Koopmans, *Three Essays on the State of Economic Science* (New York: Mcgraw-Hill, 1957), pp. 129–166.

[9] Masters, p. 346. On scarcity of positions also see Richard F. Fenno, Jr., *The Power of the Purse: Appropriations Politics in Congress* (Boston: Little, Brown, 1966), Table 2.8 at p. 64; and Charles S. Bullock, III, "Committee Transfers in the United States House of Representatives," *Journal of Politics*, 35 (February, 1973), footnote 39 at p. 101.

[10] Masters, p. 357.

[11] Ripley, *Party Leaders in the House of Representatives*, p. 60. Also, see Manley, *The Politics of Finance*, p. 24.

eve they encourage compliance. Thus, having esources to allocate benefits the leaders; indeed, iore resources provide more opportunities to ain leverage with the members. Furthermore, the erceived marginal cost of increasing the supply f seats is always less than the perceived value of ie expected increase in compliance. That is, there always some committee which if increased in ze would increase expected compliance for some iembers.

What, then, are the consequences of these remises? First consider the supply of seats. Re-all that the majority party leaders can increase ieir resource base, i.e., they can fix the size of the anding committees. Therefore:

PROPOSITION I: The leaders expand their resource base by increasing the number of committee positions they can dispense.

Empirically, then, we are interested in the time ath of the total number of committee seats in the louse. Given the theory, this time path will be ome monotonically increasing function of time. his theoretical relationship is displayed in igure 1.

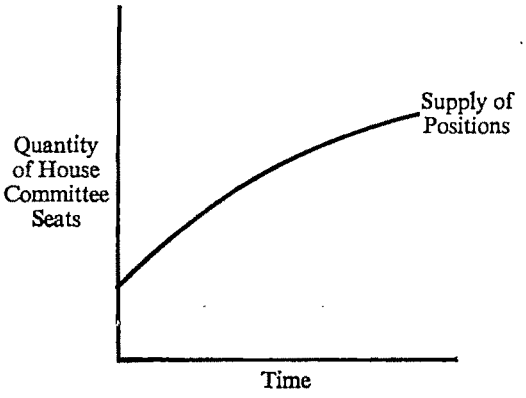


Figure 1. Representation of the Supply of Committee Seats: Dynamic on Time

Evaluation

Are the data consistent with the theory? The ata consist of the number of committee positions uring the period 1927–1971.[12] We begin with 927 because the number of standing committees i the House remained relatively unchanged from iat date until the Legislative Reorganization Act f 1946. And except for the creation of the Com-iittee on Science and Astronautics in 1959, the umber of committees remained unchanged from 947–1971. (The Committee on Standards of fficial Conduct is excluded.)

Now let us evaluate this theory first for the eriod following the Legislative Reorganization ct of 1946. Figure 2 displays the time path of the otal number of committee seats for the period 1947–1971. Even if the twenty-five seats created by the establishment of the Committee on Science and Astronautics are removed, the pattern is clear. The number of committee positions has increased since 1947 as an increasing function of time.[13]

Looked at from a slightly different perspective, the data again show a systematic pattern. The total number of committee seats at any point in time is obtained by aggregating over committees. The product of the number of committees at a point in time and the number of Congresses observed in a time period provides the total number of "opportunities" to observe a change in the size of each committee. Of the total "opportunities" in the period 1947–1971 there were less than a dozen instances (3.8 per cent of the total "opportunities") of a reduction in size of a standing committee. Furthermore, only one committee, Veterans' Affairs, experienced a net loss of seats during this period. Thus, aggregation does not obscure the results. The interpretation is straightforward; the data are consistent with the argument.[14]

The Legislative Reorganization Act of 1946 forces us to deal separately with the period 1927–

[12] All the data come from the available *Congressional Directory*, closest to the opening of each Congress, which lists committee assignments. In some cases was necessary to use the *Congressional Directory* r the second session of a Congress. Since the committee system was reorganized in 1946, beginning with 927 also provides two distinct time periods of approximately the same length. A list of the standing ommittees and their respective sizes may be found *Cannon's Precedents of the House of Representatives*, 3 (Washington: Government Printing Office, 935), 8–9. The Congresses listed there include the 0th-73rd Congresses. The Committee on Memorials 3 seats) was created in the 70th Congress. The Committee on Un-American Activities has been excluded om the period preceding the Legislative Reorganization Act of 1946. Prior to 1927, the number of positions grew even in the midst of periodic adjustments the committee system.

[13] It is worth noting that alteration of committee size occurred prior to the time period examined in this paper. "In making committee assignments for Republicans in the Fifty-ninth Congress, Cannon was faced with a problem of too few choice seats for too many Republicans. He finally reached an agreement with the Democratic Minority Leader, John Sharpe Williams of Mississippi, that would increase the size of the committees and leave the Democratic representation undisturbed" (Ripley, *Majority Party Leadership in Congress*, p. 28).

[14] One potential source of measurement error can be disregarded. The sizes of the committees may be altered during a Congress. However, the committee rosters for the second session of each Congress indicate that generally the size of committees is not altered within a Congress. And where an increase or decrease does occur, the committee is not changed to its former size at the beginning of the next Congress.

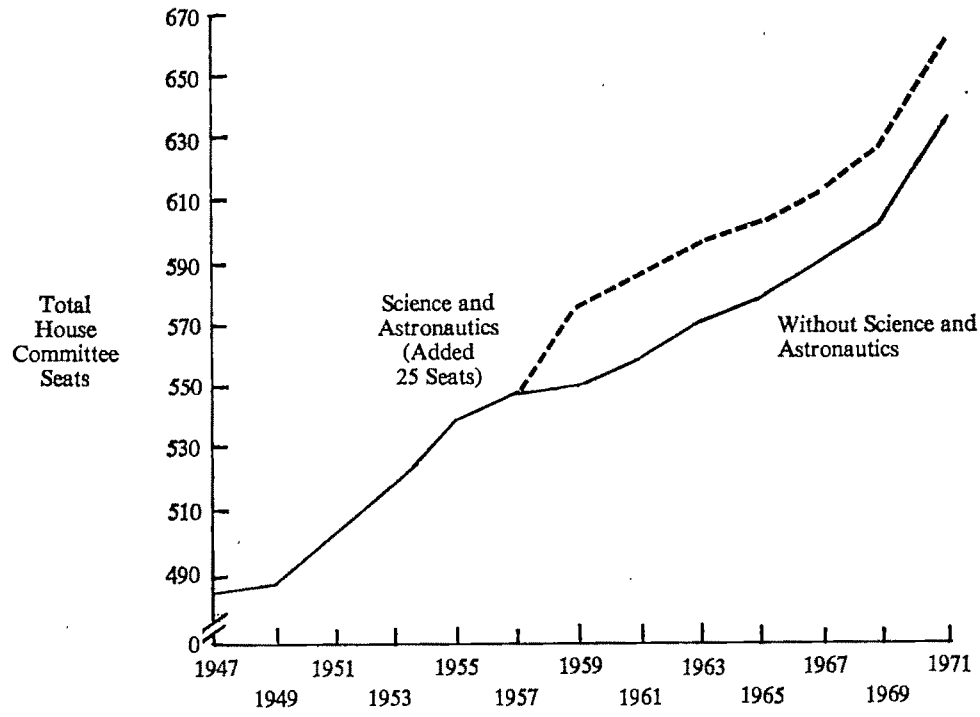


Figure 2. Increase in Seats of the Standing Committees of the House: 1947–1971

1945. But for the purposes of this analysis the reorganization is advantageous. We can extend the observations back in time and examine two distinct periods. Intuitively this earlier period should provide a much more difficult test of the argument. We know that there were a great many more committees and committee positions in the system during this period. Indeed, more than 830 positions existed in 1927 as opposed to slightly more than 480 in 1947. And with a large number of positions already in a system we might expect to find no evidence to support the argument.

Figure 3 displays the time path of the total number of committee seats for the period 1927–1945. Note again that the number of committee seats is an increasing function of time.[15] Thus, even in the midst of an abundance of positions, the leaders increase their resource base. And consider the "opportunities" to observe a reduction in committee size during this period. Again, there were less than a dozen cases of such reductions (1.4 per cent of the "opportunities"). Moreover, no committee experienced a net loss of seats during this period. Taking the two periods together, we note that there were less than twenty instances of reduction in the number of seats on committees; these cases represent 2.3 per cent of the total "opportunities."

An elementary economic argument about one leadership strategy has been constructed. The demands of the theory have been compared with observations for the two periods of time, and the data were found to be consistent with the argument.

Differential Valuation and the Distribution of the Increase in Seats Across Committees

Suppose the members prize assignments to some committees more than others. While the currency remains committee assignments, we now introduce *explicitly* the notion of differential valuation of committees.

The differential prestige of committees is established by at least two conditions: (1) formal rules and (2) the transfers of members from one committee to another. Transfers of members from one committee assignment to another have been used frequently to establish the hierarchy of committee prestige. And, as Bullock and Sprague

[15] There is an interesting interpretation to be made from the data of Figure 3. Note the rate of change in the interval 1931–1933. A party, long out of power and suddenly gaining majority status with an overwhelming numerical advantage may have strong incentives to expand rapidly the number of rewards early in the game. Were the Republicans suddenly to achieve majority status by a large margin the same phenomenon may occur. Note, however, that this interpretation does not fall out of the argument. The argument presupposes only a weak condition (an increasing function of time) on the time path of the total number of committee positions.

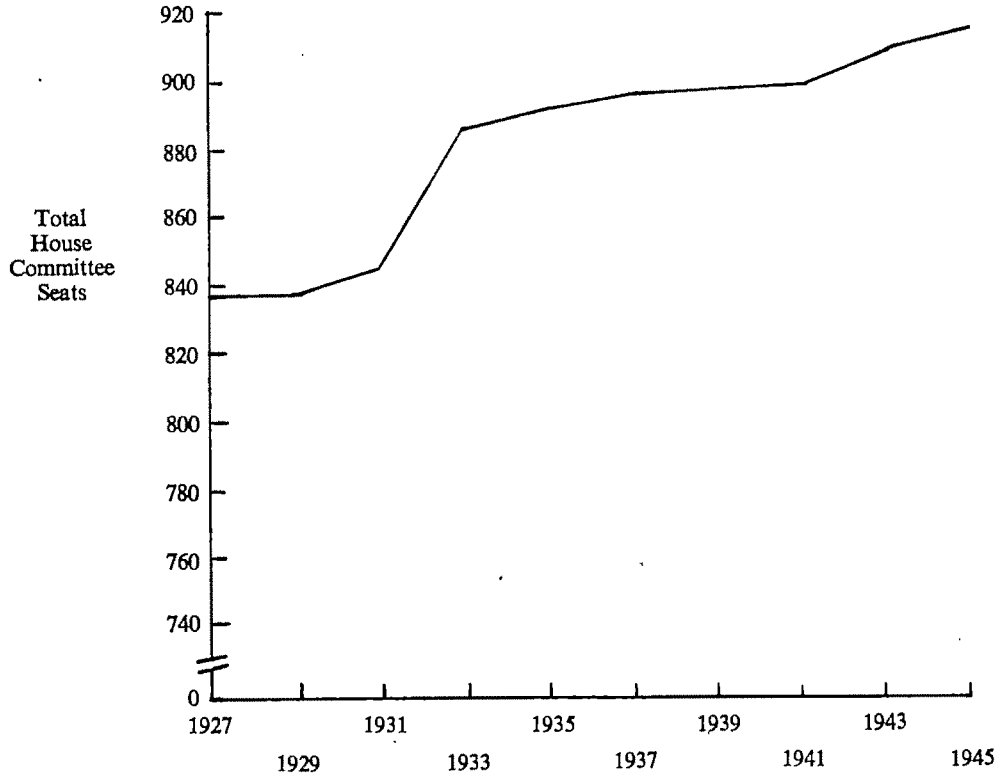


Figure 3. Increase in Seats of the Standing Committees of the House: 1927–1945

oint out, the formal rules dealing with privileges f committees and restrictions on multiple com- ittee assignments recognize at least ordered asses of committees.[16] That committees are ratified in terms of prestige or desirability is sig- ificant because the leaders can be expected to ld positions to committees in some nonrandom shion. What is the advantage to the leaders of eating new positions on those committees which e not valued by the members? Having desirable signments to allocate is what brings leverage to eal with members. Thus, we move from the con- deration of aggregate change (the time path of ie total number of committee positions) to an amination of the distribution of the increase in ositions across the hierarchy of committee estige.

First, some method of ordering the standing ommittees in terms of desirability or prestige is required. In order to produce such an ordering conveniently, we shall adopt the convention of Bullock and Sprague.[17] Their measure expresses the number of transfers to a committee relative to the number of departures plus transfers. Since desirable committees draw members from other committees and lose few members to other committees, the higher the value of the index, the more prestigious the committee. Bullock, using this method, provides a measure of committee prestige for the period 1947–1967.[18] This same method can be employed to produce a committee prestige ordering for the earlier period. Only the prestige ordering for the majority party during each period will be used, because this theory accounts only for accommodation within the majority party.[19]

[16] Charles Bullock and John Sprague, "A Research ote on the Committee Assignments of Southern Dem- ratic Congressmen," *Journal of Politics,* 31 (May, 969), 493. For one example of the use of com- ittee transfers, see George Goodwin, Jr., *The Little egislatures: Committees of Congress* (Amherst: Uni- rsity of Massachusetts Press, 1970), pp. 114–115. nd the positions on desirable committees are scarce. e footnote 9 above.

[17] Bullock and Sprague, pp. 496–501.

[18] Charles Bullock, "The Committee Assignments of Freshmen in the House of Representatives, 1947–1967" (dissertation, Washington University, St. Louis, 1968), p. 35.

[19] As we have noted, the Democrats controlled most of the Congresses during this time period. Hence, for our purposes, the majority party empirically will mean the Democrats. Furthermore, the prestige rankings of committees for Democrats and Republicans are empirically distinct. See Bullock and Sprague, pp. 496–501. Although theoretical reasons dictate the use of majority party rankings, use of rankings for Repub-

Table 1. Prestige Ranking of Committees for House Democrats: 1947–1967[a]

Ways and Means	1.000	Government Operations	.371
Rules	1.000	District of Columbia	.273
Appropriations	.983	Education and Labor	.250
Foreign Affairs	.852	House Administration	.243
Armed Services	.750	Interior	.241
Commerce	.743	Banking and Currency	.231
Judiciary	.708	Merchant Marine	.222
Agriculture	.541	Veteran's Affairs	.075
Public Works	.485	Post Office	.050
Science and Astronautics	.480	Un-American Activities	.000

[a] From Charles Bullock, "The Committee Assignments of Freshmen in the House of Representatives, 1947–1967" (Ph.D. dissertation Washington University, St. Louis, Missouri, 1968), Table 3 at p. 35.

The prestige rankings for the two periods are displayed in Tables 1 and 2. Although there may be some quarrel with the ranking of several minor committees in the earlier period (Printing and Library, for example), these rankings correspond to other evaluations of committee prestige.[20]

Therefore, assume the following:

A single ordering of committee prestige can represent the "average" preferences of the members, and this measure generally will be stable through each time period.

This assumption presupposes two conditions: (1) "average" preferences of the members can be assigned and (2) the "average" committee prestige ranking will be stable over time. It is clear that

licans or House members as a whole, both distinct from Democrats alone, will not dramatically alter the results reported below. This occurs because there is a correspondence between the rankings for the more desirable committees, the ones I shall argue take up most of the overall expansion in seats.

[20] For the modern period see Goodwin, pp. 114–116. For the earlier period see George B. Galloway, *The Legislative Process in Congress* (New York: Thomas Y. Crowell, 1955), p. 128; Floyd M. Riddick, *Congressional Procedure* (Boston: Chapman and Grimes, 1941), pp. 115–116; Chang-Wei Chiu, *The Speaker of the House of Representatives Since 1896* (New York: Columbia University Press, 1928), p. 69; and George B. Galloway, *Congress at the Crossroads* (New York: Thomas Y. Crowell, 1946), p. 90.

Table 2. Prestige Ranking of Committees for House Democrats: 1935–1945[a]

Ways and Means	1.000	Immigration	.259
Rules	1.000	Coinage	.250
Appropriations	.989	Labor	.250
Agriculture	.915	Flood Control	.222
Commerce	.877	Accounts	.200
Naval Affairs	.867	Irrigation	.192
Judiciary	.841	Expenditures	.174
Banking and Currency	.836	Insular	.174
Military Affairs	.800	Indian	.167
Printing	.750	Territories	.167
Foreign Affairs	.588	Enrolled Bills	.143
Election of President	.556	Public Lands	.130
Rivers	.550	Elections #1	.111
Executive Papers	.500	Pensions	.105
Library	.500	Civil Service	.100
District of Columbia	.440	Elections #2	.100
Merchant Marine	.421	World War Veterans	.069
Memorials	.333	Public Buildings	.031
Patents	.333	Claims	.000
Roads	.333	Elections #3	.000
War Claims	.333	Invalid Pensions	.000
Post Office	.317	Mines	.000
Census	.292	Revision of Laws	.000
Education	.263		

[a] Source: *Congressional Directory*. 73rd through 79th Congresses (Washington, D.C.: Government Printing Office, various dates).

partitioning the members of the House into various groups will produce somewhat different prestige orderings. Nevertheless, the correspondence between committee prestige rankings of various groups of House members is quite high.[21] Therefore, it is assumed that a single ordering of committee prestige can represent the "average" preferences of the members.

The second condition, in effect, eliminates any consideration of change in preference over time. Of course, the longer the time period the more dubious is the standing of this assumption, for the probability of obscuring changes in preference surely increases as the time period lengthens. In practice this assumption demands the stability of each ordering over approximately a twenty year period, 1927–1945 and 1947–1971. The prestige measure for the 74th–79th Congresses will represent the ordering in the former period, and the measure for 1947–1967 will represent the prestige hierarchy in the latter period.

We now need to specify the particular set of members for whom the measures are said to hold. Because this is a theory of majority party leadership behavior, we assume the following:

The leaders of the majority party are concerned, first and foremost, with the committee preferences of their party members.

This assumption seems quite reasonable and needs little further comment. To be sure, the leaders may consider the needs of their colleagues in the minority party, particularly on a case by case basis. Still, we offer as a rule the notion that majority party leaders are more interested in the desires of their party's rank and file.[22]

One final assumption is required; it specifies the amount of information in the system:

The majority party leaders know the committee preference ordering of their party members.

The leaders, though most often not the most senior members of their party, nevertheless have accumulated many terms of service and generally have had ample opportunity to learn the relative desirability of the various standing committees. Thus, we demand of the leaders of the majority party the ability to reproduce the "average" committee prestige ranking of their party members.

As we have seen, reductions in the sizes of the standing committees have been most infrequent. Thus, given the notion of differential valuation of committees by the members, we can now examine the distribution of the total increase in committee positions across the prestige ranking of committees.

Suppose the leaders estimate expected compliance on the part of a member as a function of the prestige of the *new* seats to be created. We can suppose that expected compliance varies directly with prestige of the new seat. That is, the leaders expect greater compliance as the prestige of the new seat to be created increases. But increasing the number of positions reduces the total value of seats as resources. Specifically, the total value of these resources declines as a function of the prestige of the newly created seat. Adding seats to the very best committees reduces the value of total resources more than does adding positions to somewhat less prestigious committees. Thus, benefits (member compliance) increase with the expected prestige of the new position. But costs (depletion of seat value) also increase with the prestige of the new seat to be created. The economic interpretation, involving exchange and scarcity, is crucial. If the currency (the assignments) were not scarce and if anyone could have the assignment he desired, then seats would not constitute leadership resources.

On this argument, then, we can anticipate where the growth in committee positions will occur:

PROPOSITION II: The leaders, in expanding their resource base, will concentrate the increase in the middle to upper range of the committee prestige ordering.

Empirically this means that a relative frequency distribution of the total increase in seats across committees will take the form displayed in Figure 4. This form results from the preceding argument and from the process of aggregating over a number of leaders and members. That is, a leader's estimate of the average minimum price in prestige to secure minimally acceptable expected compliance varies from one member to another. And one leader's estimates are likely to differ from those of another leader. Rayburn's estimates may not be the same as Albert's estimates. Hence the empirical distribution should have a mean at the higher prestige level with values distributed around that mean.

The interpretation is straightforward. Why increase the number of positions on committees that are not valued by the members? After all, such committees earn their low prestige standing through their inability to hold current members and attract new members from other committees. Moreover, why dispense more of the most prized positions? Expanding only the very best committees rapidly depletes the value of these leadership

[21] See Bullock and Sprague, pp. 496–501.

[22] It is impossible to reconstruct the particular circumstances of the creation of each new seat. For example, a new minority position might be created to reflect a new party ratio and to prevent a reduction in majority positions. Such an action is consistent with the argument.

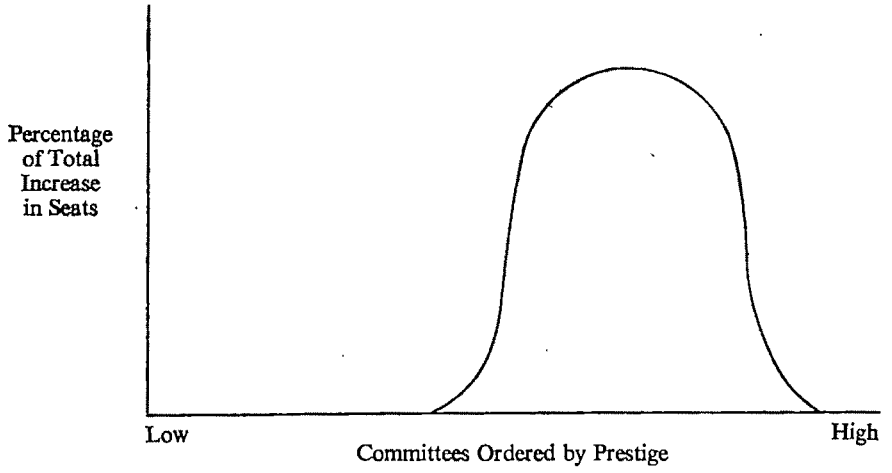


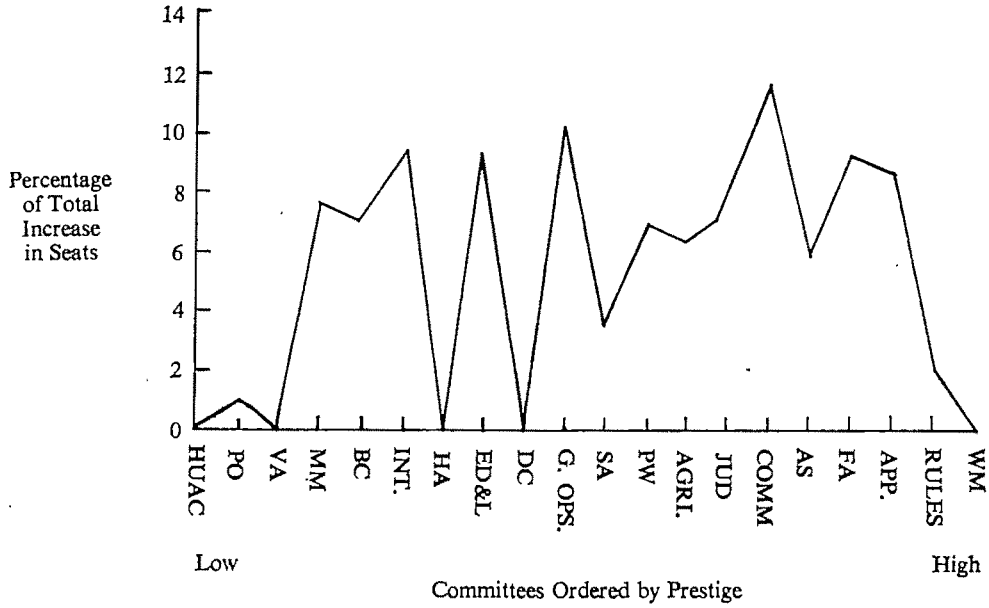
Figure 4. Expected Distribution of the Total Increase in Committee Seats

resources. In short, the leaders try to buy cheap and sell dear.[23]

Figure 5 displays the empirical distribution of total seat additions for the period 1947–1971. Notice that the bulk of the increase does occur at the higher prestige levels. Moreover, at the highest level of prestige the distribution does indeed tail off. At the other end of the distribution, however, several low prestige committees have gained seats. Still it seems fair to conclude that the data are consistent with the argument.

Figure 6 displays the distribution of committee seat additions for the earlier period, 1927–1945.

[23] One might suppose there is interdependence here. That is, adding positions to a committee may increase the number of transfers to that committee and hence affect the prestige measure. But prestige is also influenced by departures from a committee. Therefore, in principle it is possible to observe low prestige committees increasing in size.



* Veterans Affairs suffered a net loss of one seat during this period; it has been counted as a zero increase in size.

Figure 5. The Distribution of the Total Increase in Committee Seats: 1947–1971*

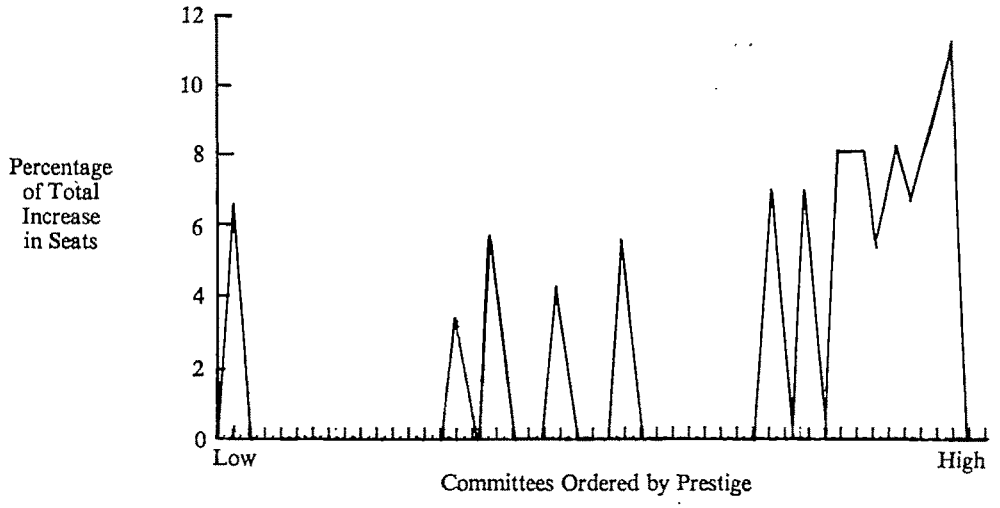


Figure 6. The Distribution of the Total Increase in Committee Seats: 1927–1945

It is clear that the distribution takes the general form specified by the theory. In this case the fit is much closer to the theoretical prediction. It may be that the propensity to limit the increase in seats to the best committees is particularly strong when there already are substantial numbers of committee positions in the system.

In this period, however, the distribution does not tail off at the high prestige level as the theory demands. Undoubtedly this is due in large part to the confounding influence of the size of the Appropriations Committee. For both time periods, this committee consistently has been one and one-half to two times the size of the other committees in the House. An increase of five seats on a large committee probably does not have the same substantive meaning in terms of the argument as an increase of five positions on a much smaller committee of comparable prestige. Therefore, let us re-examine the increase in committee size. We will look at the increase in positions on a committee relative to the size of that committee.

Figure 7 contains the data for the modern period; Figure 8 displays the data for the period 1927–1945. With the data ordered in this way note the tendency for the increases in size to tail off at the very high prestige level. The effect of taking into account the size of the Appropriations Committee is particularly noticeable in Figure 8. Therefore, both the time path of the total number of committee seats and the distribution of the increases in size across committees are consistent with the theory.

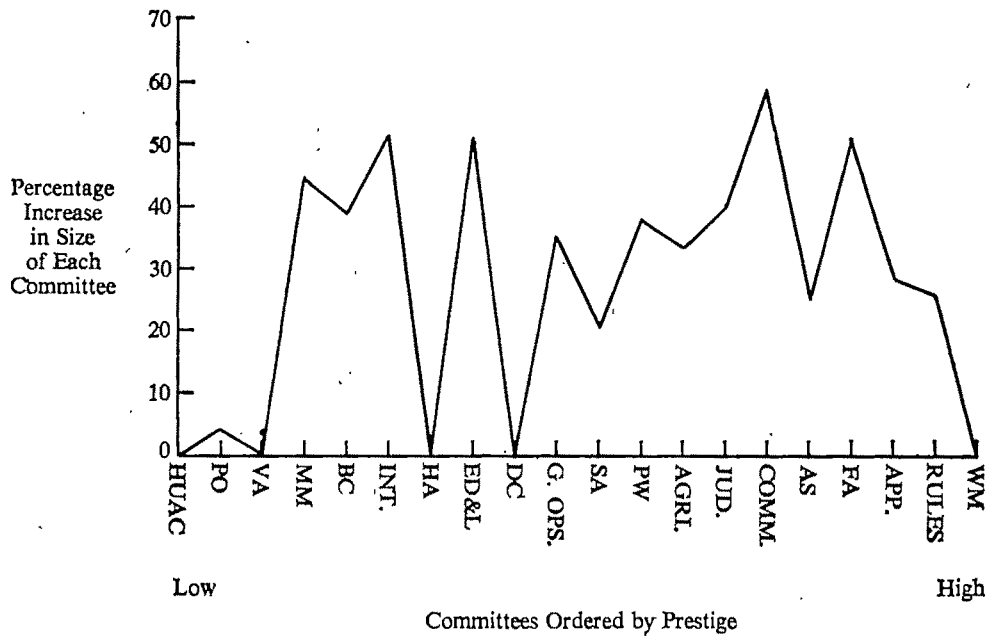
A Long-Run Consequence of A Short-Run Strategy

In evaluating the argument to this point we have confined our attention to the supply of committee positions, which is a directly observable variable. This leadership strategy, however, is likely to have other important consequences. Indeed, these decisions to expand positions at particular points in time have a cumulative effect. This consequence can be best examined by turning to the expressed demand for additional positions and considering the relationship between the supply of and demand for positions through time. We ask, in other words, what does the theory tell us about the long-run consequence of this particular leadership strategy?

Scarcity makes committee positions valuable leadership resources. The preceding argument provides an interpretation for the growth in the number of committee seats. But as the supply of committee positions increases, scarcity declines thereby altering the relationship between the demand for and the supply of positions, i.e., the gap between demand and supply decreases. Thus, as currency becomes more and more plentiful, the basis of exchange is impaired. The leaders must revalue the currency or turn to another medium of exchange.[24] This reasoning leads to a third proposition:

PROPOSITION III: In time the currency (committee positions) will be devalued to the point

[24] The crucial notion of this argument is the *relative* time rate of change of supply and demand. We might consider the demand for positions to be fixed and constant over time. Alternatively, we may assume that interests proliferate as social, political, and economic systems become more complex thereby increasing the demand for positions. Or demand for positions may decline through time. As long as the time rate of change of supply is greater than the time rate of change of demand, the basis of exchange between the party leaders and followers is impaired.



* Veterans Affairs suffered a net loss of one seat during this period; it has been counted as a zero increase in size.

Figure 7. Increase in Size of the Standing Committees: 1947–1971*

where the system of resources must be overhauled.

The interpretation is straightforward.[25] Short-run decisions designed to gain leverage with the members in time seriously erode the value of the leadership's resource base. Clearly the argument

[25] Clearly this proposition, the long-run consequence of a leadership management strategy, cannot be evaluated empirically in the same manner as the earlier portions of the argument. It takes time for the process to work itself out. Nonetheless, this argument can easily be formalized. Specific rate of change assumptions for D_t (demand) and S_t (supply) would make it possible to estimate at what point the committee system would be under stress, i.e., at what point conditions favorable to reorganization are likely to occur.

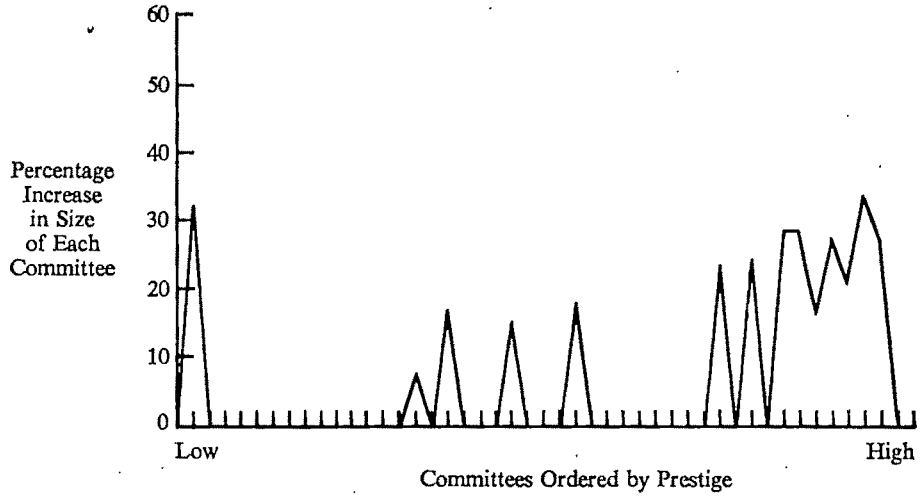


Figure 8. Increase in Size of the Standing Committees: 1927–1945

provides an economic interpretation for periodic readjustments in the committee system or committee reorganization. Short-run decisions do indeed have long-run consequences: inflation brings impairment of the leadership resource base. Thus, another major reorganization of the committee system similar to that of 1946 should come as no surprise, i.e., there is a theoretical prediction that it will occur and a theoretical interpretation for its occurrence.[26]

Conclusion

An elementary theory of leadership expansion of committee positions has been presented. The data, the time path of the total number of committee positions, and the distribution of the increase in seats across the prestige hierarchy of committees are consistent with the argument. (And more than a dozen *new* committee positions were added at the opening of the 93rd Congress.) Furthermore, it has been shown that quite reasonable short-run decisions have a significant long-run consequence.

Possible alternative explanations of this phenomenon have not been examined. One such explanation, however, can be rejected. The House relies extensively on an elaborate division of labor, in committees and subcommittees, to conduct its business. The volume and technical complexity of legislation has increased through time. With this growth in workload has come an increase in the legislative budget.[27] Congressional staffs have grown as have funds for committee operations. To cope with the increased burdens on the work groups (the committees), one might suppose that increasing the number of committee members would be an appropriate management response: more members to deal with the increasing work load. Such a response might account for the growth in committee positions after World War II. But prior to 1946 an abundance of positions existed. And surely the volume and technical complexity of legislation from the 1920s to the 1940s was less than after 1946. Yet, even with a large number of positions in this earlier period, the leaders continued to create additional committee positions.

But is the creation of additional positions to meet an increasing work-load a reasonable management strategy? Adding staff positions and funds for committee operations may be. But additional positions for the members must be filled from a constant membership base.[28] More positions, then, require more multiple memberships, more demands on a member's time and energy. And although members with multiple assignments have some freedom to choose where to concentrate their committee efforts, surely some committee responsibilities must suffer in the process. Thus, increasing committee positions as an organizational management strategy to cope with a growing work load seems to be neither a plausible nor compelling alternative explanation.[29]

Finally, although the phenomenon examined in this paper is but one aspect of congressional behavior, the argument presented is not at odds with other research on Congress. Indeed, it is quite reasonable in light of the discussions of the institutionalization of the House.[30]

With the rise of the seniority system, a committee position in practice has become a property right of the individual member. That is, leaders cannot arbitrarily move members from one committee to another. A member, as long as he remains in the House, is entitled to retain his pres-

[26] The leaders may also increase the size of a committee in order to reshape its composition for policy reasons. A good example would be the expansion of the Rules Committee. See Milton C. Cummings and Robert L. Peabody, "The Decision to Enlarge the Committee on Rules: An Analysis of the 1961 Vote," In *New Perspectives on the House of Representatives*, ed. Robert L. Peabody and Nelson W. Polsby (Chicago: Rand McNally, 1963), pp. 167–194. In such cases the leaders' desire for compliance is directed at a work group rather than a specific individual. Although this theory consists of assertions at the level of individuals, leadership decisions directed at work groups are not at odds with the argument. As long as scarcity of positions is important in the relations between leaders and followers the argument holds.

[27] See Nelson W. Polsby, "The Institutionalization of the U.S. House of Representatives," *American Political Science Review*, 62 (March, 1968), 158–160.

[28] As we have seen new positions on some committees are not matched by a reduction in size of other committees. In the period examined prior to 1946 almost half the members held multiple assignments. And the number of multiple memberships has steadily increased since 1946. See Gawthrop, "Changing Membership Patterns in House Committees," pp. 366–367.

[29] There are other data which suggest the process discussed in this paper may be a general leadership management strategy, i.e., not confined solely to the House of Representatives. From 1927–1945 the time path of the total number of Senate committee positions is an increasing function of time. And this growth took place in the midst of an abundance of positions; the ratio of the number of seats to the number of senators was more than four to one. After the Reorganization Act of 1946, the number of positions once more steadily increased. After 1969 the time path begins to oscillate slightly. But some departure from the House pattern is not surprising. Inflation of seats has been much more pronounced in the Senate; even after 1946 the ratio of seats to members exceeded two to one.

[30] See Polsby, pp. 148–168; Nelson W. Polsby, Miriam Gallaher, and Barry Spencer Rundquist, "The Growth of the Seniority System in the U.S. House of Representatives," *American Political Science Review*, 63 (September, 1969), 787–807; and Michael Abram and Joseph Cooper, "The Rise of Seniority in the House of Representatives," *Polity*, 1 (Fall, 1968), 52–85.

ent assignment. And as House service becomes more attractive and House turnover declines, a rigidity develops in the committee system. The leaders are constrained in the use of assignments as a resource, a means to gain leverage with the members. Vacancies alone determine the size of this leadership resource base. Hence, the leaders, quite plausibly, can be expected to make their resource base more flexible, larger and more responsive to their needs. Resorting to "manufacturing" committee positions, then, is a not surprising leadership response to the conditions of the modern House.

We might also speculate about the particular conditions under which the leaders would choose to add to the existing supply of committee positions. One such condition involves the magnitude of House turnover. Presumably there is little need to increase resources available for allocation when House turnover produces a number of vacancies. Rather it is when there is little House turnover, when deaths, retirements, and decisions at the polls do not produce a number of vacancies, that the leaders need to *create* vacancies, i.e., add positions. Thus, all other things being equal, the level of House turnover should vary inversely with the number of newly created positions. And, for the periods examined in this paper, this negative relationship does occur. More new seats are created when House turnover itself fails to produce the needed vacancies. (See appendix.) The leaders are free from dependency on House turnover alone to produce committee vacancies for resource creation to gain leverage with the members is possible. In sum, this additional datum is consistent with the argument of this paper which, in turn, complements other research on Congress.

Appendix

The relationship between the creation of new seats and the magnitude of House turnover can be shown in the following manner. Resources (committee seats) can be expressed as the ratio of committee positions to the number of House members at the beginning of each Congress. Call this measure L_t. The change in resources, ΔL_t, may be defined as $\Delta L_t = L_t - L_{t-1}$. ΔL_t then represents short-run leadership decisions to alter the supply of positions. House turnover can be measured by the common convention, the proportion of 1st term House members. Call this measure H_t. Committees frequently have been partitioned into three classes, Top, Service, and Duty. This partitioning generally coincides with the prestige hierarchy of committees. The most prized committees fall into the Top Committee class. Service Committees are somewhat less prized than Top Committees but are more prestigious and desirable than Duty Committees, the least valued positions. Since we have seen that the number of positions on the least prized committees is seldom increased, we can confine our attention to the Top and Service Committees. The regression of ΔL_t on H_t illustrates that more new seats are created when House turnover itself fails to produce the needed vacancies:

New Seat Creations and House Turnover: The Regression of ΔL_t on H_t

	Intercept	Slope	R^2
Top Committees			
1935–1971	.038	−.140	.163
1949–1971	.044	−.177	.149
Service Committees			
1935–1971	.030	−.121	.342
1949–1971	.033	−.133	.317

The modern period has been considered separately to take into account the effects of the vastly greater number of committee positions prior to 1946. By the criterion of fit, substantial variation remains statistically unexplained; the correlations range between −.39 and −.58. Yet the relationships are in the expected direction and easily interpretable. They are consistent with the argument of this paper.

Ethnic Politics, Representative Bureaucracy and Development Administration: The Zambian Case*

DENNIS L. DRESANG
University of Wisconsin, Madison

Given the political authority and economic resources of public bureaucracies, a major goal of ethnic[1] politics is to affect the criteria of bureaucratic recruitment. Ethnic groups seek access to administrative positions in government as opportunities for individual mobility and as a means of presenting group interests in the decision-making process.

The basic argument for a bureaucracy that draws from all segments of society for its personnel assumes that individual civil servants will articulate the values and interests of their social background and thus influence the content and the implementation of policy.[2] Few efforts have been devoted to the task of determining whether in fact an individual's social identity affects his behavior as an administrator. This essay examines the Zambian situation in order to assess the effects of ethnic politics on the composition and the performance of public bureaucracy.

The major argument here is that the representative character of government is more important as an issue of ethnic politics than as a determinant of how administrators pursue economic development. In pursuing this argument, we will first examine the nature of regional, ethnic, and class cleavages in Zambian politics. This discussion will be followed by an analysis of the interrelationships of ethnicity, national politics and development administration. Here the evidence is that economic and technocratic considerations and the perceptions of what is necessary for personal career advancement, rather than ethnic parochialism, provide the major guidelines for development decision making.

The Bases of Ethnic Conflict

Ethnically based loyalties and groups do not in themselves pose a threat to the integrity or stability of a political system.[3] What is important for political order and political conflict is the degree to which ethnicity represents both subjective images and objective differences.

There is a wide variety in the patterns of congruence between ethnic identity and other traits. In the United States, ethnicity is correlated with occupations, income, and the geographic areas of some cities and states. Politics in Malaysia offers an example of interaction between an ethnic group that has a predominance of political power and another ethnic group that has a predominance of economic wealth.[4] In South Africa there is a tight and institutionalized congruence between race and levels of economic wealth, political power, and social status.[5] Zambia, like many other African states, provides a case of ethnic differences that coincide with distinct geographic areas.

What, in the political arena, are referred to as "tribes" in Zambia are basically divisions in the society that group people according to regional background, language and subjective stereotype. Anthropologists would find little justification for the groupings. Politically, however, they make sense.[6] Linguistic affinity provides a ready vehicle

* This essay is based on research conducted in Zambia from 1967–69 and again in 1972. Financial support was provided by the Foreign Area Fellowship Program for the earlier visit and by the Center for Development, University of Wisconsin, for the later one. Helpful comments were provided at various stages of the development of this work, and I would here like to express my gratitude to Fred M. Hayward, Michael F. Lofchie, Robert Molteno, Ira Sharkansky, George K. Simwinga, Richard L. Sklar, and M. Crawford Young.

[1] The term "ethnicity" conveys only imperfectly the intended meaning. This term is certainly less pejorative and more useful than "tribalism" or "race," but it is not more precise in describing the nature of the cultural and political bonds that provide a basis for group identity. Ethnicity may coincide with class, occupational, sectional or other differences, but the use of a term like "sectional" by itself neglects the affective component that ethnic labels and stereotypes bring to the dynamics of politics. When describing the Zambian situation, I will use the term "ethno-regional" to refer to the most salient political divisions.

[2] A review of the relevant literature is available in V. Subramaniam, "Representative Bureaucracy: A Reassessment," *American Political Science Review*, 61 (December, 1967), 1010–1019.

[3] For the emergent consensus on this point, see Robert Melson and Howard Wolpe, "Modernization and the Politics of Communalism: A Theoretical Perspective," *American Political Science Review*, 64 (December, 1970), 1112–1113.

[4] Cynthia H. Enloe, *Multi-Ethnic Politics: The Case of Malaysia*, Research Monograph No. 2 (Berkeley: Center for South and Southeast Asia, University of California, 1970).

[5] Pierre van den Berghe, *South Africa: A Study in Conflict* (Berkeley and Los Angeles: University of California Press, 1967).

[6] Zambia is not unique in this regard. See Melson and Wolpe, "Modernization and the Politics of Communalism" 1123 and 1124; Ronald Cohen and John Middleton, eds., *From Tribe to Nation in Africa: Studies in Incorporation Processes* (Scranton, Pa.: Chandler Publishing, 1970), and Victor A. Olorunsola, ed., *The Politics of Cultural Sub-Nationalism in Africa*

of communication and mobilization in a country with no widely-used *lingua franca* and with a high rate of illiteracy. Regional differences encompass variations in natural resource bases, levels of economic development, and supplies of educated manpower. When articulating regional demands, spokesmen can use stereotypes to simplify the issues and rally constituent support. The fact that all these divisions coincide with one another intensifies and reinforces the cleavage and the conflict.

Four major Zambian political groups that can be identified on an ethnic-linguistic-regional basis are: Bemba, Nyanja, Lozi and Tonga. The name of each unit refers to the major ethnic community and the major language spoken within each grouping. There is a high level of congruence between the homelands of these groups and the administrative boundaries of Zambia's provinces. The obvious exceptions to this geographical distribution are the urban areas, where all ethnic categories are represented. Bemba, for instance. refers to the Bemba and to the Bisa, Mambwe, and other small groups in Northern and Luapula Provinces. The Nyanja include Chewa, Tumbuka, Ngoni, and other Nyanja-speaking peoples who are primarily from the Eastern Provinces. The Tonga are from Zambia's Southern Province, and the grouping also includes the Ila and a number of rather small communities in the area. Lozi are actually outnumbered by the peoples in Western Province that they used to govern. In contemporary Zambia, however, the term Lozi is used to refer to almost everyone from Western Province.

In addition to these four major political groups, there are a large number of small communities, the most prominent being the two Lunda groups, one in Luapula Province and one in Northwestern Province. It is notable that despite language affinity and common historical origins,[7] the Lunda in these two provinces have neither formed any alliance nor pooled their energies in any political contest since the establishment of colonial rule. They are separated by the Zaire pedicle and by very different ecological bases. They have found little in common to struggle for. Until 1971, in fact, the Lunda in Luapula Province were very close allies of the Bemba.

Importantly, none of the major urban centers in Zambia are located in the area of any of these ethnic groups. This means that people were recruited and attracted from all over the country to the urban areas. As in other societies, there was a clustering together of those of similar language, culture, and region and the emergence of ethnic stereotypes.[8]

Regional imbalance in economics and education, as well as cultural differences, contribute to the formation of the subjective images of different peoples. The Nyanja peoples were advantaged by educational facilities provided by the Church of Scotland missionaries in the Eastern Province and neighboring Nyasaland/Malawi. Nyanja, as a result, filled most of the clerical and white-collar jobs open to Africans during the colonial period. The Tonga, on the agriculturally rich plateau of the Southern Province, took advantage of their land and their position near the single railway line in the country and established a tradition of farming and ranching. In contrast, the Bemba were located on land with a low agricultural potential. Given this disadvantage, along with their relative proximity to the copper mines that began operating in the late 1920s, Bemba formed the bulk of the non-clerical, African labor force in the mines. The Lozi, too, had rather poor land and provided many of the laborers in the mines of South Africa and Southern Rhodesia. The Barotse National School, established by the Paris Evangelical Missionary Society in the early 1900s, provided the Lozi with a small, very well educated group of men. The stereotypes of each group emerged from these different backgrounds and resources. The Nyanja are viewed as methodical and clerical, the Tonga as rural and conservative, the Bemba as tough and hard-working, and the Lozi as proud and intelligent. Needless to say, there are as many exceptions to these stereotypes as there are to those that exist in other societies.

Table 1 summarizes some of the important differences between the major ethno-regional groups.

The Politicization of Ethnicity

In the process of competing for votes, Zambian leaders have appealed to geographic constituencies and thereby politicized the real and significant linguistic, economic, and other sectional differences that already existed in Zambia. These appeals were prominent during the period of most intense political mobilization—the struggle for independence from the United Kingdom.[9]

(Garden City, N.Y.: Doubleday, 1972). These horizontal differences do not connote any particular hierarchical order, but they do define the actors involved in establishing which group will have a preponderance of wealth and power.

[7] C. M. N. White, "The Ethno-History of the Upper Zambezi," *African Studies*, 21 (1962), 10–27 and Jan Vansina, *Kingdoms of the Savanna* (Madison: University of Wisconsin Press, 1966), pp. 70–97 and 160–173.

[8] For a discussion of identity and integration of Africans in urban areas, see Max Gluckman, "Tribalism in Modern British Central Africa," *Cahiers d'études africaines*, 1 (January, 1960), 55–70, and Clyde Mitchell, *The Kalela Dance* (Manchester, England: Manchester University Press, 1956).

[9] For an analysis of the Zambian independence movement, see David C. Mulford, *Zambia: The Politics of Independence, 1957–1964* (London: Oxford Univer-

Table 1. Ethno-Regional Differences in Zambia

Ethnic Group	Province(s)[a]	Language	Dominant Type of Traditional Agriculture	Primary Staple Crop	Predominant Soil Type	Denomination of Missionaries in the Area	Dominant Cash Economic Activity	Commonly Held Stereotypes	Shifts in Political Party Allegiances[b] (1964–1972)
Bemba	Northern, Luapula	Bemba	Fishing and Bush-Fallow (Chitemene)	Finger Millet	Ferrallitic	White Fathers	Labor Supplied to Copperbelt	Tough hard-working	UNIP ↓ UPP
Lozi	Western	Lozi	Transitional Ash-Culture (Northern and Southern Kalahari)	Bulrush Millet	Sands	Paris Evangelical Missionary Society	Timber, Labor to Urban Areas	Proud, intelligent	UNIP ↓ United Party ↓ ANC
Tonga	Southern	Tonga	Pastoral and Hoe and Plough	Maize	Lithosols	Primitive Methodist and Jesuit	Cattle Maize	Rural, conservative	ANC
Nyanja	Eastern	Nyanja	Hoe and Plough	Kaffir Corn, Maize & Finger Millet	Vertisols	Church of Scotland	Tobacco, Cotton, Groundnut, Labor to Urban Areas	Methodical, clerical	UNIP and ANC ↓ UNIP

[a] All are represented in the urban areas along the railway that runs through Southern, Central and Copperbelt Provinces.
[b] UNIP = United National Independence Party (the ruling party since Independence),
ANC = African National Congress (the initial party of the independence movement from which UNIP splintered),
UPP = United Progressive Party
Sources: D. Hywell Davies, ed., *Zambia in Maps* (London: University of London Press, 1971), pp. 26, 27, 38–41, 46, 47, 52–55, 58, 59 72–79, and George Kay, *A Social Geography of Zambia* (London: University of London Press, 1967), p. 52.

A dramatic and highly visible example of the effects of administrative boundaries on politics is the case of the Western Province (formerly Barotseland Protectorate). The Lozi Kingdom had a separate legal status with the colonial government that distinguished it from the rest of Northern Rhodesia/Zambia. During the independence struggle, the issue of the status of the Lozi area within independent Zambia emerged. Some efforts to secure independence as a separate state were defeated in favor of legal integration with Zambia, but in the process of settling this issue the Lozi identified themselves in national politics as a separate group with rather distinct problems.

The split within the major nationalist movement in Northern Rhodesia/Zambia provided the basis of postcolonial ethnic conflict for the rest of the country. When the militant nationalists left the African National Congress (ANC) and formed the United National Independence Party (UNIP), the incumbent ANC President, Harry Nkumbula, retained the support of his home area, the Southern Province, and allied himself with conservative traditional leaders who felt threatened by the rhetoric and leadership in UNIP. UNIP, on the other hand, had its most firm base of allegiance in Northern, Luapula, and most of Copperbelt provinces, the homes of many of the Party's founders. Eastern Province was, for a long time, a battleground between UNIP and ANC. Most of the ANC leadership in the province defected to UNIP when the split occurred, but ANC worked closely with traditional leaders and retained a strong grass-roots following. From 1960 onward, the struggle between the two parties in the east was cast almost entirely in terms of who would best represent regional interests.[10] It was not until 1968 that UNIP, through a combination of persuasion and intimidation, established clear dominance in the Eastern Province.

Zambia, in short, provides support for one of the theoretical propositions of Robert Melson and Howard Wolpe: "In a culturally plural society, the participation of the masses in the political process encourages aspirant politicians to make appeals to the most easily mobilized communal loyalties, and to define themselves primarily as the representatives of communal interests."[11] The congruence of objective and perceived differences between the various ethnic-linguistic-regional groups provides a ready base of potential support for those engaged in political competition.

Class Formation and the Public Bureaucracy

An alternative perspective for political analysis is to emphasize socioeconomic class, rather than

sity Press, 1967), Robert I. Rotberg, *The Rise of Nationalism in Central Africa: The Making of Malawi and Zambia, 1873–1964* (Cambridge, Mass.: Harvard University Press, 1965) and Richard Hall, *Zambia* (New York: Praeger Publishers, 1965).

[10] Mulford, *Zambia*, pp. 121–131 and 327–330.
[11] Melson and Wolpe, "Modernization and the Politics of Communalism," 1122.

ethnic identity or geographical region, as a basis for group and individual behavior. This has been the most prevalent approach for those interested in representative bureaucracy.[12] Zambia, however, presents a case of class formation without a high level of class consciousness. There are objective differences among the population in levels of income and types of occupation that provide the basis for distinct sets of vested interests among the Zambian population. These objective differences, however, have not been accompanied by subjective identity or by political organization and articulation. No political party has emerged to represent or further class interests. Instead, parties have had strong regional and ethnic orientations.

In part, class divisions are relatively unimportant in Zambian politics because Africans have only recently been able to enter positions above the level of unskilled and semi-skilled work. As a result, classes have not yet developed or entrenched themselves in Zambian society. The data on the social background of Zambian civil servants, presented in Table 2, illustrate this point. The picture presented here sharply contrasts with the evidence that V. Subramaniam collected in his six-country study of administrators. Subramaniam's survey indicated that 81 to 97 per cent of the civil servants in Denmark, Great Britain, France, the United States, Turkey and India had fathers with middle-class occupations.[13] Applying Subramaniam's definition of middle-class occupations to the data in Table 2, only 54 per cent of the Zambian civil servants have fathers in this type of employment. Similarly, less than 10 per cent of the Zambian civil servants are the sons of governmental employees, whereas 46 per cent and 51 per cent, respectively, of the civil servants in Turkey and India—the two developing countries in Subramaniam's survey—have followed their fathers into government service.

The data in Table 2, however, indicate an increase in the civil servants coming from the urban, modern sectors. This trend is most noticeable for middle-level civil servants and university students. While 52 per cent of the older civil servants in clerical and executive positions are sons of traditional rulers, small-scale farmers, and fishmongers, only 32 per cent of their younger colleagues are, and only 16 per cent of the university students come from this background.

If, as expected, the future corps of senior civil servants in Zambia will consist primarily of those who are currently attending a university, then about two-thirds will come from middle-class families. Moreover, the proportion of civil servants who enter the bureaucracy with middle-class backgrounds, is likely to continue to increase. Given the requirements for entry into the civil service, middle-class families would seem to be in a better position than the peasantry to offer their children the needed education, experience, and career orientation.

In his study of elite values among Zambian politicians and senior civil servants, James Scarritt found some evidence that the distinctions being made indicate the emergence of class interests. Scarritt found a high level of agreement among members of the governing elite that they should receive special rewards and privileges. " 'The uneducated man—the laborer—cannot expect to be equal to the educated man holding a responsible position,' it was said."[14] As important as Scarritt's findings are, they measure only a single attitude related to class interests,[15] and cannot therefore be regarded as evidence of class consciousness or class-based political activity.

The studies of Zambian mineworkers by A. L. Epstein and Robert H. Bates emphasize the importance of distinguishing between class attitudes and the bases for participation in politics. Epstein and Bates have concluded that class interests are highly salient to the mineworkers and that their solidarity vis-à-vis mine management is intensified by the racial (i.e., white-black), cultural, and income differences between the two.[16] Common interests in the mining context did not, however, lead to mixing among the various ethnic groups in social relations and, according to Bates, did not result in common political allegiances or activity. Involvement of mineworkers in national politics has maintained an ethno-regional orientation.[17]

[12] Subramaniam, "Representative Bureaucracy," 1010–1019; W. Lloyd Warner, Paul P. Van Riper, Norman H. Martin, and Orvis F. Collins, *The American Federal Executive* (New Haven: Yale University Press, 1963); J. Donald Kingsley, *Representative Bureaucracy: An Interpretation of the British Civil Service* (Yellow Springs, Ohio: The Antioch Press, 1944), and R. K. Kelsall, *Higher Civil Servants in Britain* (London: Routledge and Kegan Paul, 1955).

[13] Subramaniam, "Representative Bureaucracy," p. 1016. Middle-class occupational titles, according to Subramaniam, include shopkeepers and merchants, governmental employees, business employees, professionals and skilled workers. Excluded from this category are unskilled workers, farmers, and agricultural workers.

[14] James R. Scarritt, "Elite Values, Ideology, and Power in Post-Independence Zambia," *African Studies Review*, 14 (April, 1971), p. 42.

[15] Scarritt's study was designed primarily to determine the political ideology and values of top party and government officials and to gauge the degree of unity within the elite; it was not specifically concerned with the issue of class consciousness.

[16] A. L. Epstein, *Politics in an Urban African Community* (Manchester, England: Manchester University Press, 1958), pp. 160–161, and 240, and Robert H. Bates, *Unions, Parties and Political Development: A Study of Mineworkers in Zambia* (New Haven: Yale University Press, 1971), pp. 114–119.

[17] Bates, *Unions, Parties and Political Development*, p. 118.

Table 2. Trends in the Social Backgrounds of Zambian Civil Servants, 1969

	Father's Occupation							
			Middle Class					
	Traditional Ruler %	Small-Scale Farmer, Fishmonger %	Teacher, Clergyman %	Civil Servant, Clerk, District Messenger %	Merchant %	Semi-skilled Urban Worker or Mine-Worker[a] %	Unskilled Worker %	Other %
Senior Civil Servants over 35 (N=97)	7	22	13	12	25	18	1	4
Senior Civil Servants under 35 (N=68)	3	29	13	6	13	19	9	7
Middle-level Civil Servants over 35 (N=61)	21	31	7	7	2	21	8	2
Middle-level Civil Servants under 35 (N=93)	7	27	11	10	3	32	7	4
Undergraduates at University of Zambia in Arts, Law and Humanities (N=223)	9	8	32	26	4	5	10	6

[a] Semi-skilled workers include such positions as plumber, bricklayer, mechanic, and hospital assistant. Unskilled workers include gardeners, houseboys, and handy-men.

Sources: Data on Middle-level civil servants from the Zambia Localized Civil Servants' Association Survey. (See footnote 25 for details about this survey.) Data on senior civil servants and students at the University of Zambia from V. Subramaniam, "The Social Background of Zambia's Higher Civil Servants and Undergraduates," paper presented to University Social Science Conference, Nairobi, December, 1969.

Richard L. Sklar and M. G. Smith have, from different vantage points, discussed the tendency in many societies for ethnicity and culture to coincide with class differences and inequities in the distribution of political power, economic wealth and social status.[18] In Zambia, ethnic groups are *not* identified with socioeconomic classes; instead each include members of all classes and levels of society. Ethno-regional communities, however, have been noted for occupying distinct places within a hierarchy of political power. A major concern of postcolonial politics is the determination of how this power and wealth will be structured and who will occupy the most favored and pre-eminent positions.

Ethnicity, Politics and Administration: Bemba Dominance

For the first years after Independence, Zambia was ruled by a government and party whose top officials had been selected with a view to achieving a balance between expertise and political support on the one hand and a representation of all major ethnic groups on the other.[19] This balance, however, was rudely spoiled at the 1967 Annual Conference of UNIP, held at Mulungushi. The bitterly fought elections for positions on the party's Central Committee revealed publicly the deep ethnic and regional divisions within the party. Alliances were formed between the Bemba and the Tonga on one side and the Lozi and the Nyanja on another. Some individuals, like Grey Zulu and, most notably, President Kenneth D. Kaunda, remained aloof and neutral in this struggle. Their ethnic identity was nonetheless important in calculating the effects of the Mulungushi Conference on ethnic representation. The Bemba-Tonga alliance won decisively. Of the eleven members of the Central Committee, six were Bemba and two were Tonga. One Lozi won an uncontested position and other posts went to individuals outside the major ethno-regional groups.[20]

President Kaunda was frankly disappointed about the way in which the votes were solicited. He told the party:

> We have canvassed so strongly and indeed, viciously, along tribal, racial and provincial lines, that one wonders whether we really have national or tribal and

[18] Richard L. Sklar, "Political Science and National Integration—A Radical Approach," *The Journal of Modern African Studies*, 6 (1967), 1–11 and M. G. Smith, "Institutional and Political Conditions of Pluralism," and "Some Developments in the Analytic Framework of Pluralism," in *Pluralism in Africa*, ed. Leo Kuper and M. G. Smith (Berkeley and Los Angeles: University of California Press, 1968), pp. 27–66 and 415–458. See also the discussion of incorporation in Cohen and Middleton, *From Tribe to Nation in Africa*, pp. 1–34.

[19] For details on the background of these officials, see Mulford, *Zambia*, pp. 330 and 331 and Robert I. Rotberg, "Tribalism and Politics in Zambia," *Africa Report* (December, 1967), pp. 29–35.

[20] Rotberg, "Tribalism and Politics in Zambia," pp. 29–35.

provincial leadership. I must admit publicly that I have never experienced in the life of this young nation, such a state of hate, based entirely on tribe, province race, colour and religion, which is the negation of all that we stand for in this Party and Government.[21]

A significant aspect of the Bemba-Tonga alliance in UNIP is that the Tonga distinctly constituted the weak partner. Since the Southern Province, the Tonga area, has been a stronghold of the African National Congress,[22] UNIP officials from that area could not claim to have popular support or to be able to deliver votes from their constituencies. Their strength within UNIP depended almost entirely on their right to cast a vote and their membership in an alliance. The Bemba, on the other hand, represented one of the areas of Zambia with the most organized local level support for UNIP.

Representative Bureaucracy

Not surprisingly, the charge leveled after the 1967 Mulungushi Conference was that the government was Bemba-dominated. The Lozi-based United Party, which formed after Mulungushi, charged in its publication, *The Mirror:*

> . . . There are 73 tribes in Zambia and their interests must be balanced. But look at this arrangement: The President, the Vice-President, the Chairman of the Public Service Commission—which deals with Promotions of Civil Servants; Teaching Service Commission—which deals with Promotions of teachers, Police Service Commission—which deals with Promotions of police; University Council of Zambia and Judiciary Service Commission belong to one tribe. The Commissioner of Police and the Secretary to the Cabinet belong to one tribe. These are the people governing the country and all the other ministries and departments are merely branches of some form or other of the above. The same tribe has majority [sic] of Permanent and Under Secretaries than any other tribe. It has more people in the foreign service than any other tribe. It has more Directors in Charge of Departments and Semi-Government Organizations such as the Zambia Railways, Zambia Broadcasting Services and the Commissioner of Traffic Departments, etc., etc.
>
> What is all this about? Does it mean that they are the most dedicated ones that other tribes? [sic] It is also estimated that the same tribe has nearly 150 people in the executive and higher positions of office in the Public Service. The Tongas, the Ngonis, and the Lozis next range between 30 and 50 people each in similar positions. Can anybody explain why?[23]

Civil servants, too, felt anxiety after the Bemba attained predominance in the ruling party. In a questionnaire distributed through the Zambia Localized Civil Servants Associations,[24] 26 per cent of the respondents answered affirmatively to a question asking if they felt they had ever been mistreated in the civil service because of their tribal affiliation. When asked whether it was more important to abide by the bureaucratic norm of basing appointments and promotions solely on merit or whether a conscious effort should be made to maintain a tribal balance in the bureaucracy, 45 per cent of the respondents indicated that the latter was essential for Zambia.

The pattern of response by ethno-regional identity is interesting in showing that the Lozi and those from the small ethnic groups of the Northwestern, Copperbelt and Central Provinces express the greatest concern about tribalism in the civil service and that despite the charges of Bemba domination, a significant percentage of the Bemba respondents also indicated concern about tribalism. Among the respondents, 43 per cent of the Lozi, 39 per cent of those from small ethnic groups, and 22 per cent of the Bemba said they believed that they had been mistreated because of their tribe. Indicating their feeling for a need to maintain an ethnic balance in the civil service were 52 per cent of the Lozi respondents, 78 per cent of those from the smaller groups, and 38 per cent of the Bemba. Least concerned about tribalism were the Nyanja-speaking civil servants: only 15 per cent said they had been mistreated because of their ethnic identity and 30 per cent of them said there was a need for tribal balance.[25]

The charge of tribal domination means that a

[21] Republic of Zambia, Information Services, *Mulungushi Conference* 1967 (Lusaka: Government Printer, 1967), pp. 52 and 53.

[22] Mulford, *Zambia,* 107–142. Also, Thomas Rasmussen, "Political Competition and One-Party Dominance in Zambia," *Journal of Modern African Studies,* 7 (1969), 407–424.

[23] "Tribalism in Zambia, Who Are Encouraging It?" *The Mirror* (Lusaka), I (March, 1968), 3.

[24] The questionnaire was distributed to a random sample of Zambian civil servants posted to stations outside Lusaka. The sample was drawn from staff lists of all civil servants, rather than from the membership rolls of the Association. Out of 195 questionnaires mailed, 154—78.9 per cent—were returned. Attempts to extend the mailed questionnaire to civil servants working in Lusaka received so little cooperation that the effort was abandoned. Interviews conducted by the author provided evidence that the attitudes of civil servants posted to Lusaka do not differ significantly from those of colleagues working elsewhere. I am grateful to the Zambia Localized Civil Servants' Association for their cooperation and, of course, accept full responsibility for the interpretation of the data.

[25] Based upon the observations reported by Ian Scott and Robert Molteno, "The Zambian General Elections," *Africa Report* (January, 1969), 42–27, and on field research done by the author in the Eastern Province, it appears that UNIP local officials in the Nyanja-speaking areas are more concerned about Bemba domination than are Nyanja-speaking civil servants. In the middle and lower ranks of the civil service, Nyanja-speaking civil servants have fared relatively well and so expressed less experience and less fear of ill-treatment because of their tribe than other civil servants have. See Dennis L. Dresang, *The Zambia Civil Service: Entrepreneurialism and Development Administration* (Nairobi: East African Publishing

certain ethno-regional group is accused of receiving preferential treatment when appointments and promotions are made. The evidence given to support such a claim is that the proportion of people in a given position who are from the allegedly favored group is higher than the proportion of the total population that group represents. The accusation does not always carry with it a charge that unqualified people are filling positions. Tribalism can mean that when other factors are equal members of one ethnic group are consistently treated better than those from others. It is important to remember that it is an ethno-regional group's *percentage of the total population* which is important in calculating the representative nature of the bureaucracy. Ethnic breakdowns of qualified graduates or some other grouping more relevant than the total population are not used by those alleging the existence of tribalism. Alternatively, representative bureaucracy is defined as balance, i.e., equivalent shares of positions by each of the major ethnic groups. Whichever standard is used, the Zambian government is ethnically heterogeneous but not representative.

It is indeed curious that, as charged by the United Party, virtually all of the most senior government positions in Zambia after the 1967 Mulungushi Conference were occupied by Bemba. With the exception of the President, the United Party enumeration of senior posts occupied by members of the same ethno-regional group in 1968 is accurate.[26] Table 3 presents the ethno-regional breakdown of the total population and of people occupying senior governmental positions as of December, 1968. The data indicate that the smallest groups were grossly underrepresented; the Bemba and the Nyanja overrepresented. Bemba dominance was most striking in positions at the head of government commissions and ministerial departments.

Ascriptive identity as a possible factor in appointing and promoting personnel can be felt in certain ministries and departments as well as in government as a whole. The evidence and the "folklore" indicate that specific units of the Zambian bureaucracy appear to be the preserve of certain ethno-regional groups. In December, 1968, the senior posts of the Ministry of Local Government and Prisons, both at headquarters and in the provinces, were staffed primarily with Lozi. Four of the top six Zambian civil servants at headquarters and one-half of the Provincial Local Government Officers were Lozi. Of the 14 Zambians in substantive posts in the Office of National Development and Planning, 8 were Lozi. Similarly, 8 of the 10 senior Zambians stationed at headquarters in the Ministry of Labour were Bemba. In the Ministry of Youth, Co-operatives and Social Development, 9 out of 12 were held by Bemba.

The tendency for units within the government to consist of personnel primarily from one ethnic area may be regarded as evidence of patronage. The data are not available to ascertain whether this distribution reflects patron-client ties or something less personal.[27] Whatever the causes, the resulting pattern contributes to the perception that ethnicity is a salient force. For individual civil servants, the importance of their social identity may lead them to conclude that their career prospects are better in one ministry or department than they are likely to be in another. For the society as a whole, ethnic differences are maintained.

In sum, after the 1967 UNIP elections, the Bemba took the lion's share of the very senior government positions in the ministries and on government commissions. Their dominance was not so pervasive, however, as to make it impossible for members of other groups to enjoy a career and mobility either in the civil service generally or in particular units of the bureaucracy.

Development Administration

Ethnicity and Development Allocations. The rationale for representative bureaucracy would lead us to expect that Bemba domination in the senior echelons of government would lead to public policy that favors Bemba and neglects or sacrifices the needs of other groups. One way of testing these expectations is to examine the pattern of development expenditures in the provinces. Development policy issues constitute an area of the highest long-term and short-term political significance, and thus are an appropriate focus for this analysis. Furthermore, since development plans and allocations are determined by senior civil servants and political officials,[28]

House, 1974), pp. 63–65. One suspects that on the level of national politics, as opposed to intra civil-service matters, Nyanja-speaking civil servants might express the same apprehensions and desires as politicians, farmers, and others from the Eastern Province.

[26] The term "tribe" as used by Zambians in discussing their national politics refers to the ethnic and regional differences that have been delineated above. The quotations by President Kaunda and the United Party are evidence of this. The author used interviews with some of the respondents of the survey to confirm that they too interpreted "tribe" as synonomous with ethno-regional divisions.

[27] For a discussion of patron-client relationships, see Rene Lemarchand, "Political Clientelism and Ethnicity in Tropical Africa: Competing Solidarities in Nation-Building," *American Political Science Review*, 66 (March, 1972), 68–90, and James C. Scott, "Patron-Client Politics and Political Change in Southeast Asia," *American Political Science Review*, 66 (March, 1972), 91–113.

[28] A more detailed discussion of development policies and administration in Zambia is available in Dresang, *The Zambia Civil Service*, pp. 110–207.

Table 3. Ethno-Regional Breakdown of Senior Government Officials[a]

	Total African Population %	Cabinet Ministers %	Permanent Secretaries %	Heads of Departments %	Total Superscale %
Bemba	31	27	32	50	38
Nyanja	19	20	26	13	25
Tonga	13	13	15	16	16
Lozi	13	27	15	19	15
Other	24	13	11	3	7
	N=4.1 million	N=14	N=19	N=32	N=289

[a] In calculating these figures, only black African Zambian personnel were included. Superscale positions include all supervisory posts (generally Heads of Departments, the equivalent and above) with a salary of K5,200 or more. (One Kwacha is equivalent to U. S. $1.40.)

this policy area provides an opportunity to correlate available ethnic background data with the outputs of the decision-making process. The effects of social identity might be explored in other policy areas, such as the granting of licenses and the enforcement of various regulations. Data here, however, are not readily available.

From the data presented in Tables 4 and 5, it appears that development allocations to provinces have not been designed to favor one particular region, but rather to emphasize generally efforts in the least developed areas of the country.

Table 4 illustrates the differences in the degree of development and urbanization in the Copperbelt, Central, and Southern provinces on the one hand, and in the rest of the country on the other. In Zambia, as in many countries, the concentration of growth and development has followed the railway line that pioneered contact with the outside commercial world. The single "line of rail," as it is referred to in Zambia, goes through the three provinces with the highest indications of economic development.

The most marked pattern of development expenditure, as shown in Table 5 is that the most developed province, Copperbelt, is receiving the fewest capital funds. Furthermore, no particular province or set of provinces have been singled out for punishment for their political behavior. On a per capita basis, in fact, the Northern Province (Bemba) does not score very high. This is offset to some extent by the economic activity likely to be generated by a major national development project, the construction of the railway from Zambia through to Dar es Salaam, Tanzania. Also, the figures for development expenditures in the Northern Province reflect the relatively low economic potential of that region. Economic factors also influence the relatively high per capita expenditures for Western and North-Western Provinces, the two areas with the poorest base in infrastructural resources. What appears to be followed, in short, is a strategy of striving for rapid and balanced growth,[29] regardless of the political record of the various ethno-regional groups.

An examination of a specific policy area, the development of primary school facilities, further illustrates the strategy of development administration. The field of education offers some advantages for an analysis of patterns of allocation. This policy area is not encumbered by such factors as soil conditions, the location of mineral deposits, and unhealthy areas that obstruct the equal distribution of agricultural, mining and health services. Schools can be built wherever there are people. Education is, moreover, an issue of high political visibility and salience.[30] If the pattern of ethnic dominance among senior government officials has an impact on the use of development funds, that impact should be reflected in the development of educational facilities. Table 6, however, indicates that Zambia has acted to equalize levels of provincial development, rather than to favor areas with high representation in government. The columns showing the percentages of school age population enrolled in school demonstrate a leveling tendency since independence in 1964. The slightly higher than average percentages recorded in Southern, Luapula, and Northern Provinces in 1969 reflect unanticipated declines in the number of school age children in these areas.[31] On the other hand, the population in Lusaka increased 82 per cent between 1964 and 1969, making it difficult to provide adequate

[29] For analyses on Zambia's failure to achieve rapid and balanced economic development, see Charles Elliot, ed., *Constraints on Economic Development in Zambia* (Lusaka: Oxford University Press, 1971).

[30] See the discussion in John Mwanakatwe, *The Growth of Education in Zambia Since Independence* (London: Oxford University Press, 1968).

[31] Republic of Zambia, *Statistical Yearbook, 1970,* p. 3 and *Second National Development Plan,* pp. 169–171.

Table 4. Patterns of Provincial Development

Province	Population (thousands)	Farmers in Traditional Agriculture (thousands)	Number of Manufacturing Establishments	Employed Africans % of Pop.	Dwellings with Electricity Available (%)	Dwellings with Private Water Taps (%)
Copperbelt	544	8.1	317	29	43	47
Central	505	18.7	166	27	17	17
Southern	466	36.5	41	18	10	9
Luapula	357	52.8	1	12	2	1
Northern	564	48.8	1	9	2	2
Western	363	51.1	1	8	1	1
Northwestern	211	32.4	1	11	1	1
Eastern	480	80.7	4	8	1	1

Source: Calculated from Republic of Zambia, *Statistical Yearbook, 1970* (Lusaka: Government Printer, 1971), 3–10, and *Second National Development Plan, 1972–1976* (Lusaka: Government Printer, 1972), 172–174.

schools, despite heavy expenditures. It should also be pointed out that the population in Southern Province is highly concentrated, particularly in the towns along the line of rail. Thus, relatively low amounts of funds could expand educational facilities in a way that resulted in the highest percentage of primary school enrollment. These caveats aside, it is clear that the effort is to provide all provinces with roughly equivalent opportunities to educate their children.

The pattern of development expenditures, whether general or specific to education, does more than demonstrate the absence of Bemba favoritism. It also serves as a reminder that classifying people according to their circumstances of birth or language is, after all, not grouping them according to any criteria necessarily related to administrative behavior or attitudes. Even in a society where ethnic political cleavages are as important as they are in Zambia, civil servants are guided by their bureaucratic environment. Not all administrators who are inclined to act prejudicially for their own ethnic and regional groups are able to do so. Some are constrained by the discretionary authority of their job and the policy requirements of specific issues. Since cotton is not readily grown in Western Province, for instance, a Lozi Cotton Officer can do little to act partially for his area. Others who are in policy areas that allow more options must work with other officials and agencies. The interests of other segments of the bureaucracy may differ from and thus check the designs of a parochially oriented administrator.

The concern of civil servants for their own individual careers provides a fundamental source of guidance for administrative behavior. Zambia's bureaucracy, like that of most other countries, is at least partially oriented toward merit and achievement. Civil servants seeking advancement know that promotions depend not just on seniority and education but also on demonstrated

Table 5. Provincial Development Expenditures
(in Kwacha)

Province	Development Expenditures up to 31 Dec. 1971	Per Capita Expenditures up to 31 Dec. 1971	Development Expenditures Total Estimated Cost	Per Capita Total Estimated Cost
Copperbelt	1,579,000	2.90	9,124,000	16.78
Central	5,410,000	10.71	9,430,000	18.66
Southern	4,236,000	9.09	10,078,100	21.63
Luapula	4,860,000	10.81	10,166,000	28.48
Northern	5,152,000	9.12	9,991,000	17.71
Western	5,784,000	15.93	11,185,000	30.81
North-Western	3,761,000	13.09	7,866,000	37.28
Eastern	4,795,000	9.99	10,123,000	21.09

Source: Calculated from Republic of Zambia, *Estimates of Revenue and Expenditures*, 1972 (Lusaka: Government Printer, 1972). Figures do not include the spending of national projects and programs in the provinces. Provincial allocations are made by the ministries of Rural Development, Lands and Natural Resources, Education, Power, Transport and Works, Health, Legal Affairs, and Provincial and Local Government and Culture.

Table 6. Development of Primary School Facilities, 1964–1969

Province	Planned Expenditures (In Kwacha)	Actual Expenditures Up to Dec. 31, 1969 (in Kwacha)	Planned Expenditures Per School Age Child 1969 (in Kwacha)	Actual Expenditures Per School Age Child 1969 (in Kwacha)	1964 No. of Students	1964 % of School Age Pop. in School	1967 No. of Students	1967 % of School Age Pop. in School	1969 No. of Students	1969 % of School Age Pop. in School
Copperbelt	5,247,000	140,000[a]	23.8	0.6[a]	82,645	56	116,005	61	135,594	62
Central	4,399,000	3,062,000	22.8	15.8	55,199	41	169,440	50	105,395	55
Southern	1,739,000	719,000	12.9	5.4	60,722	48	82,505	63	96,165	68
Luapula	1,882,000	998,000	20.7	11.0	33,535	35	47,164	51	58,828	65
Northern	3,401,000	1,797,000	23.8	12.2	50,316	33	75,583	51	94,379	64
Western	2,149,000	1,549,000	19.4	13.9	38,151	38	50,176	48	63,736	58
North-Western	1,273,000	839,000	20.3	13.4	15,360	27	23,278	39	29,948	47
Eastern	3,058,000	986,000	22.2	7.2	42,711	52	60,657	53	77,236	56
Total	24,148,000	10,090,000	22.0	5.0	378,639	42	539,353	53	661,281	60

[a] During this period, the mining companies continued to provide educational facilities for most of the families working on the Copperbelt. Thus, central government funds were needed only for the rural areas of that province.

Sources: Zambia, *Estimates of Revenue and Expenditures*, 1972, 157–168, and *Statistical Year-Book, 1970*, 3, 14–20.

competence. In addition, the perception of the advantages and disadvantages of a given ethnic identity is important. What may be relevant, in other words, is the belief that Bemba dominance exists; not the actual extent of its existence. The effects of these perceptions can be seen by looking at the behavior of Zambian administrators with responsibility for promoting economic development.

Ethnicity and Entrepreneurialism. Ethnic identity and personal self-confidence affect calculations of what must be done in order to have a successful career. These calculations, in turn, influence the degrees of enthusiasm and innovation with which administrators approach their work, but have little effect on the allocations to ethno-regional groups. An official may, for instance, feel that he cannot rely on his ethnicity to secure promotions. Depending on his self-confidence, either he will work hard at becoming known as an unusually competent administrator and thus compensate for his ethnic identity or he will seek security through low visibility and anonymity.

Zambian evidence is instructive on this point. From data collected through in-depth interviews and an examination of files and documents relevant to cases of development decision making, a select group of thirty-three Zambian officials were studied and classified according to their career orientation.[32] Interviews were designed and case studies selected in order to determine self-confidence, job motivation, career aspirations, and participation in decision making. An administrator was regarded as entrepreneurial if: (1) he had access to resources of funds, equipment and personnel that could be used to promote development; (2) he was self-confident, specifically in that he felt able to influence his own future; (3) he believed that opportunities existed for rapid upward mobility; and (4) he concentrated on one project or program in order to maximize chances of its success and his visible identification with it. The selection of senior officials with development responsibility purposely biases the sample in order to maximize the likelihood of entrepreneurialism. Of the 33 Zambian officials interviewed, 19 demonstrated entrepreneurial traits. Seven of these were Bemba, who generally were men eager to take optimal advantage of their administrative position and their ethnic identity. The twelve non-Bemba calculated that it was essential to demonstrate exceptional ability if they were to overcome the handicap of not possessing the most advantageous ascriptive traits. The nine Bemba and five non-Bemba who were not entrepreneurially oriented lacked ambition and self-confidence and viewed their responsibilities primarily as routine administrative tasks.

The Zambian case suggests that ethnicity does have an impact on development administration. The effects are most clear on the career orientations of individuals. The calculations of an administrator of what he can and should do to seek career advancement, it should be noted, affect entrepreneurialism and innovation, but not the beneficiaries of policy outputs. Ethnic group

[32] The study also included 55 expatriates working in Zambia. All officials held a position of department head, the equivalent or above and were in the then Office of National Development and Planning or one of the following ministries: Agriculture, Health, Mining and Natural Resources, Youth, Cooperatives and Social Development, Finance, and Local and Provincial Government. For a more detailed discussion of this study, see Dennis L. Dresang, "Entrepreneurialism and Development Administration," *Administrative Science Quarterly*, 18 (March, 1973), 76–85.

needs and career demands are not necessarily incompatible, but neither are they consistent with each other. From the pattern of development expenditures, it would appear that bureaucrats concerned with development are guided primarily by professional criteria, and not by parochial interests.

Zambian administrators would be unique if they never deviated from wholly economic and commercial norms and never granted parochial favors. The point here, however, is that any role as spokesman of an ethnic group that a bureaucrat might assume is constrained by his area of authority and is compromised by his desire to pursue a successful career.

Although the Bemba victories at the 1967 Mulungushi Conference do not appear to have resulted in substantial disadvantages for other groups in the country, there was widespread belief that non-Bemba were in fact suffering. This belief prompted the political changes that began in late 1969 and culminated in the reversal of Bemba dominance and the decision made early in 1972 to make Zambia a one-party state. These events sharply demonstrate that the representative character of the top levels of government—both politicians and civil servants—is critically important politically, regardless of the actual impact on decision making.

Ethnicity, Politics and Administration: Balance

The discontent generated by the Bemba hold on senior positions in the government and ruling party took a variety of forms. The formation of the United Party has already been mentioned. When it seemed clear that this party was widening its Lozi base and securing support from concerned elements in other provinces, President Kaunda banned the party and detained its leaders. In the Eastern Province, local UNIP officials helped to foster a movement called "Umodzi Ku Mawa" (Unity in the East), which threatened secession of Nyanja areas if the imbalances of the Mulungushi Conference were not redressed. In three provinces there were demonstrations protesting visits of Bemba officials, including the vice president, touring the area.

The General Elections held in December, 1968, clearly signaled the apprehension. Although UNIP won a comfortable majority of the seats in the National Assembly, it lost two major regions of the country, and victory elsewhere was sometimes accompanied by violence and intimidation. The Southern Province continued its overwhelming support for the African National Congress. With the United Party banned, the Lozi electorate turned to ANC, which used United Party organization and ran United Party leaders as its candidates. Bemba domination was a major grievance throughout the country. ANC officials used the issue in their campaigns and local UNIP officials in non-Bemba areas tried to steal ANC thunder by themselves opposing Northern Province control.[23]

The 1968 elections not only dramatized the widespread concern about Bemba domination but also complicated the problems of imbalance. Voters in the Western Province, for instance, chose between ANC and UNIP candidates. All but one of these candidates were Lozi. In expressing their discontent with the ruling party and electing ANC candidates, voters in the Western Province removed from office three Lozi cabinet ministers and one junior minister. The effect was to eliminate almost completely Lozi representation in the upper echelons of the ruling party. The only Lozi cabinet minister who survived the elections had a Copperbelt constituency. Other Lozi political leaders sat on opposition benches in the post-1968 parliament.

It was clear that in the interests of national unity and stability, President Kaunda had to take a more active role in ensuring that all groups within Zambia felt represented in the highest and most visible echelons of government. In mid-1969, the President began slowly and gradually to change the government and the ruling party in order to achieve greater ethnic balance. The public sector was expanded through the establishment of public enterprises, thus making more positions available. Ministries were reorganized. Plans were initiated to revise the party structure to prevent reoccurrences of the 1967 Conference.

Since a major purpose of these changes was to substitute a pattern of predominance of one group with a pattern of balance, those most threatened were members of the allegedly dominant group. Indeed, in August, 1969, Vice President Kapwepwe resigned his post, complaining that there was discrimination against the Bemba and that he was not accepted as the legitimate Vice President by all sectors of the society. Within 24 hours after Kapwepwe's resignation, President Kaunda seized the opportunity to disband the Central Committee that had been elected in 1967, suspend the party constitution, appoint governing committees within UNIP to be responsible to him and reorganize government ministries and portfolios. Kapwepwe was persuaded to rescind his resignation but was stripped of the substantial power he had been given immediately after the 1968 elections, when both planning and finance had been placed under his charge. The two top ministries, as defined by President Kaunda—Rural Development and

[23] Scott and Molteno, "The Zambian General Elections."

Table 7. Comparison of Ethno-Regional Identity of Senior Government Officials, 1968 and 1972

	Total Zambian Population %	Cabinet Ministers		Perm. Sectys.		Total	
		1968 %	1972 %	1968 %	1972 %	1968 %	1972 %
Bemba	31	27	22	32	13	31	17
Nyanja	19	20	13	26	42	22	28
Tonga	13	13	17	15	4	14	11
Lozi	13	27	13	15	21	22	17
Others	24	13	35	11	21	11	28
	N=4,100,000	N=17	N=23	N=19	N=24	N=36	N=47

Finance and Planning—were assigned to a Nyanja and a Tonga, respectively.

As part of the changes set in motion by the President, in November, 1970, Mainza Chona was appointed by Kaunda as Zambia's Vice President, and Simon Kapwepwe was assigned a ministerial portfolio. This change involved more than the replacement of a Bemba by a non-Bemba. Kapwepwe has acquired, rightly or wrongly, the reputation of being the Bemba leader, and so the symbolic blow to that group was substantial.

The response by Bemba Members of Parliament, among others, was a series of charges of discrimination and corruption and a call to establish a new political party. After some hesitation, Kapwepwe launched the United Progressive Party (UPP) in August, 1971. The UPP had a Bemba base, although it tried to broaden that base by articulating the grievances of those unhappy with the slow pace of economic development and the direction of leadership in Zambia.[34] The UPP had only a limited opportunity to test its ability to mobilize an electorate, however. Violent clashes ensued between UNIP and UPP supporters when they vied with one another in by-elections. More than 200 UPP leaders and sympathizers were placed in detention, and finally President Kaunda banned the party and arrested Kapwepwe himself. In the aftermath of these events President Kaunda departed from his intent to persuade Zambian voters to make the country a one-party state through the ballot box. In February, 1972, he declared that the country was going to be a single-party state, and appointed a commission to determine how the constitution should be changed to implement that decision.

Like the non-Bemba complaints of discriminatory treatment in 1968, Bemba charges in 1972 are only partially justifiable. All Bemba who had been occupying top positions have not been removed. Men of Bemba identity occupy cabinet posts, permanent secretary positions and directorships in public corporations. On the other hand, non-Bemba are now serving as Vice President, Head of the Civil Service, Commander of the Air Force, Commander of the Armed Forces, and other positions once held by Bemba. Furthermore, as Table 7 demonstrates, the Bemba can argue that they have less than their share of the most visible posts in government, especially at the permanent secretary level.

According to the data in Table 7, the composition of the senior levels of government failed to reflect the size of ethno-regional groups both in 1968 and in 1972. As long as nonascriptive criteria are used in appointing permanent secretaries and other senior civil servants in an effort to secure the most appropriate talent and experience for these jobs, the goal of a perfectly representative bureaucracy will probably always be elusive. Ethnic arithmetic appears to have been employed more extensively in filling positions at the cabinet level and at the head of major commissions, corporations, and military units. Of the top 29 positions in this category, there were Bemba in 24 per cent, Nyanja in 17 per cent, Tonga and Lozi in 14 per cent each and members of smaller groups in the remaining 31 per cent. In general, those outside of the major ethno-regional groups were receiving significantly more favorable treatment in 1972 than they received in 1968. The Bemba, on the other hand, were under-represented.

Since December, 1972, Zambian leaders have sought support throughout the nation by maintaining a representative balance in the ethnic backgrounds of senior officials and containing political conflict within the framework of a one-party state. The legacy of previous events, however, presents a formidable barrier to national unity. President Kaunda and his lieutenants are confronted by the Bemba perception that there is a systematic attempt to disadvantage them. The Tonga, under the leadership of the African National Congress, have always been suspicious of

[34] Robert Molteno, "Zambia and the One Party State," *East Africa Journal*, 9 (February, 1972), 6–18.

UNIP and the one-party state idea. In addition, he conflict and hostility unleashed by the 1967 Mulungushi Conference in almost all sections of he country have left many scars and cast doubt on the capacities of Kaunda as a leader. Considering as well the grievances emerging from Zambia's conomic difficulties and the new level of authoriarianism in the country, one must be less than anguine about political stability in the immediate uture. International crises in Southern Africa, owever, may neutralize domestic discontent and xtend the tenure of the incumbent Zambian egime.

Conclusion

In applying the lessons of Zambia to other ituations, one must understand the fundamental haracter of the bureaucratic environment that nay counter the forces for ethnic representation. n a government commission, for instance, the ppointment of an individual to represent the inerests of an ethnic community may clearly and lirectly bind that individual to maintain the conidence of his ethnically defined constituency. Loss f that constituency may decrease an incumbent's sefulness and prompt a search for another repreentative. For a civil servant, on the other hand, hile ethnic identity may affect career advanceent, there is no need to maintain an ethnic folowing. Here the basic forces prompting repreentative behavior are absent. Ethnic politics, owever, are not likely to distinguish between ypes of administrative units in pressing for repreentative bureaucracy.

The fundamental political importance of ethicity in a country like Zambia is that differences n language, geographic area, political constituency, administrative divisions and development eeds coincide and are summarized in political hetoric with ethnic labels. In some respects, the lynamics of politics in Zambia resemble sectional or regional politics in countries with longer political histories as self-governing nations and with higher levels of economic development. Class divisions in Zambia demonstrably exist, but class identity has not been politicized and is not important subjectively.

An important area of concern in the ethnoregional conflict in Zambia is that one group might seize control of the public bureaucracy and use it for its own advantage. This concern was particularly evident when the Bemba acquired a dominant position within the ruling party in 1967. However, Bemba predominance never went further than appointments at the uppermost echelon of government. The demise of Bemba pre-eminence within the ruling party and state bureaucracy was the result both of their own limited political power and of the fears and activity among non-Bemba that were generated by ethnic imbalance.

The Zambian evidence indicates that an ethnic group cannot rely on representation in the bureaucracy as a resource for effectively pursuing group interests. Individuals are constrained by the authority attached to their positions, by the requirements and options of specific policy issues, and by the criteria used in determining career advancement. To the extent that ethnicity is perceived to be a factor for securing promotions, it is likely to affect the level of initiative and activity, but not necessarily the content of that activity.

Participants in the Zambian political process have acted on the common sense conclusion that the social identity of government officials influences their behavior and priorities. Patterns of development activity do not support this conclusion. Regardless of the actual direction and content of administrative performance, however, the perceived representative character of the most visible echelons of the government has a vital impact on ethnic politics.

Levels of Sociocultural Development Among Soviet Nationalities: A Partial Test of the Equalization Hypothesis*

BRIAN SILVER
Florida State University

Official party doctrine has always emphasized the equality of nationalities in the Soviet Union. In addition to the juridical guarantee of self-determination and the right to develop native cultures that are "national in form and socialist in content," this doctrine envisions the eventual equalization of social and economic development of nationalities and regions. Since nationalism and ethnic antagonisms in the past are said to have derived from inequalities among and within nationalities, the elimination of such inequalities in the framework of a socialist system is viewed not only as an important goal in itself but as a necessary condition for the elimination of ethnic conflict.

In this paper I shall assess the degree to which major Soviet nationalities have in fact tended to converge or diverge in their levels of sociocultural development. Evidence confirming a trend toward equalization may be taken as supporting the official view that the "nationalities problem in the USSR has been solved" through the increased drawing-together (*sblizhenie*) in the development of nationalities and national regions. But such evidence cannot be taken to support the view that leveling is an accomplishment peculiar to the socialist economic and social organization of the Soviet Union, since declines in regional inequality (and in internal economic inequality more generally) have been shown to be characteristic also of nonsocialist societies as they reach relatively advanced stages of development.[1]

In fact, empirical evidence does tend to confirm that regional and ethnic inequality in the USSR has been reduced over time and continues to decline on certain social and cultural dimensions but that inequality has increased on others. This evidence at once complements and contrasts with the findings of two scholars who have relied on economic rather than sociocultural indicators. In his detailed study of regional economic inequality in the USSR, Vsevolod Holubnychy showed that in contrast with a tendency toward equalization during the period prior to World War II, the levels of regional development tended to diverge between 1958 and 1964.[2] Using different indicators, I. S. Koropeckyj similarly showed a gradual leveling tendency between 1950 and 1958 but an increasing disparity in regional economic development between 1958 and 1968.[3] Thus the case for the equalization hypothesis is at best mixed and appears to be contradicted by recent trends in economic indicators.

This paper will test the equalization hypothesis on units of analysis not employed by Holubnychy and Koropeckyj. These authors used measures focusing not on *ethnic groups* but on *administrative regions*. Given the shortage of economic data pertaining to specific ethnic groups and the authors' formulation of the problem in terms of the regional distribution of investment and resources, this regional focus is understandable. But both authors did intend also to determine whether the economic development of *ethnic groups* was equalizing, not simply whether regional inequali-

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[1] Jeffrey G. Williamson has noted a curvilinear relationship between level of economic development and the existence of regional inequality: as a rule inequality is lowest when the levels of economic development are at the lower and higher ends of the continuum, while countries in an intermediate stage are typified by greater inequality. See J. G. Williamson, "Regional Inequality and the Process of National Development: A Description of the Patterns," *Economic Development and Cultural Change*, 13, No. 4, Part 2 (July, 1965), 3–84. For confirmation of this pattern in the Canadian case, see Alan G. Green, "Regional Inequality, Structural Change, and Economic Growth in Canada—1890–1956," *Economic Development and Cultural Change*, 17 (July, 1969), 567–583. A similar curvilinear pattern has been hypothesized and demonstrated to be true of income distributions (not necessarily strictly related to regional differentials): see Simon Kuznets, *Economic Growth and Structure: Selected Essays* (New York: Norton, 1965), pp. 257–287; and Irving B. Kravis, "International Differences in the Distribution of Income," *Review of Economics and Statistics*, 42 (November, 1960), 408–416.

[2] Vsevolod Holubnychy, "Some Economic Aspects of Relations Among the Soviet Republics," in *Ethnic Minorities in the Soviet Union*, ed. Erich Goldhagen (New York: Praeger, 1968), pp. 50–120.

[3] I. S. Koropeckyj, "Equalization of Regional Development in Socialist Countries: An Empirical Analysis," *Economic Development and Cultural Change*, 21 (October, 1972), 68–86. For a similar argument, although not presenting empirical evidence, see Ann Sheehy, "Some Aspects of Regional Development in Soviet Central Asia," *Slavic Review*, 31 (September, 1972), 557–558.

ties existed. Therefore, it is important to try to verify the equalization hypothesis by employing indicators bearing more directly on ethnic equality. This is especially important because measures of regional equality seriously overestimate the degree of ethnic equality, as I shall later show. This paper, then, will not only employ a number of measures of social and cultural development that Holubnychy and Koropeckyj did not use but will make comparisons of ethnic group development in addition to comparisons of regional development.

Sources of Conflict with Egalitarian Goals

In addition to advocating the achievement of ethnic and regional equality, the Soviet regime adheres to several competing goals. First, as Alex Inkeles has noted, the goal of regional equality has always been subordinated to the goals of *military security* and country-wide *economic growth*.[4] In a similar vein, Koropeckyj has attributed the recent reversal of the trend toward inter-republican leveling to the military-strategic objective of dispersing productive capacity to the eastern regions of the country (primarily *within* the Russian Republic) and to the attempt to maximize the growth rate of the entire economy by directing investment away from economic sectors and regions with low productivity and toward those with higher immediate returns on investment.[5]

Second, the promotion of *regional specialization* may conflict with regional equalization. It is not surprising to find in the recent volume outlining the ninth five-year plan (1971–1975) a lengthy section on the "Geographical Distribution of Productive Forces of the USSR and the Development of the Economies of Union Republics."[6] But it *is* surprising, in light of the doctrinal emphasis usually given to regional equalization, that this section refers not once to "equalization" but stresses instead the overall growth of production and living standards in the union republics as well as the rational development and allocation of the country's resources. Here, the need for regional specialization appears to supersede the goal of regional equality. The importance of regional specialization was also emphasized in a recent article in the authoritative party journal *Kommunist* by the Chairman of the State Planning Commission, N. K. Baibakov:

> Each republic has the possibility in a planned way to specialize in those sectors and products that correspond to its natural and economic conditions, the labor skills of the population, [and] the socio-historical and nationality factors; that is, [to specialize] in those sectors and products that are most efficient for it and at the same time necessary for the complex development of the country as a whole.[7]

For purposes of this study, then, one cannot assume that leveling of regional or ethnic development implies that all regions are to become identical to one another.[8]

But as an important monograph on the development of communism in the USSR notes, the need for specialization does not preclude some leveling of regional differences:

> The eventual equalization of the economic and cultural level of national Soviet republics is being accomplished now on a new material-technical base common to all nations of the USSR, on the basis of a socialist division of labor, of specialization and cooperation in the production of Soviet republics. . . . This excludes both the tendency towards unnecessary universalism in the economy of each republic and the narrow, deformed specialization characteristic of capitalism.[9]

In other words, while natural conditions or other priorities in the planned economy of the entire country may compel some regional specialization of economic roles, all regions are expected both to become more internally diversified and to experience substantial progress toward equality of

[4] Alex Inkeles, "Soviet Nationality Policy in Perspective," in *Social Change in Soviet Russia* (New York: Simon and Schuster, 1968), p. 262.

[5] As Koropeckyj notes, the shift in investment from the western to the eastern parts of the Soviet Union began in the late 1920's and 1930's, principally as a strategic measure. See I. S. Koropeckyj, *Location Problems in Soviet Industry Before World War II: The Case of the Ukraine* (Chapel Hill: University of North Carolina Press, 1971), Part I.

[6] N. K. Baibakov, ed., *Gosudarstvennyi piatiletnii plan razvitiia narodnogo khoziaistva SSSR na 1971–1975 gg.* (Moskva: Politizdat, 1972), pp. 245–281. Note, however, that the projected rates of growth in industrial production are generally higher for the less advanced regions than for the more industrialized ones, so conceivably the goal of equalization is implicitly recognized.

[7] N. Baibakov, "Ekonomika SSSR—Edinyi narodnokhoziaistvennyi kompleks," *Kommunist*, 1972, No. 16, p. 35. For a similar point, see the recent authoritative monograph on nationality relations: P. N. Fedoséev et al., eds., *Leninizm i natsional'nyi vopros v sovremennykh uslovliakh* (Moskva: Politizdat, 1972), p. 248.

[8] Koropeckyj has observed that the goal of regional economic specialization may compete with that of equalization but he assumes, nevertheless, that the eventual development aim of Soviet doctrine is to achieve an equal amount of industrialization in all regions of the USSR (Koropeckyj, *Location Problems in Soviet Industry*, esp. chap. 4). While this assumption may be essentially valid as an expression of official doctrine, one may question the validity of relying primarily on indices of "industrialization" as the principal indicators of regional equalization. Some alternative measures will be suggested below.

[9] *Stroitel'stvo kommunizma i razvitie obshchestvennykh otnoshenii* (Moskva: "Nauka," 1966), p. 256.

social and economic development (such as increased living standards and educational levels).

Finally, the goal of *regional* equalization may conflict with the equalization of *ethnic groups*. In the more backward areas of the Soviet Union, the levels of urbanization and industrialization of the *regions* in fact far exceed the levels of urbanization and industrialization of the *indigenous populations* of these areas. The cities of the non-Russian republics are often numerically dominated by Russians and other nonindigenous workers, and these "immigrants" tend especially to dominate the more highly skilled occupations. Indeed, migration in the USSR among most of the non-Russian nationalities examined here has been primarily *intra*-republican from villages and isolated settlements to urban areas, while *inter*-republican migration, sometimes planned to accelerate the development of certain regions, has predominantly involved movement of members of the economically more advanced groups (such as the Russians, Armenians, Tatars, and Jews) to the less developed areas or to regions of rapid economic growth.[10] As John A. Armstrong has emphasized, the Soviet Union has largely avoided the path commonly followed in other countries in the early stages of development where an "internal proletariat" of relatively unskilled workers migrates from the poorer regions of the country to the richer ones.[11]

These migration patterns not only commonly provoke ethnic conflict over "cadres policy," but the very process of social equalization of nationalities may build resentment among newly trained locals against the "outsiders" who occupy "their" jobs. This source of tension is sometimes explicitly acknowledged by Soviet social scientists.[12] At the same time, the unskilled native who lacks requisite linguistic and technical skills may be discouraged from seeking jobs in cities dominated by skilled Russians and other Russian-speaking immigrants.[13] Of course, as educational levels and the quality of Russian-language training improve among indigenous rural populations, one would expect rural youths to compete for higher-skilled and more remunerative jobs in the cities.[14] Nevertheless, some rural non-Russians possessing the necessary skills might choose to stay in the countryside if they perceived urbanization to be concomitant with an undesired degree of Russification. Moreover, there is evidence of impediments to official efforts to expand the teaching of Russian.[15]

Thus an examination of the possible gap between the development of national regions and of native populations is important not only because of its direct relevance to the equalization hypothesis but also because of the broader implications of such a lag for various aspects of Soviet social policy.

Measuring Sociocultural Equality

The consideration of possible conflicts between ethnic equalization and other regime goals influences the selection of indicators for social and cultural development. First, economic specialization of regions may counteract absolute equalization of regions but at the same time *need* not do so. Regional *populations* can tend to converge in their levels of social development even while economic differentiation persists. Any measure of regional equalization must therefore avoid laying exclusive emphasis on highly skilled manpower, industrialization, or urbanization. It must on the contrary anticipate the endurance of a division of the labor

[10] See V. I. Perevedentsev, "Contemporary Migration in the USSR," *Soviet Geography*, 10 (April, 1969), 192–208.

[11] John A. Armstrong, "The Ethnic Scene in the Soviet Union," in Goldhagen, *Ethnic Minorities in the Soviet Union*, pp. 7–8; and John A. Armstrong, "Communist Political Systems as Vehicles for Modernization," in *Political Development in Changing Societies: An 'Analysis of Modernization*, ed. Monte Palmer and Larry Stern (Lexington, Mass.: D. C. Heath, 1971), pp. 136–137. The typical pattern of migration in the early stages of development, from the less advanced to the more advanced regions, has been outlined by Williamson, "Regional Inequality," 5–6 and 8.

[12] See, for example, O. I. Shkaratan, "Etnosotsial'naia struktura gorodskogo naseleniia Tatarskoi ASSR (po materialam sotsiologicheskogo obsledovaniia)," *Sovetskaia etnografiia*, 1970, No. 3, p. 14; and Iu. V. Bromlei and V. I. Kozlov, "Leninizm i osnovnye tendentsii etnicheskikh protsessov v SSSR," *Sovetskaia etnografiia*, 1970, No. 1, p. 13. For discussions of conflict over staffing of administrative and party positions in the USSR, see Yaroslav Bilinsky, "The Rulers and the Ruled," *Problems of Communism*, 16 (Sept.-Oct., 1967), 16–26; and Teresa Rakowska-Harmstone, *Russia and Nationalism in Central Asia: The Case of Tadzhikistan* (Baltimore: The Johns Hopkins University Press, 1970), esp. chap. 5.

[13] See V. I. Perevedentsev, *Migratsiia naseleniia i trudovye problemy Sibiri* (Novosibirsk: "Nauka," 1966), chap. 4, translated in *Soviet Sociology*, 9 (Winter, 1970–1), 456–458.

[14] This is the commonly proposed solution to rural overpopulation and urban manpower shortages in some of the national areas. See, for example, the recent account of this problem in Tadzhikistan, in "The Tadzhik Republic's Labor Problem," *Current Digest of the Soviet Press*, 25, No. 4 (February 21, 1973), 11–12.

[15] See, for example, the criticisms of the inadequate amount and quality of Russian-language instruction in Azerbaidzhan, in *Current Digest of the Soviet Press* (*CDSP*), 24, No. 43 (November 22, 1972), 23, and *CDSP*, 24, No. 51 (January 17, 1973), 23–24; as well as the criticisms of such instruction in Estonia and Turkmenia, in *CDSP*, 24, No. 51 (January 17, 1973), 23.

force between industrial and agrarian sectors (and regions). As a result, measurement of regional differences in economic structure alone would not disprove the equalization hypothesis. But since modernizing tendencies are expected to affect both rural and urban areas (indeed, official Soviet doctrine forecasts eventual parity in income levels, educational and cultural facilities, and living conditions between agricultural and industrial populations), evidence of persistent or increasing sociocultural disparity among regions or among urban and rural populations taken separately *would* contradict the equalization hypothesis.

Second, because national republics and the indigenous populations in these republics may not develop at the same rate, in assessing interregional variance in sociocultural development one should measure the development of indigenous and nonindigenous populations separately, or at the very least, the differences between ethnic group and regional levels of development. Such differences have not been examined in the previous studies cited here.

Ideally, then, one should employ indicators sensitive to both urban-rural and nationality differences. But systematic published data that meet *both* of these requirements are rather scarce. Despite the incompleteness of data on income and production for regions of the Soviet Union, Holubnychy and Koropeckyj were able to use a number of indirect indicators of regional economic development. If, however, one seeks to compare *nationalities* residing within particular regions, the shortage of data is more severe. The lack of data on occupational structures, incomes, or consumption for specific nationalities forces the researcher to rely primarily on social and cultural rather than economic indicators. Fortunately, Soviet census reports, regional statistical handbooks, and a few scattered monographic and periodical sources do provide some systematic data on a number of indicators of sociocultural development for regional and ethnic populations. Although these data do not cover a broad array of social and cultural dimensions, they do provide enough *systematic* evidence to permit cross-sectional and longitudinal comparisons of the degree of equalization experienced by Soviet ethnic groups.

Because it is presently impossible to examine a wide variety of indicators of sociocultural modernization that meet the dual criterion of urban-rural and nationality specificity, this paper employs a limited set of indicators that other scholars have found to be useful for measuring social modernization. Although no rigorous effort will be made to test how cohesive the various indicators are, they are intended to reflect two dimensions of sociocultural development: educational or occupational skill levels; and family role structures.

Education and Occupational Skills. Given the extensive scholarly literature on the subject, it is unnecessary to demonstrate that formal education is a key medium for spreading the communicative and technological skills essential to modern economies and societies.[16] The present analysis employs two types of indicators of educational achievement: (a) indicators showing whether groups have achieved the minimum level of education necessary for skilled labor roles; and (b) indicators of the attainment of higher education and specialized training.

Published statistics provide information on the proportion of persons in each nationality group that has achieved at least an "incomplete secondary" education—seven or eight years of formal schooling. This is an important stage in a student's career, for at this point he encounters four basic educational options (or fewer if his previous school performance does not qualify him for certain choices): (1) to continue his general education in a secondary school (through 10 years) and then, if he qualifies, to enroll in a higher educational institution; (2) to enroll in a *tekhnikum* for three or four years of specialized training in a technical skill at a semi-professional level; (3) to enroll in a vocational school to acquire a skilled manual-labor or low-level technical skill; or (4) to enter the labor force, usually in a low-skilled occupation.[17] The completion of "incomplete secondary" education thus signals the achievement of the minimal level of formal education essential for access to most skilled occupations. Consequently, equalization in the proportions of different nationality groups that had attained incomplete secondary education would

[16] For a useful brief analysis of the relevant literature, see Olive Banks, *The Sociology of Education* (London: B. T. Batsford, 1971), chap. 10. For discussion of the development and structure of the Soviet educational system, see Jaan Pennar, Ivan I. Bakalo, and George Z. F. Bereday, *Modernization and Diversity in Soviet Education: with Special Reference to Nationality Groups* (New York: Praeger, 1971).

[17] The variety of options is actually greater than revealed here, because a person who enters full-time employment may still enroll in evening or correspondence courses, and a person who eventually graduates from a specialized secondary educational institution (or *tekhnikum*) may later enroll in a regular secondary school. Furthermore, a graduate of a regular secondary school may also enroll in a *tekhnikum*. For a useful brief outline of the structure of the Soviet school system, see Ann S. Goodman and Murray Feshbach, *Estimates and Projections of Educational Attainment in the USSR: 1950–1985*, U.S. Bureau of the Census, International Population Reports, Series P-91, No. 16 (Washington: Government Printing Office, 1967).

indicate that all nationalities possessed an equal quotient of individuals available for skilled occupations.

But the common acquisition of this minimal level of education would not imply equalization in more advanced training. How far each nationality has progressed beyond this minimal level may be gauged by figures on the number of individuals of each nationality who: (a) are enrolled in specialized secondary or higher educational institutions, (b) have graduated from secondary (both general and specialized) and higher educational institutions, or (c) are currently employed as "specialists" or "scientific workers." To avoid redundancy, I shall rely on indicators of higher educational achievement and current occupation.

The data on specialized manpower have a particular drawback not shared by the figures on educational attainment: they have an urban bias. This problem arises because the figures on specialists do not distinguish the types of occupations but simply lump together all "specialists" by level of education. Since the overwhelming majority of graduates of higher and specialized secondary educational institutions enter nonagricultural jobs, the available gross figures on specialists may exaggerate the inter-nationality and inter-republican disparities not simply because there is wide inter-regional disparity in the skill levels of urban and rural workers taken separately but also because of inter-regional differences in levels of urbanization.[18] Consequently, one must treat the data on specialized manpower with some caution in order to avoid simply introducing an unintended proxy variable for urbanization into the analysis. This is not to deny the significance of gaps in the professionalization of labor forces taken as a whole, but to say that the relatively gross occupational indicators currently available are not sensitive to the variety of social processes actually occurring within urban and rural areas taken separately. On the other hand, the new data on *education* in the 1970 census are divided into urban and rural components.

Family Role Structure. A second set of indicators of social modernization that meet the criteria of urban-rural and nationality specificity pertains to the status of women in the household and the economy. Scholars have frequently linked changing family structures, particularly the breakup of extended family networks or their adaptation to urban and industrial settings, with modernization. Although we lack systematic data on the decline of extended families among Soviet nationalities, we do have a few other indicators of family role structure. Taking the nuclear family as a frame of reference, I shall assume that the liberation of women from strictly domestic roles denotes the modernization of family structures.[19] Although certainly not indicating the total elimination of sex discrimination in employment or other spheres of social activity, the extent to which women have been freed from the primary burden of child-rearing and have attained access to formal education and to occupations outside the household will be taken as an indicator of the modernization of the family structure.[20]

Specifically, four indicators of female status will be used: (1) the relative educational levels of females and males; (2) the incidence of early marriage; (3) birthrates; and (4) female employment outside the household.

To measure the relative educational levels of females and males one may use a method similar to that adopted by D. Peter Mazur.[21] One may calculate for each nationality a "female-male educational ratio" by dividing the percentage of women who have attained incomplete secondary education by the percentage of men who have attained that level. A ratio of unity represents parity in the achievement of this educational level by females and males.

Second, many scholars have noted the relationship between urbanization and the tendency to delay marriage. The correlation between societal development and age at first marriage is especially strong for women.[22] The Soviet Union conforms

[18] This argument is based on the assumption that urban work forces absorb greater proportions of highly skilled manpower than rural work forces. Consequently, if Republic A has a larger proportion of specialists than Republic B, this may be because A is more urbanized than B even though within the urban and rural sectors *taken separately* Republic A and Republic B may have identical proportions of skilled manpower.

[19] For discussion of the link between the breakup of the extended family and the growth of equality between the sexes, see William J. Goode, *World Revolution and Family Patterns* (New York: Free Press, 1963), pp. 20–22. For an interesting empirical analysis of the relationship between level of education and intra-family equality in the USSR, see A. L. Pimenova, "Novyi byt i stanovlenie vnutrisemeinogo ravenstva," in *Sotsial'nye issledovanila, vyp. 7: Metodologicheskie problemy issledovanila byta* (Moskva: "Nauka," 1971), pp. 34–45.

[20] For further discussion of the relationship between modernization and changing family role structure, see Allan Schnaiberg, "Measuring Modernism: Theoretical and Empirical Explorations," *American Journal of Sociology*, 76 (November, 1970), 399–425; and Allan Schnaiberg, "The Modernizing Impact of Urbanization: A Causal Analysis," *Economic Development and Cultural Change*, 20 (October, 1971), 80–104.

[21] D. Peter Mazur, "Fertility Among Ethnic Groups in the USSR," *Demography*, 4, No. 1 (1967), 172–195.

[22] For a review of relevant literature and an analysis of some trends contrary to this generalization in recent years (within industrialized countries),

to the general pattern: taking the entire population of the USSR together, the average age of new brides rose from 23 in 1910 to 27 in 1960, while the percentage of women who married before age 20 declined from 54.5 to 26.3 during the same period.[23] But there were and still are dramatic differences in the age at marriage of various Soviet ethnic groups; despite a nearly universal trend in the Soviet era toward increasing age at marriage for all nationalities, groups that traditionally adhered to Islam still have a substantially greater propensity toward early female marriage than do groups adhering to the Orthodox or some other Christian faith. Measurement of the degree of convergence among Soviet nationalities in the incidence of early marriage may therefore highlight the weakening of previous religious sanctions on marriage practices as well as the increasing tendency toward women's emancipation.

A third indicator of female status for which data may be brought to bear is birthrates.[24] The lack or crudeness of data unfortunately limits the usefulness of this indicator, since actual birthrates are available for administrative regions but not for specific nationalities. As a rough approximation of differences in birthrates across nationalities, however, one may calculate child-woman ratios from the 1926 and 1959 census reports. Since the 1959 census report provides nationality-specific age distributions only for relatively gross categories (especially for the younger age groups), I shall calculate the child-woman ratio as the number of children aged 0–9 per 1000 women aged 20–49.[25]

A fourth, and final, indicator of female status is female employment outside the household. It has long been a goal of the Soviet regime to free women for productive labor by releasing them from the exclusive role of managing the household and raising children. The three indicators of female status discussed heretofore may thus be viewed as bearing on this fourth indicator, either because the three are independent causal variables affecting female employment or because all four measures, including female employment, are indicators or products of a single underlying factor: attitudes toward women. The meagerness of available data unfortunately forbids a detailed analysis of female employment. The single source supplying information on women's employment by nationality for 1959 does not reveal the types of employment. Because of this limitation, as well as the lack of data to make longitudinal comparisons, I shall treat female employment primarily as a check on the validity of interpretations of the trends in the other indicators of female status.

By means of this basic set of indicators, the levels of sociocultural development of the fifteen nationalities having union republic status will be compared. While these fifteen groups are but a fraction of the 93 official "indigenous" nationalities of the USSR (or of the 126 nationalities enumerated in the 1959 and 1970 census reports, including 33 groups identified as foreign), taken together they account for 90 per cent of the population of the USSR as of 1970. Moreover, they are accorded the highest political status in the Soviet federation; consequently, they are not only the politically most significant groups but available social and economic statistics are far more extensive and complete for these fifteen groups than for the remaining nationalities.[26]

Analysis

The Equalization of Ethnic Groups

Education and Occupational Skills. The analysis of the relative sociocultural development of ethnic groups begins with an examination of how closely the fifteen union republic nationalities have approached parity in attaining incomplete secondary education. Tables 1a and 1b present the raw data and summary statistics covering four census dates. In Table 1a the scores for 1926 represent the number of *literate* persons per 1000 members of the given nationality; the scores for 1939 and 1959 represent the number of persons per 1000 popula-

see Ruth B. Dixon, "Explaining Cross-Cultural Variations in Age at Marriage and Proportions Never Marrying," *Population Studies*, 25 (July, 1971), 215–233; and Goode, *World Revolution and Family Patterns*, pp. 40–48.

[23] V. I. Kozlov, *Dinamika chislennosti narodov: metodologiia issledovaniia i osnovnye faktory* (Moskva: "Nauka," 1969), p. 120. For longitudinal figures on age at first marriage and the relationship between marriage age and birthrates, see R. I. Sifman and L. E. Darskii, "Pokazateli brachnosti i plodovitosti zhenshchin," *Vestnik statistiki*, 1967, No. 8, pp. 87–95, translated as "Indices of Rate of Marriage and Fertility in Women," in *Soviet Sociology*, 8 (Summer, 1969), 94–109.

[24] For discussion of the links between economic development, family structure, and fertility, see especially Kingsley Davis and Judith Blake, "Social Structure and Fertility: An Analytic Framework," *Economic Development and Cultural Change*, 4 (April, 1956), 211–235.

[25] This is the procedure adopted by Mazur, "Fertility Among Ethnic Groups," and Kozlov, *Dinamika chislennosti narodov*.

[26] For brief descriptions of the cultural and political backgrounds of Soviet nationalities, see the dated but still indispensable source: Frank Lorimer, *The Population of the Soviet Union: History and Prospects* (Geneva: League of Nations, 1946), pp. 50–65. For more recent classifications and descriptions of the political and social characteristics of the nationalities, see especially Armstrong, "The Ethnic Scene in the Soviet Union," and Vernon V. Aspaturian, "The Non-Russian Nationalities," in *Prospects for Soviet Society*, ed. Allen Kassof (New York: Praeger, 1968), 143–198.

Table 1a. Educational Levels of Soviet Nationalities in 1926, 1939, and 1959[a]

	1926[b]			1939[b]			1959[b]		
	Urb	Rur	Both	Urb	Rur	Both	Urb	Rur	Both
Russians	688	391	445	165	36	82	364	188	284
Ukrainians	599	392	415	164	48	81	395	211	278
Belorussians	610	344	365	191	31	58	383	163	219
Moldavians	(354)[d]	(254)[d]	(257)[d]	—[c]	—	—	282	137	151
Estonians	—	—	—	—	—	—	435	194	307
Latvians	—	—	—	—	—	—	482	269	369
Lithuanians	—	—	—	—	—	—	315	97	170
Georgians	687	336	392	351	69	137	548	282	375
Armenians	508	211	270	207	41	92	392	241	320
Azerbaidzhanis	238	55	86	146	25	52	314	209	247
Kazakhs	240	66	70	67	13	22	264	171	194
Kirgiz	270	45	47	115	6	10	376	185	206
Tadzhiks	72	8	11	74	3	11	244	191	201
Turkmenians	211	18	20	66	7	14	315	233	242
Uzbeks	310	18	37	69	7	16	278	198	214
Mean for 11 Groups[e]	401.2	171.3	196.2	146.8	26.0	52.3	352.1	206.5	252.7
Coefficient of Variation (V) for 11 Groups	.507	.914	.873	.556	.778	.767	.229	.159	.214
Mean for 15 Groups							359.1	197.9	251.8
Coefficient of Variation (V) for 15 Groups							.228	.232	.263

[a] Sources: Data for 1926 are from the Soviet census: Ts. S.U., *Vsesoiuznaia perepis' naselenila 1926 goda* (56 vols.; Moskva: Ts. S.U., 1928–1933). Data for 1939 and 1959 are from the 1959 census report: Ts. S.U., *Itogi vsesoiuznoi perepisi naselenila 1959 goda* (16 vols.; Moskva: Gosstatizdat, 1962–3). Volume and page numbers are omitted from citations of census figures to avoid unnecessary cumbersomeness.

[b] The figures for 1926 represent the number of persons per 1000 population who were *litterate*, because no educational figures were offered in the census. The 1939 and 1959 figures represent the number of persons per 1000 population who have "incomplete secondary" or higher educational attainment. All figures pertain to the portion of the nationality group residing within its official national area (not to the USSR-wide population).

[c] Dashes indicate data not available.

[d] The figures for the Moldavians were not included in calculation of means and coefficients for 1926, because at that time only a small portion of the present Moldavian territory was included in the Soviet Union. Although the Belorussians and Ukrainians also experienced substantial accretions of territory and population within the USSR as a result of annexations during and after World War II, we have included these two groups in the calculations for all periods because the newly acquired populations for these groups were not as significant an addition as were the Moldavian acquisitions (relative to the numbers of persons of the given nationality already residing in the USSR).

[e] Calculations for "eleven groups" exclude the Moldavians, Estonians, Latvians, and Lithuanians. A similar procedure is followed in subsequent tables.

tion (of the given nationality) who have attained incomplete secondary *education*. In Table 1b the scores record the number of persons per 1000 population (of the given nationality) aged 10 and over who have attained incomplete secondary education as of 1959 and 1970. The goal of adducing some measures for longitudinal comparison of low-level educational achievement necessitates the use of slightly different indicators for different periods. Inasmuch, however, as chief emphasis here is placed on the *variation* among nationalities rather than overall changes in levels of educational attainment, the differences in types of measures are not crucial. In fact, the particular summary statistic chosen to compare the dispersion in scores among nationalities is specifically designed to compensate for differences in the mean scores. Following Williamson and Koropeckyj, I shall place primary reliance on the coefficient of relative variation (V), which measures the degree of dispersion in the scores relative to the mean score.[27]

[27] For further discussion of this statistic, see Hubert M. Blalock, Jr., *Social Statistics*, 2nd ed. (New York: McGraw-Hill, 1972), p. 88. The computational formula is:

$$V = \frac{\sigma}{\bar{X}} = \frac{\sqrt{\frac{\Sigma(\bar{X} - X_i)^2}{N}}}{\bar{X}}$$

Table 1b. Educational Levels of Soviet Nationalities in 1959 and 1970[a]

	1959			1970		
	Urban	Rural	Both	Urban	Rural	Both
Russians	454[b]	247	364	580	320	494
Ukrainians	476	264	343	605	332	458
Belorussians	478	212	282	616	270	401
Moldavians	360	190	208	548	289	337
Estonians	500	230	358	581	314	462
Latvians	549	314	426	600	370	490
Lithuanians	382	120	209	523	212	356
Georgians	666	365	474	734	458	578
Armenians	533	354	451	599	375	519
Azerbaidzhanis	449	316	365	536	359	437
Kazakhs	380	251	282	522	355	403
Kirgiz	536	280	309	613	366	407
Tadzhiks	359	284	298	465	358	387
Turkmenians	413	346	363	488	407	433
Uzbeks	403	298	318	511	389	420
Mean for 11 Groups[c]	467.9	292.5	349.9	569.9	362.6	448.8
V for 11 Groups	.176	.281	.174	.127	.127	.126
Mean for 15 Groups	462.5	271.4	336.7	568.1	344.9	438.8
V for 15 Groups	.178	.236	.222	.113	.166	.140

[a] Source: 1970 Soviet census: Ts. S.U., *Itogi vsesoiuznoi perepisi naseleniia 1970 goda* (Moskva: "Statistika," 1973).

[b] The main cell entries represent the number of persons per 1000 population aged 10 and over who have "incomplete secondary education." The figures pertain to the portion of the nationality group residing within its official national area.

[c] Calculations for "11 groups" exclude the Moldavians, Estonians, Latvians, and Lithuanians.

An examination of the scores for 1926 (Table 1a) suggests several important initial conclusions. First, there is a stark contrast of scores between the highest and lowest groups, ranging from the Russians, with 44.5 per cent of the population literate, to the Tadzhiks, with a mere 1.1 per cent. Second, the gap between the Christian groups taken as a whole and the Muslim groups is enormous. The data in this and subsequent tables are arranged so that the Muslim groups are the bottom six (beginning with the Azerbaidzhanis) and the Christians, the top nine. One can readily see that in 1926 all of the Muslim groups were located well below the mean literacy score for the eleven nationalities, while all of the Christian groups were well above the mean; that is, there is a clear bimodal distribution, with the mean score for the Muslim groups being 4.5 per cent literate, and the mean for the Christians, 37.7 per cent. If the education of nationalities were to be equalized, these six Muslim groups had the greatest burden of catching up. The 1926 scores also suggest a third tentative conclusion: not only are urbanites substantially more literate than ruralites, but the degree of dispersion among nationalities is substantially lower among urban residents than among rural residents. Although the differences in the absolute magnitudes of the scores for the highest and lowest groups are greater for urban than for rural populations, the average degree of

where

V = coefficient of variation,
σ = standard deviation,
$\bar{X}$ = mean score of all groups,
X_i = score of the i'th group, and
N = number of cases.

Williamson and Koropeckyj rely upon a formula that *weights* the individual cases by population size. This is justified in that the data-reporting units (usually administrative units) are often somewhat arbitrary divisions that do not correspond to theoretically or empirically meaningful regional divisions of the country. In the present study, however, we have relied on unweighted scores because the boundaries of nationality areas and of the actual ethnic populations under study do demarcate meaningful analytic units. It is precisely the gaps among nationalities, and among nationality regions (republics) taken as units, with which we are concerned. Moreover, using weighted scores tends to obscure more "general" trends because of the inordinate population sizes of the two largest regions: the RSFSR, with 130 million inhabitants (in 1970), and the Ukrainian SSR, with 47 million, represent approximately 73 per cent of the population of the fifteen republics in our sample. Finally, and most importantly, from the standpoint of official doctrine, there is no theoretical reason to treat the scores of some nationalities as more important than any others, especially since our study is restricted to the fifteen nationalities accorded the highest official political status.

Table 2. Variation in Educational Attainment of Nationalities by Urban-Rural and Age Groups: 1959[a]

	Age Group									
	16–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60+
Coefficient of Variation[b]										
Total (1)	.103	.143	.282	.284	.358	.487	.585	.694	.813	.904
Urban (2)	.075	.102	.224	.220	.296	.395	.475	.571	.682	.741
Rural (3)	.140	.175	.340	.327	.362	.466	.578	.716	.908	1.107[c]
Mean Per Capita Educated[b]										
Total (4)	704.5	637.3	475.5	488.9	417.3	279.4	187.0	130.3	88.5	44.0
Urban (5)	795.9	737.1	566.5	587.3	536.1	408.1	304.6	235.1	176.6	92.9
Rural (6)	652.7	568.7	406.9	417.3	338.0	191.9	111.5	65.3	40.1	19.5
Urban minus Rural (7)	143.2	168.4	159.6	170.0	198.1	216.2	193.1	169.8	135.6	73.4
Rural/Urban (8)	.820	.772	.718	.711	.630	.470	.366	.278	.227	.210
Year Born:	1940–1943	1935–1939	1930–1934	1925–1929	1920–1924	1915–1919	1910–1919	1905–1909	1900–1904	before 1900

[a] Source: Calculated from figures in the 1959 Soviet census, Ts. S.U., *Itogi vsesoluznoi perepisi naselenila 1959 goda: SSSR* (Moskva: Gosstatizdat, 1962), pp. 238–239.

[b] The mean scores are the number of persons who have attained at least "incomplete secondary" education per 1000 persons of the given nationality. The means and coefficients of variation are unweighted, and are based on the populations of the 15 nationalities of union republic status in 1959. The figures pertain to the portion of each nationality residing in the "area of primary settlement" as defined in the census for each nationality.

[c] The coefficient of variation can take on values exceeding unity if the distribution is extremely skewed. In this instance, the scores of 92 for the Latvians and 43 for the Estonians enormously increase the sum of the squared deviations beyond the squared deviations attributable to the other cases, while increasing the mean by a relatively smaller amount.

variation is lower among urban ($V=.507$) than among rural ($V=.914$) groups. This evidence tends to confirm that modernization (here: urbanization) correlates with a reduction of inequality among nationalities.

The data for subsequent periods corroborate the above conclusions. Given the change of measures, one cannot compare the absolute magnitudes of the 1926 scores with those of later years, but the degree of variation among nationalities shows a substantial reduction over time in all three population categories: urban, rural, and total populations. Only the slight increase between 1926 and 1939 of variation among urbanites appears anomalous, a result which might be explained by the change of indicators. Overall, substantial progress has taken place not only toward reducing inter-nationality variation but also toward raising the levels of education among those groups that had most severely lagged in the pre-Soviet era. If the six Muslim nationalities continue on the average to be less educated than non-Muslim groups as of 1970, it is the traditionally Orthodox Moldavians and (predominantly) Roman Catholic Lithuanians that are the least educated groups.

One can gain a more revealing picture of the longitudinal trends if one turns to comparisons of age cohorts. The evidence in Table 2, which shows the attainment of incomplete secondary education for ten age cohorts in 1959, reveals a strong trend toward increasing education and decreasing inter-nationality variation over time. With the single exception of the 25–29 year-olds (born between 1930 and 1934), who suffered an enduring loss of educational opportunities because of the destruction and disruption of World War II, each successive age cohort displays a reduction in the inter-nationality variation within the urban and rural segments as well as the whole populations. One may also observe that the rural inhabitants still lag behind the urbanites in average educational attainment and reduction of variation: scores comparable to the mean and variation for the youngest rural age cohort had already been reached by urbanites attending school five to ten years earlier (the rural mean of 652.7 and V of .140 for the 16–19 year-olds interpolate the scores for the urban cohorts aged 20–24 and 25–29).

In summary, although the rural inhabitants continue to lag behind the urbanites, both populations have undergone enough homogenization (across nationalities) to permit one to predict nearly total equalization in the attainment of incomplete secondary education in the next few decades. The scores for the youngest age cohort

d 16–19) in the 1970 census simply continue rends among the younger cohorts depicted in le 2: the inter-nationality variation and urban- l differences are much smaller for the 16–19 -olds in 1970 than for the 16–19 year-olds in) (this conclusion is based on figures not vn here). Taking the attainment of universal omplete secondary education" as an accom- ied fact, Soviet educational planners have in already set the goal of universal secondary ation.

efore turning to other types of educational ., one additional facet of Table 2 must be d. If one compares the absolute differences in percentages of urbanites and ruralites who finished incomplete secondary school, be- ing with those aged 60+ in 1959, one finds the absolute gap between the average urban rural levels of education tends to increase stantially with each successive age cohort until, hing a peak among the 40–44 year-olds, it be- to close. In other words, the relationship veen increasing education (over time) and the nitude of the urban-rural gap in education is ilinear and takes the shape of an inverted " The inter-nationality gap displays a similar rted "U" curvilinearity: if one measures the rquartile range of the educational scores ng the fifteen nationalities, beginning again the 60+ category, the range increases with successive cohort until, reaching a peak with 40–44 year-olds for the urbanites and the 25– ear-olds for the ruralites, it declines.[28] This of curvilinear relationship between develop- t (here: education) and inequality (here: the an-rural and inter-nationality differences) con- ns with that suggested by Williamson. Only *absolute* differences, however, reveal such a ern. The *relative* differences in scores, mea- d by the rural/urban education ratio and the ficient of variation (for the inter-nationality erences), decline continuously with each new ort. Thus, initial stages of development are racterized by a growth in absolute inequality veen urban and rural areas and among nation- es even while relative inequality decreases. h either measure, however, the trend among youngest age cohorts is toward greater ality.

the above evidence reveals considerable gress toward equality in educational attain- t at the minimal level associated with incom- e secondary education, the available data on her levels of education reveal more substantial differences among nationalities. Tables 3 and 4 show both the degree of change and the continuing differences among nationalities. Table 3 presents figures on the number of specialists per capita employed in 1960 and 1970.[29] Contrasting the coefficients of variation in Tables 1a and 1b with those in Table 3, one can readily observe that the variation in numbers of specialists per na-

This summary is based on calculations not shown . In this fifteen-case sample, the interquartile ge is calculated as the absolute difference between scores of the 4th highest and 12th highest cases each age cohort.

Table 3. Numbers of Specialists Among Soviet Nationalities in 1960 and 1970[a]

(per 10,000 population)

	Specialists with Higher Education		Specialists with Secondary Education	
	1960	1970	1960	1970
Russians	181	313	301	498
Ukrainians	139	253	220	396
Belorussians	120	225	205	373
Moldavians	51	121	92	168
Estonians	195	355	293	492
Latvians	178	302	272	438
Lithuanians	129	251	190	388
Georgians	329	460	247	340
Armenians	266	360	201	287
Azerbaidzhanis	163	224	168	245
Kazakhs	96	183	114	198
Kirgiz	97	165	100	153
Tadzhiks	78	139	92	125
Turkmenians	104	170	100	143
Uzbeks	77	152	82	143
Mean	146.9	244.9	178.5	292.5
Coefficient of Variation	.494	.382	.423	.442

[a] Sources: Figures on specialists in 1960 are drawn from Ts. S.U., *Narodnoe khoziaistvo SSSR v 1960 godu* (Moskva: "Statistika," 1961). Figures on specialists in 1970 are from Ts. S.U., *Narodnoe obrazovanie, nauka i kul'tura v SSSR* (Moskva: "Statistika," 1971). The population bases for the 1960 figures are the 1959 Soviet census totals, while the base for the 1970 figures is the 1970 census report.

[29] The category "specialists" includes all employed persons who have diplomas from higher educational institutions or specialized secondary educational institutions regardless of their field of study or the type of work currently engaged in. For highly useful discussions of Soviet manpower statistics, see Murray Feshbach, *The Soviet Statistical System: Labor Force Recordkeeping and Reporting Since 1957*, U.S. Bureau of the Census, International Population Statistics Reports, Series P-90, No. 17 (Washington, D.C.: Government Printing Office, 1962); and Ann S. Goodman, *Estimates and Projections of Specialized Manpower in the USSR: 1950–1975*, U.S. Bureau of the Census, International Population Reports, Series P-91, No. 21 (Washington, D.C.: Government Printing Office, 1970).

Table 4. Numbers of Scientific Workers Among Soviet Nationalities in 1939, 1960, and 1970[a]

(per 100,000 population)

	1939	1960	1970
Russians	59	201	474
Ukrainians	34	95	246
Belorussians	32	80	210
Moldavians	—[b]	27	92
Estonians	—	207	466
Latvians	—	190	381
Lithuanians	—	127	306
Georgians	93	309	568
Armenians	98	287	567
Azerbaidzhanis	40	169	297
Kazakhs	10	63	149
Kirgiz	5	60	131
Tadzhiks	8	62	110
Turkmenians	12	71	120
Uzbeks	10	62	132
Mean for 11 Groups[c]	36.5	132.6	273.1
Coefficient of Variation for 11 Groups	.880	.678	.629
Mean for 15 Groups		134.0	283.3
Coefficient of Variation for 15 Groups		.636	.581

[a] Sources: The 1939 and 1960 figures on scientific workers are from Ts. S.U., *Vysshee obrazovanie v SSSR: statisticheskii sbornik* (Moskva: "Statistika," 1961). Figures for 1970 are from Ts. S.U., *Narodnoe obrazovanie, nauka i kul'tura v SSSR* (Moskva: "Statistika," 1971). The population bases for the 1939 figures are from Gosplan SSSR, *Kul'turnoe stroitel'stvo SSSR: statisticheskii sbornik* (Moskva i Leningrad: Gosplanizdat, 1940). The bases for 1960 and 1970 figures are derived as in Table 3.

[b] Dashes indicate data not available.

[c] The "eleven groups" exclude the Moldavians, Estonians, Latvians, and Lithuanians.

tionality greatly exceeds the variation in incomplete secondary education in comparable years.

Comparison of the coefficients for specialists at the two time points for which data are available discloses a mixed pattern: between 1960 and 1970 the inter-nationality variation in numbers of specialists with higher education decreased from .494 to .382, a substantial drop, while the variation for specialists with secondary education increased slightly from .423 to .442.

Once one uses more stringent standards for comparing inter-nationality variation, then, the inequality in social development becomes more apparent. The figures in Table 4 regarding "scientific workers" also support this conclusion.[30] Although one may observe a moderate degree of movement toward equalization in the numbers of scientific workers per capita, the coefficient of variation for scientific workers in 1970 was .581, much higher than the coefficients for specialists and for incomplete secondary education in the same year. Tables 3 and 4 thus reveal large persistent inter-nationality gaps and only moderate progress toward the elimination of those gaps in recent years. A quite similar pattern may be discerned in figures on higher educational attainments in the 1970 census, which show that although the coefficient of variation declined from .708 to .458 between 1959 and 1970, substantial variation remains (the coefficients are based on raw data not presented here). From this evidence one may infer that it will be a long time, if ever, before the considerable equalization achieved at lower levels of education is matched at more advanced levels of training.

The groups lagging furthest behind are once again those that had furthest to go at the beginning of the Soviet era: the Muslim nationalities. But the Ukrainians, Belorussians, Lithuanians, and Moldavians also trail behind a number of other groups. One might explain this lag in part by the absence of Soviet control over all or substantial portions of the populations of these nationalities during the interwar period. The annexations of new territories exposed the new populations to the greatly increased educational opportunities that had been created for the other Soviet nationalities in the 1920s and 1930s. In effect, then, the newly annexed populations had an approximately 20-year deficit in educational opportunities to make up—except, in such regions as Estonia and Latvia, where extensive school networks existed under the independent governments. The severe war devastation in both the newly acquired and pre-existing Soviet territory may also have contributed to the delayed development of these groups. Age-specific figures on the "incomplete secondary education" of Belorussians, Ukrainians, and Russians (and to a very mild degree, also Armenians) indicate that both the 25–29 and the 30–34 year-olds obtained less education on the average than their older or younger co-nationals. For all other nationalities, however, only the 25–29 year-olds showed such a decline. The significant lag of these three nationalities in gaining specialized secondary and higher education may therefore partially derive from

[30] The category "scientific workers" partially overlaps with that of "specialists" and includes every employed person to whom the Ministry of Higher and Secondary Specialized Education has granted a degree and/or an academic title, *plus* persons lacking such a degree or title if they are employed in certain occupations or organizations (such as scientific research institutions or higher educational institutions). For further discussion, see Feshbach, pp. 29–31.

Table 5. Class Structure of Soviet Nationalities: 1939 and 1959[a]

(in percentages)

	White Collar "Employees"		Workers		Collective Farmers		Totals[c]	
	1939	1959	1939	1959	1939	1959	1939	1959
Russians	18.7	22	38.2	54/23[b]	38.5	24	95.4	100.0
Ukrainians	13.1	13	29.3	34/15	54.6	53	97.0	100.0
Belorussians	10.8	12	20.7	31/11	60.4	57	91.9	100.0
Moldavians	—[d]	4	—	13/4	—	83	—	100.0
Estonians	—	22	—	51/19	—	27	—	100.0
Latvians	—	18	—	46/19	—	36	—	100.0
Lithuanians	—	14	—	34/12	—	52	—	100.0
Georgians	17.0	23	12.4	23/10	58.2	54	87.6	100.0
Armenians	19.7	20	21.5	40/17	52.5	40	93.7	100.0
Azerbaidzhanis	9.5	15	12.2	24/8	70.1	61	91.8	100.0
Kazakhs	8.0	16	25.6	44/6	60.8	40	94.4	100.0
Kirgiz	4.6	8	7.5	22/4	83.8	70	95.9	100.0
Tadzhiks	5.7	8	8.7	18/4	79.2	74	93.6	100.0
Turkmenians	6.1	9	11.7	22/5	77.2	69	95.0	100.0
Uzbeks	5.4	8	11.1	27/4	79.6	65	96.1	100.0
Mean for 11 Groups	10.8	14.1						
V for 11 Groups	.492	.385						
Mean for 15 Groups		14.0						
V for 15 Groups		.414						

[a] Source: Iu. V. Arutiunian, "Izmenenie sotsial'noi struktury sovetskikh natsii," *Istoriia SSSR*, 1972, No. 4, pp. 6 and 13.

[b] Figures on "workers" in 1939 include "state farmers." The first figure for 1959 also includes state farmers in the worker category; while the second figure includes only industrial workers.

[c] Totals in 1939 do not add to 100 because some persons fall into an "other" category, which includes primarily private farmers. The "others" are included with the figures on collective farmers in 1959. This classification and treatment of the "others" is unavoidable because our source treats the data in this way.

[d] Dashes indicate data not available.

their loss of school facilities during the war, and their inordinately high involvement in the war. Certainly, before concluding that there had been a deliberate policy of "holding back" specialized education from Ukrainians and Belorussians, one must consider these alternative explanations.[21]

Table 5 presents another type of evidence on the relative numbers of skilled workers among nationalities. Calculating the percentage of the work force holding jobs as white collar "employees" (*sluzhashchie*), one finds the variation among nationalities to have declined from .492 in 1939 to .385 in 1959.[22] But once again substantial disparity characterizes the rates of access to more highly skilled economic roles. It is important to add that such disparity is not confined to the predominantly urban-oriented jobs so-far discussed (scientific workers, specialists, and white-collar workers). Although we lack comparable data for earlier years, Table 6 shows that as of 1959 there was a good deal of variation in technical skills among the agrarian work forces of different nationality groups. Available data on the attainment of higher education do reveal, however, that

[21] Bilinsky overlooks these factors when examining the apparent limitations on the training and use of specialized Ukrainian manpower, while Aspaturian says that Stalin deliberately restricted Ukrainian access to higher education (a policy that Aspaturian says was reversed by Stalin's successors). Although there may be some evidence to support Bilinsky's and Aspaturian's views, certainly one cannot automatically attribute all deviations from strict proportionality in access to education to deliberate intentions of the Soviet leadership without first considering other factors that might account (or partially account) for such deviations. See Yaroslav Bilinsky, "Assimilation and Ethnic Assertiveness Among Ukrainians of the Soviet Union," in Goldhagen, *Ethnic Minorities in the Soviet Union*, pp. 147–184; and Aspaturian, "The Non-Russian Nationalities," pp. 178–179.

[22] The term "employee" (*sluzhashchie*) as used in the present analysis roughly denotes persons employed in nonmanual occupations. Sometimes the term is used in a more restrictive sense to refer to low- and middle-level administrative, supervisorial, or office personnel, as distinguished from the "intelligentsia" of specialists with higher education or of persons in high administrative positions.

Table 6. Skilled Agricultural Workers as Percentage of Total Agricultural Work Force in 1959[a]

(for residents in official national area only)

	(1) Supervisors and Specialists	(2) Equipment Operators	(3) All skilled Workers (1+2)
Russians	3.6	10.6	14.2
Ukrainians	2.7	7.0	9.7
Belorussians	2.3	4.3	6.6
Moldavians	1.1	4.2	5.3
Estonians	5.3	6.4	11.7
Latvians	4.7	6.2	10.9
Lithuanians	3.2	4.7	7.9
Georgians	3.9	1.5	5.4
Armenians	3.8	3.3	7.1
Azerbaidzhanis	3.5	3.3	6.8
Kazakhs	6.5	7.9	14.4
Kirgiz	2.4	3.2	5.6
Tadzhiks	1.9	2.2	4.1
Turkmenians	1.9	2.9	4.8
Uzbeks	2.2	4.3	6.5
Mean	3.27	4.80	8.07
Coefficient of Variation	.426	.487	.400

[a] Source: Iu. V. Arutiunian, *Sotsial'naia struktura sel'skogo naseleniia SSSR* (Moskva: "Mysl'," 1971), p. 85. Also available in Iu. V. Arutiunian, "Izmenenie sotsial'noi struktury sovetskikh natsii," *Istoriia SSSR*, 1972, No. 4, p. 14.

within both the urban and rural populations internationality variation declined considerably between 1959 and 1970: between 1959 and 1970 the variation dropped from .636 to .426 among urbanites, and from .581 to .306 among ruralites (these figures are based on data not shown here).

To summarize the analysis thus far, although by almost every measure there has been substantial movement towards equalization of nationalities during the Soviet era, this movement is strongest for the least demanding indicator (incomplete secondary education), and especially for the youngest age cohorts. Once one compares the equalization of higher educational attainment and skilled manpower, whether for the entire nationalities or for their urban and rural populations separately, one observes significant remaining dispersion among nationalities.

Family Role Structure. We shall now examine change in family role structures among nationalities. One measure of the relative status of women and men is the female-male education ratio discussed above: the proportion of females having incomplete secondary education divided by the proportion of males who have achieved such education. The results of these calculations are presented in Tables 7a and 7b. For want of education figures in 1926, one must again rely on literacy rates, thereby reducing the strict equivalency of measures over time. Nonetheless, the overall trend reveals a remarkable decline in the magnitude of the female-male differences. The average female-male literacy ratio in 1926 was .284, indicating that only 284 women were literate for every 1000 literate men.[33] For some nationalities, particularly the Muslim groups, these ratios were minuscule. Only 5 Tadzhik women were literate for every 1000 literate Tadzhik men; while the highest ratio among Muslim nationalities amounted to a mere 170 literate women per 1000 literate men. By 1970, however, although the ratios for Muslim groups still trailed behind those of all Christian nationalities, the size of the gap between the Muslim and Christian groups had significantly decreased. Consequently, the coefficient of variation among the total populations of the nationalities had fallen from the very high figure of .853 in 1926 to a low .102 in 1970. In short, even if as of 1970 only 830 women had attained incomplete secondary education for every 1000 men with that degree of education, the absolute gap between women's and men's education and the absolute and relative differences among nationalities had progressively decreased from earlier levels.

Age at marriage is a second indicator of female emancipation. In the immediate pre-Soviet era it was especially Muslim groups that encouraged females to marry early.[34] But the Armenians, who adhered predominantly to the Gregorian (Orthodox) Church, also favored early female marriage.[35] One would therefore expect the loss of religious sanctions over marriage practices and the breakdown of numerous customary practices resulting from the increasing education and urbanization of Soviet nationalities to reduce the prevalence of early marriages. The evidence in Table 8, which

[33] When speaking of the relative "numbers" of women and men who have attained literacy, I shall for convenience assume that the proportions of men and women in the population are identical. But note that because the ratio is calculated as a ratio between the *proportions* of women and men who have attained literacy (education), the ratio is not actually distorted by the unequal numbers of men and women in the population.

[34] For detailed empirical analyses of female status and birthrate differentials among (former) Muslim groups, see G. A. Bondarskaia, "Rol' etnicheskogo faktora v formirovanii territoriial'nykh razlichii rozhdaemosti," in *Voprosy demografii (Issledovaniia, problemy, metody)*, ed. A. G. Volkov et al. (Moskva: "Statistika," 1970), pp. 160–175; and the various articles in M. K. Karakhanov, ed., *Problemy narodonaseleniia* (Moskva: Izdatel'stvo Moskovskogo universiteta, 1970).

[35] Mary K. Matossian, *The Impact of Soviet Policies in Armenia* (Leiden: E. J. Brill, 1962), p. 5.

Table 7a. Female-Male Education Ratios: 1926, 1939, 1959[a]

	1926			1939			1959		
	Urban	Rural	Total	Urban	Rural	Total	Urban	Rural	Total
Russians	.814	.507	.583	.908	.609	.804	1.031	.914	.996
Ukrainians	.712	.462	.496	.774	.590	.684	.909	.759	.825
Belorussians	.723	.407	.441	.833	.537	.686	1.008	.799	.889
Moldavians	(.252)	(.311)	(.249)	—	—	—	.783	.706	.719
Estonians	—	—	—	—	—	—	1.009	1.010	1.016
Latvians	—	—	—	—	—	—	.957	.949	.960
Lithuanians	—	—	—	—	—	—	.987	1.000	1.000
Georgians	.885	.677	.727	.880	.663	.791	.991	.995	.969
Armenians	.713	.272	.400	.707	.367	.578	.972	.851	.922
Azerbaidzhanis	.282	.093	.170	.394	.167	.287	.667	.590	.616
Kazakhs	.233	.067	.081	.245	.167	.189	.650	.563	.589
Kirgiz	.181	.024	.034	.424	.100	.188	.681	.596	.600
Tadzhiks	.055	.006	.005	.294	.167	.222	.628	.517	.672
Turkmenians	.127	.030	.026	.182	.077	.125	.610	.786	.730
Uzbeks	.199	.097	.161	.343	.182	.250	.682	.662	.664
Mean for 11 Groups	.4476	.2402	.2840	.5440	.3296	.4367	.8026	.7265	.7702
V for 11 Groups	.6753	.9376	.8526	.4859	.6590	.5888	.2086	.1960	.1913
Mean for 15 Groups							.8377	.7771	.8111
V for 15 Groups							.1927	.2074	.1924

[a] Source: 1926 data are from the 1926 Soviet census (cited earlier), and represent the proportion of females *literate* divided by the proportion of males literate. The 1939 and 1959 data are from the 1959 Soviet census (cited earlier) and represent the proportion of females having incomplete secondary education (or beyond) divided by the proportion of males who have achieved such education. All figures apply to the portion of each nationality residing in its official national territory only.

records the proportion of women aged 16–19 who are married, supports this expectation. From an average of 46.1 per cent of young women married in 1926, the figure declined to 15.6 per cent in 1970 (for the 11 groups for which data are available at both time points).

At the same time, Table 8 presents some unexpected relationships. The inter-nationality variation (*V*) *increased* between 1926 and 1959 rather than decreased: from a *V* of .531 in 1926 to a *V* of .749 in 1959 (for the 11 nationalities for which data are available at both time points). But then the variation *decreased* significantly by 1970 to .355—a level below that for 1926. How might one account for this pattern? It is clear that every nationality examined here exhibits a decline in the incidence of early marriage over time. But the inter-nationality differences grew relative to the mean between 1926 and 1959. In fact, many of the Muslim groups deviated absolutely much more from the mean score in 1959 than in 1926. Thus, despite the decreasing *incidence* of early marriage between 1926 and 1959, affecting every Union Republic nationality, there was an increased *relative variation* among nationalities. But by 1970, both the absolute and relative differences among nationalities declined substantially so that there is an overall curvilinear pattern to the change in inter-nationality inequality over time.

Like the early marriage indicator, the next indicator of cultural diversity—birthrates—brings inter-nationality differences into clearer focus between 1926 and 1959. Table 9 shows the coefficient of variation to have climbed from .141 in 1926 to .336 in 1959. In addition, while the child-woman ratios for all Christian groups have progressively *decreased*, the ratios for all Muslim groups except the Azerbaidzhanis have *increased*. One must, of course, keep in mind certain drawbacks to the use of child-woman ratios to measure fertility rates, especially their sensitivity to differential child mortality rates and to discontinuities in the age structures of populations. The relatively small inter-nationality differentials in the child-woman ratios in 1926 may obscure strong differentials in actual fertility at that time, while much of the increase in subsequent ratios for Muslim groups may have resulted from decreasing child mortality rates. Nevertheless, differential child mortality rates—in fact minor for 1959—cannot explain the large remaining inter-nationality variation. Moreover, as indicators of the emancipation of women from primarily domestic roles, child-woman ratios have considerable utility in their own right. These

Table 7b. Female-Male Education Ratios: 1959 and 1970[a]

	1959			1970		
	Urban	Rural	Total	Urban	Rural	Total
Russians	.966	.839	.921	.934	.838	.908
Ukrainians	.860	.702	.772	.865	.724	.794
Belorussians	.935	.729	.816	.922	.737	.824
Moldavians	.760	.660	.673	.863	.789	.799
Estonians	.961	.966	.964	.973	.945	.968
Latvians	.917	.904	.919	.948	.915	.937
Lithuanians	.942	.959	.953	.952	.872	.925
Georgians	.955	.904	.923	.964	.891	.929
Armenians	.931	.794	.874	.953	.878	.928
Azerbaidzhanis	.660	.549	.589	.749	.645	.685
Kazakhs	.642	.534	.561	.819	.722	.748
Kirgiz	.684	.556	.566	.868	.717	.744
Tadzhiks	.530	.650	.619	.658	.748	.710
Turkmenians	.620	.752	.707	.731	.824	.786
Uzbeks	.679	.633	.640	.775	.773	.767
Mean for 11 Groups[b]	.769	.695	.726	.840	.772	.802
V for 11 Groups	.199	.169	.186	.112	.094	.102
Mean for 15 Groups	.803	.742	.766	.865	.801	.830
V for 15 Groups	.185	.192	.192	.110	.105	.109

[a] Source: 1970 Soviet census (cited earlier).

[b] The figures are based only on the portion of each nationality that resides in its official national area, and represent the proportion of females (aged 10 and over) having incomplete secondary education or beyond divided by the proportion of males who have achieved such education.

data reveal tremendous inter-nationality differences in the child-rearing burdens placed on women.

In summary, viewing child-woman ratios as measures of women's domestic obligations, the relative differences among nationalities have increased over time. The previously obscured (or, perhaps, minimal) relative variation in fertility had been brought into the open by 1959 and may well have increased in the 1960s despite apparent reductions in overall rates.[36] (Unfortunately, the available 1970 census data do not allow us to calculate child-woman ratios for that year.) Perhaps as a consequence of varying child-woman burdens, or as a consequence of a common causal factor affecting women's status, female employment outside the household—our fourth indicator of female status—correlates highly negatively with child-woman ratios, as Table 10 shows.[37]

Summary. The preceding analysis has revealed a mixed pattern of relationships between sociocultural development and equality among Soviet nationalities. Measured in absolute terms, the increases in levels of education, numbers of skilled workers, and female access to formal education have been enormous, as has the reduction in the incidence of early marriage. Birthrates, too, have shown substantial change over time, though not always in the expected direction. Thus if one were to focus primary attention on the *amount* of change one would have to conclude that almost universal progress toward modernization has

[36] At least as far as one can judge from *regional* indicators, the crude birthrates of all the nationalities considered here dropped off markedly in the 1960s. See *Narodnoe khoziaistvo SSSR v 1970 godu* (Moskva: "Statistika," 1971), pp. 50–51. For more detailed analysis of longitudinal trends in fertility in the USSR, see Jerzy Berent, "Causes of Fertility Decline in Eastern Europe and the Soviet Union," *Population Studies*, 24, No. 1 (March, 1970), 35–58, and No. 2 (July, 1970), 247–292. Extensive discussion and analysis of fertility declines in general and within the USSR in particular is found also in Kozlov, *Dinamika chislennosti narodov*, pp. 162–175 and 233–249; and G. A. Bondarskaia, "Vliianie etnicheskogo faktora na uroven' rozhdaemosti v SSSR i v nekotorye sotsialisticheskikh stranakh evropy," in *Faktory rozhdaemosti*, ed. A. G. Volkov (Moskva: "Statistika," 1971), pp. 52–62.

[37] Actually, the relationship between birthrates and female employment may be reciprocal, with increased employment leading to lower birthrates. For a recent analysis, however, that questions whether any causal linkage exists between birthrates and economic independence of women in the Soviet Union, see D. Peter Mazur, "Fertility and Economic Dependency of Soviet Women," *Demography*, 10 (February, 1973), 37–51.

Table 8. Early Marriage Among Soviet Nationalities: Proportion of 16–19 Year-old Women Married[a]

	1926			1959	1970
	Rural	Rural	Total	Total	Total
Russians	—	—	.150	.091	.088
Ukrainians	.106	.131	.128	.096	.109
Belorussians	.107	.082	.084	.060	.066
Moldavians	—	—	—	.150	.113
Estonians	—	—	—	.042	.048
Latvians	—	—	—	.045	.059
Lithuanians	—	—	—	.047	.052
Georgians	.182	.311	.286	.107	.135
Armenians	.374	.639	.572	.166	.160
Azerbaidzhanis	.615	.568	.579	.272	.179
Kazakhs	.712	.535	.583	.275	.117
Kirgiz	.881	.876	.876	.448	.200
Tadzhiks	.472	.637	.615	.384	.252
Turkmenians	.606	.637	.636	.320	.189
Uzbeks	.527	.568	.560	.323	.219
Mean for 10 Groups	.458	.498	.492		
V for 10 Groups	.546	.473	.477		
Mean for 11 Groups			.461	.170	.156
V for 11 Groups			.531	.749	.355
Mean for 15 Groups				.188	.132
V for 15 Groups				.700	.474

[a] Source: 1926, 1959, and 1970 Soviet census reports (cited earlier). All figures apply to the portion of the nationality group residing in its official national area only, except for the 1926 figures on the Russians, which pertain to the entire Russian population of the USSR.

characterized the sociocultural development of union republic nationalities.

But the initial expectation of a decline in the relative differences of sociocultural development among nationalities has not always proved correct. In general, those aspects of social relations most amenable to planned change over a relatively short time-span (such as access to education) have shown the greatest change and the greatest reduction in inter-nationality variation. Least susceptible to planned change have been certain practices once supported by strong religious sanctions—those relating to marriage, fertility, and female status in general. Thus, only the most recent figures on early marriage show a reduction in variation from the 1926 levels, while fertility differentials still apparently exceed 1926 levels and are, in any event, quite large.

One should not underestimate the importance of the relative intractability of family practices. The still large inter-nationality variation testifies to the ability of family practices to withstand intensive educational campaigns and legal restrictions designed to change these practices.[38] In attempting to account for differential fertility rates among nationalities, Soviet scholars themselves have recognized that family practices are highly resistant to change. M. K. Karakhanov, for example, has observed that rapid socioeconomic progress in Central Asia has outstripped the transformation of popular customs:

> Although economic, social, and cultural progress to a certain degree influences the regulation of fertility level, the relatively persistent character of customary forms and diverse traditions—not excluding the religious—facilitates the persistence of a high level of fertility.[39]

One may propose that Soviet nationalities must advance along two dimensions of sociocultural change before they can become fully equal. First, the provision and use of cultural facilities, particularly educational facilities, must be expanded in order to increase the education and occupational skills of the population. Although Soviet

[38] For a discussion of the legislative aspect, see Gregory J. Massell, "Law as an Instrument of Revolutionary Change in a Traditional Milieu: The Case of Soviet Central Asia," *Law and Society Review*, 2 (February, 1968), 179–228.

[39] M. K. Karakhanov, "Dinamika narodonaselenlia srednei azii," Karakhanov, *Problemy narodonaselenlia*, p. 23.

Table 9. Child-Woman Ratios of Soviet Nationalities: 1926 and 1959[a]

(Number of Children Aged 0–9 per 1000 Women Aged 20–49)

	1926			1959		
	Urban	Rural	Total	Urban	Rural	Total
Russians	—	—	1233	747	1049	863
Ukrainians	940	1397	1341	576	816	714
Belorussians	1038	1512	1468	575	995	836
Moldavians	(1166)	(1395)	(1387)	831	1235	1190
Estonians	—	—	—	518	772	638
Latvians	—	—	—	513	713	612
Lithuanians	—	—	—	670	921	823
Georgians	927	1618	1484	660	1072	905
Armenians	1271	1760	1660	1048	1508	1240
Azerbaidzhanis	1593	1807	1772	1490	1836	1711
Kazakhs	1056	1269	1265	1740	1948	1896
Kirgiz	944	1161	1159	1516	1933	1885
Tadzhiks	1328	1321	1322	1753	1789	1782
Turkmenians	1401	1384	1384	1848	1795	1809
Uzbeks	1193	1096	1112	1743	1916	1878
Mean for 10 Groups[b]	1169.1	1432.5	1396.7	1294.9	1560.8	1465.6
V for 10 Groups	.184	.159	.140	.386	.265	.315
Mean for 11 Groups[c]			1381.8			1410.8
V for 11 Groups			.141			.336
Mean for 15 Groups				1081.9	1353.2	1252.1
V for 15 Groups				.474	.340	.398

[a] Source: 1926 and 1959 Soviet census reports (cited earlier). The data in 1926 apply to the portion of each nationality residing within its official national area as the borders were defined at that time. The data in 1959 apply to the "primary area of settlement" of each nationality as defined in the census report summary volume.

[b] The scores for "10 groups" exclude the Russians as well as the others omitted in previous tables from the 1926 totals. No age breakdowns for the Russian population of urban and rural areas of the RSFSR were provided in the census report.

[c] The scores for "11 groups" exclude the Moldavians, Estonians, Latvians, and Lithuanians.

nationalities have progressed significantly on this dimension, and the degree of inter-nationality variation has continuously decreased over time, at more advanced levels of training large relative differences among nationalities remain.

But the change affecting those social areas most directly linked to technological development—urbanization, education, and occupational structures, for example—only later begins to affect attitudes and practices involving interpersonal, primary-group relations. Family structure and customs related to home life would appear to be less susceptible to transformation than the more macro-level economic and social organization of communities, even though Soviet doctrine condemns "outmoded" traditions such as the subordination of women and the adherence to religious rites and dogma. Because traditional attitudes towards fertility have lingered on, inter-nationality differences in fertility have become more apparent as a result of improved sanitation and health care. In more general terms, the significant equalization of females and males as measured by access to education is often accompanied by the preservation of family practices that help maintain women's primary role in the family and household.[40] This is not to say that little change has taken place in family structure, but only that despite absolute gains in women's standing, vestiges of earlier values may lead to continuing or (for a time) even increased inter-nationality

[40] This conclusion applies not only to Muslim nationalities but also to Christians, especially the Armenians. Although as a group the Armenians are among the three or four most highly educated nationalities in the USSR, they lag behind all other union republic Christian groups in decreasing early marriage and child-woman ratios, and they lag well behind the others in the employment of women outside the household. On the last point, see A. A. Isupov, *Natsional'nyi sostav naseleniia SSSR (po itogam perepisi 1959 g.)* (Moskva: "Statistika," 1964), p. 46.

disparities as the groups undergo economic modernization. The earlier finding, however, of a curvilinear relationship between level of education and inter-nationality differences (measured in absolute terms) does appear to hold true for the early-marriage indicator and may also hold true for birthrates once we can compare more recent data with the presently available data.

Regional versus Nationality Development.

We have yet to consider the extent to which regional development has exceeded or fallen short of nationality development. To investigate this problem we shall focus on one dimension of social development that is of particular political importance: the relative positions of native and non-native specialists in the non-Russian republics. Table 11 compares the *total* number of specialists per capita with the number of *native* specialists per capita in the economies of the union republics in 1960. The differences between the two sets of figures are striking. If the mean number of (total) specialists (per 10,000 population) in the union republics in 1960 was 166.7, the mean number of *indigenous* specialists (per 10,000 natives) was 145.3. Similarly, if in 1960 the mean number of scientific workers (per 100,000 population) in the republics was 145.3, the mean number of scientific workers among the *indigenous* populations of these republics was 128.8. Thus the social development of natives lags behind the social development of the total populations of the Union Republics.

There are some exceptions found among the Baltic groups (Estonians, Latvians, and Lithuanians) and two of the Transcausasian nationalities (Georgians and Armenians). One might account for the latter two exceptions by noting that Georgia and Armenia are distinguished by having (a) very large numbers of native specialists per capita, and (b) very small proportions of non-indigenous residents (especially from outside of Transcaucasia) in their official national areas. Given the exceedingly high educational qualifications of the natives (the Georgians and Armenians are the two most highly skilled of the union republic work forces—see also Table 3), the relatively low qualifications of the longstanding enclaves of other nationalities tend to drag down the per capita figures for the republics as a whole. The high skill levels of the indigenous populations of the Baltic republics may also help explain their deviation, though this argument is less convincing for the Lithuanian case.

More important than the differences in the two sets of mean scores, however, are the differences in the *variation* between inter-republican scores and inter-nationality scores. In 1960, the coefficient of variation for the inter-*republican* differences (including all nationalities residing in each republic) was significantly lower than the coefficient for inter-*nationality* differences (among the titular indigenous nationalities of each re-

Table 10. Correlation Between Child-Woman Ratios and Female Employment Outside the Household: 1959[a]

	Child-Woman Ratio	
	600–999	1000+
Per cent of Females Employed Outside the Household[b]:		
20–34	0	7
35–49	6	0
50–55	1	1

[a] Source: Source of child-woman ratios as in Table 9. Data on female employment are from A. A. Isupov, *Natsional'nyi sostav naselenila SSSR (po itogam perepisi 1959 g.)* (Moskva: "Statistika," 1964), p. 46.

[b] Persons spending their time tending private garden plots or family livestock are not counted as "working outside the household."

Table 11. Number of Total and Native Specialists and Scientific Workers in National Republics: 1960[a]

	Specialists with Higher Education (per 10,000 pop.)		Scientific Workers (per 100,000 pop.)	
	Natives	All	Natives	All
Republic:				
RSFSR	172	177	197	207
Ukraine	124	164	70	111
Belorussia	102	137	49	85
Moldavia	53	115	27	69
Estonia	198	202	197	190
Latvia	173	195	169	160
Lithuania	134	137	129	122
Georgia	325	264	294	226
Armenia	243	233	258	242
Azerbaidzhan	176	198	187	195
Kazakhstan	108	136	74	103
Kirgizia	107	144	68	112
Tadzhikistan	76	118	69	109
Turkmenia	108	148	73	121
Uzbekistan	80	132	71	127
Mean	145.3	166.7	128.8	145.3
Coefficient of Variation	.475	.254	.619	.385

[a] Source: Ts. S.U., *Vysshee obrazovanie v SSSR: statisticheskii sbornik* (Moskva: "Statistika," 1961). The population bases are the 1959 census totals.

Table 12. Class Structures of Total and Native Populations of Union Republics: 1939 and 1959[a]

	Per cent of Work Force in "Employee" Category			
	1939		1959	
	Natives	All	Natives	All
Republic:				
RSFSR	18.7[b]	18.6	22	22
Ukraine	13.1	17.2	13	17
Belorussia	10.8	14.5	12	16
Moldavia	—	—	4	11
Estonia	—	—	22	24
Latvia	—	—	18	21
Lithuania	—	—	14	16
Georgia	17.0	17.2	23	24
Armenia	19.7	14.6	20	22
Azerbaidzhan	9.5	16.6	15	23
Kazakhstan	8.0	17.4	16	21
Kirgizia	4.6	12.4	8	18
Tadzhikistan	5.7	10.2	8	16
Turkmenia	6.1	15.4	9	20
Uzbekistan	5.4	12.9	8	17
Mean for 11 Groups	10.8	15.2	14.0	19.6
Coefficient of Variation for 11 Groups	.492	.160	.388	.143
Mean for 15 Groups			14.1	19.2
Coefficient of Variation for 15 Groups			.411	.187

[a] Source: see source for Table 5.

[b] The table entries represent the percentage of the work forces of the native population or the entire republican population occupied in "white collar" jobs. For the remainder of the distributions in broad occupational categories (workers, collective farmers) see the original sources.

public). For specialists, the inter-republican score was .254, while the inter-nationality score was .475; for scientific workers, the corresponding scores were .358 and .619. These figures support the argument presented at the beginning of this essay that indicators of *regional* equalization are seriously misleading as indicators of inter-*nationality* equalization. As projected, the presence of nonindigenous specialists raises the average qualifications of the workers in the union republics, while reducing the amount of inter-republican variation from what might have occurred if natives alone served in specialized occupations.

The official interpretation, however, that the influx of specialists from outside the regions has accelerated the development of the non-Russian populations is not necessarily confirmed (nor can it be refuted with the present data). If the presence of outsiders lowers inter-republican variation, it does not necessarily accelerate the growth of native cadres of specialists. On the contrary, the presence of outsiders may to some extent impede the opportunities for the training and employment of natives by displacing natives from jobs, raising job competition, and creating a *de facto* need for Russian in certain occupations.

In some cases the employment of non-natives in the non-Russian areas may also induce trained native specialists to find work outside their own republics, particularly for the Ukrainians and Belorussians, in whose republics non-natives occupy 40 per cent of the specialist positions, while about 25 per cent of the native specialists work outside their respective official republics. Of course, such evidence does not conclusively prove that the presence of outsiders induces the natives to emigrate. Indeed, the Ukrainian and Belorussian cases are anomalous, since large minorities of these nationalities were born, raised, and educated outside their official republics. But in most other republics the local economy absorbs at least 90 per cent of the native specialists.[41] The possibility that the influx of non-natives may stimulate the advancement of the non-Russian cadres by stimulating the growth of local industry must thus be balanced against the evidence of deleterious influences.

That the equalization of national republics exceeds the equalization of the native populations dwelling in them may also be gleaned by comparing the class structures of the official indigenous populations of these republics. Table 12 reveals once again the substantial lag of the native behind the general populations of the republics and the significantly greater variation among nationalities than among republics. More important, however, since these data cover two points in time (1939 and 1959), one can now see that among both republics and nationalities variation has significantly declined through time. Extending this comparison on the basis of 1970 census data on higher educational attainments, one finds that between 1959 and 1970 the coefficient of variation declined from .404 to .272 for the *entire* (indigenous and "immigrant") populations of republics, and from .733 to .461 for the *indigenous* populations (these coefficients are based on raw data not presented here). Not that natives are about to

[41] Armenia constitutes a major exception because it has so saturated its own specialized work force with Armenians (92 per cent of the higher specialists in Armenia are Armenians) that almost 50 per cent of all Armenian specialists with higher education must seek employment outside the Armenian Republic.

catch up with the general populations of their respective republics, but the trend toward inter-republican equalization holds true for the native as well as the general populations.

Conclusion

Like Holubnychy and Koropeckyj, we have found uneven support for the equalization hypothesis. Although Soviet nationalities have experienced substantial and continuous equalization over time on measures of education, inter-nationality differences in family role structure show greater persistence and have tended to follow a curvilinear pattern of change.

These apparently contradictory tendencies do not necessarily disprove the equalization hypothesis, however. Instead our results tend to corroborate the pattern noted by Williamson and others, who have found an inverted "U" relationship between economic development and inequality, with economic development leading first toward increasing inequality, then toward decreasing inequality. The mixed support for the equalization hypothesis may be tentatively attributed to the serial or asynchronous occurrence of different aspects of social and cultural development: the initial social change associated with urbanization, mass education, and the development of skilled labor produces increased inter-nationality inequality for a time, which is followed by increasing equality (as shown by the present data); but change in family structures not only begins later but completes its cycle at a slower pace than does change in those social relations more directly linked to technological development. Consequently, when viewing the relative variation among nationalities we may be viewing occupational and educational equality after the highest point of inequality has already been passed (before the Soviet era), while in observing the variation in family role structures we may be tracing the levels of inequality as they achieve the apogee of what may eventually prove to be an inverted "U" relationship. In other words, there is no necessary contradiction in the patterns of relationships observed in the educational and family variables, but rather a difference in the time schedules according to which the two types of social phenomena are changing.

I do not intend to imply that all of the dimensions of social change here studied follow precisely the same pattern. Indeed, the form of the relationship between economic development and inequality depends in part on the measure of inequality employed. For example, the analysis of incomplete secondary education among different age cohorts reveals that even after 1926 one may discern an inverted "U" pattern among the *absolute* differences among nationalities despite the pattern of monotonically decreasing *relative* inequality over time.[42] But for the early marriage indicator, no such curvilinear pattern in the absolute differences occurred, since at each successive time period the interquartile range declined. Furthermore, Holubnychy and Koropeckyj's analyses show that according to some indicators of economic development the relationship between development and regional inequality in the USSR may deviate from the inverted "U" pattern, with recent evidence suggesting increasing divergence among regions during the 1960s. On the basis of an examination of planned growth in per capita output for the 1971-1975 period, Noren and Whitehouse have projected that regional divergence will continue to increase.[43]

Finally, further study of the relationship between nationality and republican development in the USSR is clearly warranted. Here I have shown that measures of regional inequality seriously underestimate the degree of inequality among nationalities in the Soviet Union. The influx of non-native skilled manpower has evidently both created and perpetuated the lag of two-thirds of the nationalities here considered behind the development of their respective republics. This analysis should also caution other researchers who may wish to study regional inequality in multinational countries to distinguish between regional and ethnic inequality.

[42] A similar pattern may be discerned in the measures of specialized manpower in Tables 3 and 4. If one calculates the interquartile range, the *absolute* difference between the 4th highest and 12th highest scores *increased* over time, while the *relative* differences among nationalities tended to *decline*. One would expect both the absolute and relative differences on these measures to decline eventually, thereby conforming to the inverted "U" pattern observed with incomplete secondary education.

[43] James H. Noren and E. Douglas Whitehouse, "Soviet Industry in the 1971–1975 Plan," in *Soviet Economic Prospects for the Seventies*, U.S. Congress, Joint Economic Committee (Washington, D.C.: Government Printing Office, 1973), pp. 223–224.

Models of Statistical Relationship*

HERBERT F. WEISBERG
Ohio State University

The endless stream of proposed measures of statistical relationship is matched only by the countless suggestions offered for selecting among them. The criteria include such features of the statistics as their means of handling "ties," their dependence on the variables' numbers of categories, and especially their operational interpretations. The interpretation question has become dominant, but the central point of this article is that the most basic aspect of selecting relationship measures is choosing a "model" of a relationship. Simply stated, the extreme value conditions of a statistic must be understood before attempting to interpret its intermediate values. Different coefficients embody different models, and the choice between measures is essentially a choice between these models. A classification scheme for different models will be presented in this paper. The substantive utility of the various models will be emphasized, the basic thrust being toward encouraging greater use of multiple measures in order to study several aspects of a relationship.

Introduction

Exactly twenty years ago, Leo Goodman and William Kruskal redirected the study of "Measures of Association for Cross Classifications."[1] Their treatment has created a widespread belief that the worthiness of a coefficient is mainly a matter of its "interpretation"—specifically how intermediate values of the statistic are to be interpreted. What, for example, would a .5 correlation value *mean?* Correspondingly, the debate about the best measure has evolved into a contest between prospective interpretation systems. Probabilistic interpretations have caught the fancy of many analysts,[2] as if such interpretations possess a special mystique that renders their further justification unnecessary. Interpretations have often been based on the numbers of concordant, discordant, and tied pairs of observations,[3] without a clear reason for using pairs of observations as the units of analysis. Costner's efforts have made "proportional-reduction-in-error" interpretations particularly popular,[4] although the form of "error" involved can be so unusual that no one would have suggested its reduction.[5] Measures of predictability have been sought for nominal variables, implying without proof that such predictability is analogous to the association of interval variables. The search for interpretable measures has also spurred the development of new statistics which embody specific methodological concepts (such as those of information theory) and has legitimized the invention of ad hoc coefficients which fit particular substantive needs. But the quagmire of alternative interpretation systems has not simplified the task of the analyst seeking a measure for use in research.

Emphasis on the interpretation of intermediate values of a statistic overlooks the primary question—intermediate to what? Under what conditions does the coefficient attain its maximum and null values? An intermediate value is, after all, intermediate to the situations which the measure treats as a perfect relationship and as a null relationship, so it is important to ask whether the statistic's extreme value conditions are of substantive interest. The conditions under which a coefficient achieves its maximum and null values specify its implicit "model" of a relationship. The interpretation of a measure should be evaluated only within the context of its relationship model. Even probabilistic measures, predictability scores, information theory statistics, and ad hoc coefficients should be evaluated on this basis, though their formulations may obscure their underlying models. The utility of a statistic depends directly on the substantive appropriateness of its model of a relationship.

The stress on interpretations has led to a widespread view that some interpretations (and therefore measures) may be preferable to others. The researcher is tempted to decide which interpreta-

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[1] Leo A. Goodman and William H. Kruskal, "Measures of Association for Cross Classifications," *Journal of the American Statistical Association*, 49 (December, 1954), 732–764.

[2] William H. Kruskal, "Ordinal Measures of Association," *Journal of the American Statistical Association*, 53 (December, 1958), 814–861.

[3] Robert H. Somers, "A New Asymmetric Measure of Association for Ordinal Variables," *American Sociological Review*, 27 (December, 1962), 799–811.

[4] Herbert L. Costner, "Criteria for Measures of Association," *American Sociological Review*, 30 (June, 1965), 341–353.

[5] See, for example, Somers's presentation of a proportional-reduction-in-error interpretation of his d_{xy} in Robert H. Somers, "On the Measurement of Association," *American Sociological Review*, 33 (April, 1968), 291–292, and also Thomas P. Wilson, "A Proportional-Reduction-in-Error Interpretation for Kendall's Tau-*b*," *Social Forces*, 47 (March, 1969), 340–342.

tion seems most correct and then employ only the corresponding measure. Yet once it is realized that different statistics are intended to measure different concepts, it becomes useful to employ multiple measures to analyze various aspects of a relationship. More than one relationship model can be of substantive interest, so several coefficients can be used simultaneously.

In order to keep the task of reasonable magnitude, the present study will emphasize dichotomous independent and dependent variables. This case has been chosen because it is the simplest and most basic statistical situation and because it permits effective illustration of several different models. All comments are intended to apply only to this case unless specifically stated otherwise. Although the dichotomous variable case is somewhat specialized, it helps focus attention on the relationship model underlying a statistic. If the coefficient's underlying model is uninteresting in the dichotomous case, that model and that measure will not have greater utility in the more complex nondichotomous case. Generalization to polytomous variables is appropriate only if the model is useful in studying dichotomies.

The main example used in this paper involves party voting in legislatures, party being the independent variable and the vote for or against a motion being the dependent variable. Political scientists have long been interested in gauging party differences on legislative roll calls. Nonvoting is typically excluded from the analysis, so both variables are dichotomous in two-party legislatures (see Chart 1).[6] The regression slope (known in this field as Rice's index of party differences) is the traditional measure of party voting differences, though the potential applicability of other coefficients has been recognized.[7] The use

		Party		
		Republican	Democrat	Total
Vote	Yes	p_{11}	p_{12}	$p_{1.}$
	No	p_{21}	p_{22}	$p_{2.}$
	Total	$p_{.1}$	$p_{.2}$	1.0

Chart 1. Cross-Tabulation of Party and Vote[a]

[a] Cell entries are probabilities—proportions of the total number of cases that fall in the cells.

of the legislative illustration in the following discussion should not be permitted to conceal the generality of either the problem or the results.

Statistical Models

Measurement of a statistical relationship should be based on a specific relationship model—an image of what would constitute the maximum possible relationship between the variables along with a decision about what the absence of a relationship should mean. Certain extreme conditions merit particular attention because of their substantive meaningfulness and their common usage in statistics. The most prevalent definitions of maximum and null relationships will be listed in this section, after which the models based on combinations of these definitions will be presented and the differences between the models will be clarified.

Maximum relationship. Chart 2 shows various forms of monotonicity which can be regarded as maximum relationships between dichotomous variables. Different statistics embody these different conceptions of the perfect relationship. The definitions below will be given in terms of dichotomous variables, with the more general definitions being listed in Appendix I.

(1) "Strong monotonicity" requires that the independent variable categories be homogeneous but differing on the dependent variable. This is equivalent to two opposite zero cells in the cross-tabulation of dichotomous variables (Chart 2.1). In the legislative example, strong monotonicity corresponds to the two parties being united in opposition to one another.

Some statisticians view strong monotonicity as the only legitimate form of a perfect relationship.

[6] There would be little need to resort to a summary statistic were we considering only the relation between a single vote and party, but the interest usually extends to the importance of party across a wide range of votes. The fundamental concern would be in comparing the strength of party effects to other effects (such as regional divisions and urban-rural differences) or in comparing the role of partisan effects across issue areas, across time, between legislative chambers, and/or between different legislatures. Being able to summarize the importance of party (and other) effects on each vote permits comparing the distributions of the summary measures over the votes.

[7] Grumm has employed the phi-statistic in this regard, Turner used the related chi-square statistic, and MacRae worked with Yule's Q (without reporting specific results). Anderson's handbook on roll-call analysis mentions several standard statistical coefficients, but does not consider the stakes involved in the choice between them. MacRae's roll-call analysis text includes an excellent discussion of measuring party differences, including comments on the choice between the regression slope and Yule's Q. See John G. Grumm, "The Means of Measuring Conflict and Cohesion in the Legislature," *Southwestern Social Science Quarterly*, 44 (March, 1964), 377–388; Julius Turner, *Party and Constituency* (Baltimore: The Johns Hopkins Press, 1951); Duncan MacRae, Jr., *Issues and Parties in Legislative Voting* (New York: Harper and Row, 1970), pp. 175–200; Lee F. Anderson, Meredith W. Watts, Jr., and Allen R. Wilcox, *Legislative Roll-Call Analysis* (Evanston, Ill.: Northwestern University Press, 1966), pp. 43–54.

Vote	Rep.	Dem.	Total
Yes	.60	.00	.60
No	.00	.40	.40
Total	.60	.40	1.00

Chart 2.1
Strong monotonicity
$p_{12} = p_{21} = 0$
Also maximal if:
$p_{11} = p_{22} = 0$

Vote	Rep.	Dem.	Total
Yes	.60	.40	1.00
No	.00	.00	.00
Total	.60	.40	1.00

Chart 2.2
Moderate monotonicity
$p_{21} = p_{22} = 0$
Also maximal if:
$p_{11} = p_{12} = 0$
Or: $p_{12} = p_{21} = 0$
Or: $p_{11} = p_{22} = 0$
Or: $p_{11} = p_{21} = 0$
Or: $p_{12} = p_{22} = 0$

Vote	Rep.	Dem.	Total
Yes	.50	.40	.90
No	.10	.00	.10
Total	.60	.40	1.00

Chart 2.3
Weak monotonicity
$p_{22} = 0$
Also maximal if:
$p_{11} = 0$
Or: $p_{12} = 0$
Or: $p_{21} = 0$

Chart 2. Maximum Relationship Condition[a]

[a] As explained in the text, these are not mutually exclusive conditions; when one rule is satisfied, weaker rules are also satisfied.

Other statisticians routinely correct for unequal numbers of categories or different marginal distributions for the variables, conditions which preclude strong monotonicity. Rather than adopt either of these approaches, these adjustments will be treated here as statistical models with distinctive substantive implications.

(2) "Moderate monotonicity" adjusts for unequal numbers of variable categories, as when the dependent variable lacks variance. It is satisfied if the categories of the variable with the greater number of categories are homogeneous on the other variable. With dichotomous variables, moderate monotonicity requires two zero cells in the cross-tabulation, zero cells which can be in the same column or row (Chart 2.2) or different columns and rows (so that a case of strong monotonicity also satisfies moderate monotonicity). A unanimous legislative vote can be perfectly predicted from party, so this model would treat the relationship of party with unanimous votes as maximal.

(3) "Weak monotonicity" assumes that the categories of the independent variable are as homogeneous on the dependent variable as possible, given differences in variable marginals. This case corresponds to at least one zero cell in the cross-tabulation of dichotomous variables (Chart 2.3). (Note also that cases of strong and moderate monotonicity do satisfy weak monotonicity.) Complete voting cohesiveness by one party would be regarded as full party responsibility even if there is a factional split in the other, since no greater responsibility would be possible, *given the vote marginals.*[8]

These three definitions of a maximum relation ship cumulate. If strong monotonicity exists, then the other forms also hold; if the moderate rule is satisfied, then so is weak monotonicity. Thus fewest votes would be considered maximally related to party under a strong monotonicity criterion while the greatest number of votes would be viewed as maximally related to party under weak monotonicity.

Null relationship. Four important forms for the definition of no relationship are illustrated in Chart 3.

(a) "Accord" occurs when there is agreement between the majorities of the independent variable categories (Chart 3a). The independent variable is regarded as lacking explanatory power unless the central tendencies of the independent variable categories are conflicting. According to this position, party is irrelevant to the vote if a majority of both parties vote together. Similarly class voting would be considered nonexistent if the majorities of all classes vote for the same party.

(b) "Balance" holds when there is no tendency for the cleavages on the two variables to be reinforcing (Chart 3b). Cleavages would be reinforcing if, for example, religious and party divisions

[8] If the marginals are accepted as given, the number of Democrats voting no in Chart 2.3 could range between 0 and 10. If there were 1 Democrat voting no (and 9 Republicans voting no), the relationship between party and vote would not be as strong as in Chart 2.3 and similarly if more than 1 Democrat voted no. If there were 10 Democrats but no Republicans casting negative votes, the party responsibility would again be maximal given the vote marginals. Thus the strongest relationships between party and vote with the given marginals occur when there is one zero cell in the table—no Democrats or no Republicans voting in the negative direction. Any other case represents a weaker relationship.

Vote	Rep.	Dem.	Rep.	Dem.	Rep.	Dem.	Rep.	Dem.
Yes	.50	.30	.45	.35	.45	.30	.30	.20
No	.10	.10	.15	.05	.15	.10	.30	.20
Total	.60	.40	.60	.40	.60	.40	.60	.40
	(.50>.10 & .30>.10)		(.45+.05=.50=.35+.15)		(.45/.60=.75=.30/.40)		(.30=.30 & .20=.20)	
	Chart 3a		Chart 3b		Chart 3c		Chart 3d	
	Accord		Balance		Independence		Cleavage	
	$p_{11} \geq p_{21}$		$p_{11}+p_{22}=p_{12}+p_{21}$		$p_{11}p_{22}=p_{12}p_{21}$		$p_{11}=p_{21}$	
	and				$\left(\frac{p_{11}}{p_{\cdot 1}}=\frac{p_{12}}{p_{\cdot 2}}\right)$		and	
	$p_{12} \geq p_{22}$						$p_{12}=p_{22}$	
	Also null if:							
	$p_{21} \geq p_{11}$							
	and							
	$p_{22} \geq p_{12}$							

Chart 3. Null Relationship Conditions*

* Relations among these null conditions are described in the text.

were equivalent, so that all voters were either Protestant Republicans or Catholic Democrats, but there would be no tendency towards reinforcement if there were as many Protestant Republicans and Catholic Democrats as there were Protestant Democrats and Catholic Republicans.[9] In the dichotomous case, balance means the numbers of cases along the major and minor diagonals are equal. The relationship of party and issue position is balanced if Republicans voting yes and Democrats voting no constitute exactly half of the number of legislators. A balance between the number of Democratic liberals and Republican conservatives on the one hand and Democratic conservatives and Republican liberals on the other hand would indicate a null relationship, while an imbalance indicates the existence of a relationship.

(c) "Independence" exists when the percentage distributions across the dependent variable categories are the same for each independent variable category, as when equal proportions of Republicans and Democrats vote yes (Chart 3c). McGinnis has argued that statistical independence is the only legitimate interpretation of the lack of a relationship;[10] the present argument is that there can be legitimate substantive interest in other forms of a relationship.

(d) "Cleavage" occurs when all independent variable categories split evenly between the dependent variable categories. There is total cleavage if both Democrats and Republicans are evenly split between yes and no votes (Chart 3d). At least one party would have to show some degree of internal cohesion for there to be a relationship between party and vote.

These forms of a null relationship are interrelated. If the cleavage situation exists, there would be no relationship under any of these forms, so Chart 3d is a null relationship under all four definitions. Independence and balance are equivalent if equal numbers of cases fall in both independent variable categories. If balance occurs or if the variables are independent, then the majorities of the independent variable categories must be in accord, so Charts 3a–3d are all null under the accord rule. The fewest votes would depart from a null relationship under the accord definition, while the most votes would depart from it under the cleavage rule.

Relationship models. The above definitions of a perfect relationship and a null relationship can be combined to yield a family of different relationship models as shown in Chart 4. A few combinations are inconsistent because a particular set of data can simultaneously satisfy the maximum and null value conditions.[11] The remaining combina-

[9] The literature which argues that democratic stability depends on individuals having numerous cross-cutting group memberships is reviewed in Douglas W. Rae and Michael Taylor, *The Analysis of Political Cleavages* (New Haven: Yale University Press, 1970), chapter 4.

[10] Robert McGinnis, "Logical Status of the Concept of Association," *The Midwest Sociologist*, 20 (May, 1958), 72–77.

[11] Models 3a and 3b would regard Chart 2.3 as both a perfect and a null case. Model 2a would treat unanimous and near-unanimous votes as both perfect and null relationships. Balance can occur on unanimous (and near-unanimous) votes, which would make model 2b indeterminate. Maximum departure from cleavage does exist on unanimous votes, but

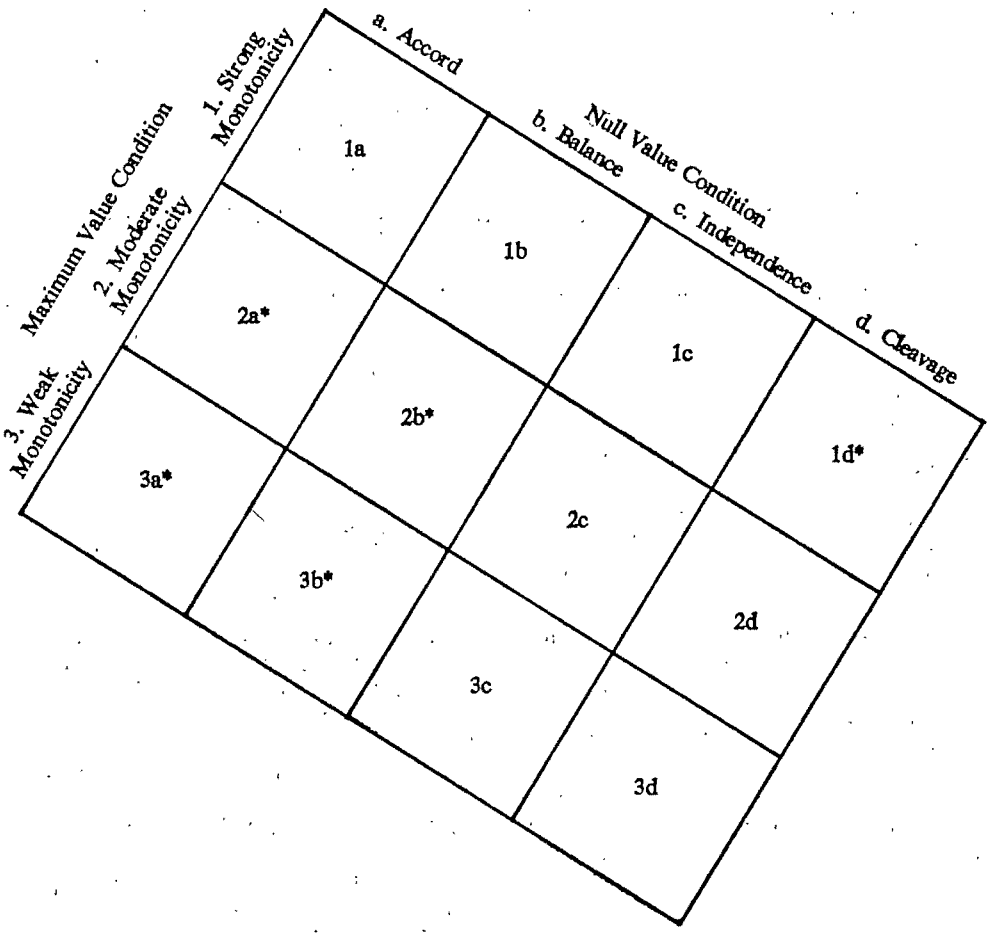


Chart 4. A System of Models of Statistical Relationship

* Inconsistent; see footnote 11.

tions—1a, 1b, 1c, 2c, 2d, 3c, and 3d—constitute the models employed by most common statistical measures. A variety of measures based on these models are listed in Chart 5.[12]

such a relationship would not fulfill the conditions for strong monotonicity; consequently model 1d cannot exist. As a result of these problems, measures cannot be based on models 1d, 2a, 2b, 3a, and 3b. Additionally, models 2c and 3c would regard all unanimous cases as both a perfect and a null relationship. Measures can be based on them only if they are undefined for strictly unanimous items.

[12] As noted in the chart, the computational formulas shown linearly transform the coefficient values to range from zero for a null relationship to one for a perfect relationship. The transformations are *not* always appropriate for nondichotomous variables, or for comparing relationships involving dichotomous variables with ones involving nondichotomous variables. Particularly model 1b and 2d measures might validly reflect the numbers of categories for the variables. Absolute values have been employed for measures of departure from statistical independence since signs are of limited meaning when dealing with dichotomous categoric variables.

Several measures are undefined for strictly unanimous items. A measure which does not approach zero as a variable becomes unanimous is regarded as satisfying the moderate (or, if appropriate, weak) monotonicity rule.

The opposition index is suggested by MacRae, *Issues and Parties in Legislative Voting*, p. 199. Koppa is a linear function of the usual simple agreement score. The cross-cutting measure is based on the work of Rae and Taylor, *The Analysis of Political Cleavages*. An unconventional formula is shown for Goodman and Kruskal's tau, based on an algebraic simplification of its formula to phi-squared for dichotomous variables. Theta is described in Robert C. Messenger and Lewis M. Mandell, "A Modal Search Technique for Predictive Nominal Scale Multivariate Analysis," *Journal of the American Statistical Association*, 67 (December, 1972), 768–772. The phi/phi-max formula differs from the usual version of that statistic in that it assumes the value of unity *whenever* there is a zero cell in the cross-tabulation. The cosine-pi approximation for tetrachoric *r* is employed. The backward regression

Model	Measure	Formula
1a	Lambda, symmetric	$(\text{Min}(p_{1.}, p_{.1}) - \text{Min}(p_{11}, p_{12}) - \text{Min}(p_{21}, p_{22}) + \text{Min}(p_{.1}, p_{.2}) - \text{Min}(p_{11}, p_{21}) - \text{Min}(p_{12}, p_{22}))/(\text{Min}(p_{1.}, p_{.1}) + \text{Min}(p_{.1}, p_{.2}))$
1a	Lambda, asymmetric	$(\text{Min}(p_{1.}, p_{.1}) - \text{Min}(p_{11}, p_{21}) - \text{Min}(p_{12}, p_{22}))/\text{Min}(p_{1.}, p_{.1})$
1a	Opposition index	$(p_{11} - p_{21})(p_{22} - p_{12})/p_{.1}p_{.2}$
1b	Koppa*	$\lvert p_{11} + p_{22} - p_{12} - p_{21} \rvert$
1b	Cross-cutting*†	$1 - 4(p_{11} + p_{22})(p_{12} + p_{21})$
1c	Phi* = Kendall's tau-b = Pearson's r	$\lvert p_{11}p_{22} - p_{12}p_{21} \rvert / \sqrt{p_{.1}p_{.2}p_{1.}p_{2.}}$
1c	Goodman and Kruskal's tau*	$(p_{11}p_{22} - p_{12}p_{21})^2 / p_{.1}p_{.2}p_{1.}p_{2.}$
1c	Uncertainty, symmetric†	$2 + 2(p_{11} \log_2 p_{11} + p_{12} \log_2 p_{12} + p_{21} \log_2 p_{21} + p_{22} \log_2 p_{22}) / (p_{1.} \log_2 p_{1.} + p_{2.} \log_2 p_{2.} + p_{.1} \log_2 p_{.1} + p_{.2} \log_2 p_{.2})$
1c	b_{yx}* = Somer's d_{yx}	$\lvert p_{11}p_{22} - p_{12}p_{21} \rvert / p_{.1}p_{.2}$
2c	b_{xy} = Somer's d_{xy}	$\lvert p_{11}p_{22} - p_{12}p_{21} \rvert / p_{1.}p_{2.}$
2c	Uncertainty$_{y/x}$	$1 + (p_{11} \log_2 (p_{11}/p_{.1}) + p_{12} \log_2 (p_{12}/p_{.2}) + p_{21} \log_2 (p_{21}/p_{.1}) + p_{22} \log_2 (p_{22}/p_{.2}))/(p_{1.} \log_2 p_{1.} + p_{2.} \log_2 p_{2.})$
2d	Theta†*	$2\,\text{Max}(p_{11}, p_{21}) + 2\,\text{Max}(p_{12}, p_{22}) - 1$
2d	Average cohesion	$(\lvert p_{11} - p_{21} \rvert / p_{.1} + \lvert p_{12} - p_{22} \rvert / p_{.2})/2$
3c	Phi/phi max*	$\lvert p_{11}p_{22} - p_{12}p_{21} \rvert / p_{.1}p_{1.}$ if $p_{11}p_{22} \geq p_{12}p_{21}$ and $p_{22} \geq p_{11}$ $\lvert p_{11}p_{22} - p_{12}p_{21} \rvert / p_{.2}p_{1.}$ if $p_{11}p_{22} \geq p_{12}p_{21}$ and $p_{22} < p_{11}$ $\lvert p_{11}p_{22} - p_{12}p_{21} \rvert / p_{.2}p_{2.}$ if $p_{11}p_{22} < p_{12}p_{21}$ and $p_{21} \geq p_{12}$ $\lvert p_{11}p_{22} - p_{12}p_{21} \rvert / p_{.1}p_{2.}$ if $p_{11}p_{22} < p_{12}p_{21}$ and $p_{21} < p_{12}$
3c	Yule's Y*	$\lvert \sqrt{p_{11}p_{22}} - \sqrt{p_{12}p_{21}} \rvert / (\sqrt{p_{11}p_{22}} + \sqrt{p_{12}p_{21}})$
3c	Tetrachoric r*	$\sin[(\pi/2) \lvert \sqrt{p_{11}p_{22}} - \sqrt{p_{12}p_{21}} \rvert / (\sqrt{p_{11}p_{22}} + \sqrt{p_{12}p_{21}})]$
3c	Yule's Q* = Goodman and Kruskal's gamma	$\lvert p_{11}p_{22} - p_{12}p_{21} \rvert / (p_{11}p_{22} + p_{12}p_{21})$
3d	Maximum cohesion	$\text{Max}(\lvert p_{11} - p_{21} \rvert / p_{.1}, \lvert p_{12} - p_{22} \rvert / p_{.2})$

Signs have been deleted for dichotomous nominal variable relationships.
Range has been modified to go from 0 to 1 for dichotomous variables.
Direction has been reversed so that 0 corresponds to null relationship.

Chart 5. Formulas for Relationship Measures

The several models have been ordered in Chart roughly in terms of their "leniency." Model 1a, the top of the chart, is most restrictive in its inition of a perfect relationship while treating greatest number of cases as lacking a relation-p. Model 3d, at the bottom of the chart, is the st lenient in its assessment of a perfect relation-p and codes the fewest situations as lacking a ationship. There is only a partial order on the ermediate models, but generally the models ir the top of the chart are less lenient than those ir the bottom.

mplicit in the categorization of models is that dels near one another in Chart 4 tend to be ilar to each other. Models sharing a common ximum (or null) value condition (and measures ed on those models) should tend to behave ilarly. The cumulation involved in the defini-ns for maximum and null value conditions also arantees some similarity among all the models, ough different models can assess the same ariate distribution in very different ways. In t, a distribution considered a perfect relation-ship by one model can sometimes be treated as null by another. For example, models 3c and 3d are maximal for the data in Chart 2.3, while models 1a and 1b are null.[12]

Two models are particularly different from the others in what they measure. Only under the cleavage rule can a relationship exist without differences in the behavior of the independent variable categories. Thus models 2d and 3d do not imply any contrasting of the groups' (parties') behavior. Relationship statistics based on these models do not measure differences.

These several themes can be illustrated by analyzing the behavior of the measures in a real data situation. The measures of Figure 5 have been calculated for the relationship between party and vote over a series of roll-call votes taken in the United States House of Representatives. To obtain differences in independent variable marginals, votes from the 83rd Congress (1953–54) in which the parties were nearly equal in size have been used together with votes from 1965 when the

fficient, b_{yx}, is included in Chart 5 since its maxi-m and null value conditions are among those ited in the text. Other measures are standard istical coefficients, simple transformations of such fficients, or totally ad hoc measures.

[12] An a priori ordering of the severity of null conditions b and c is not evident, but the ordering in the text has been chosen because condition b behaves like condition a in this instance (and in the cases mentioned in footnote 11) while condition c behaves like condition d.

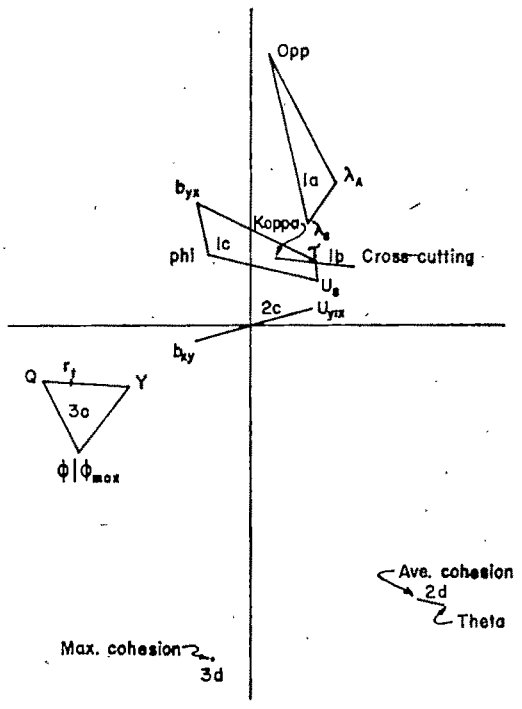


Figure 1. Factor Space for Measures Calculated on Nonunanimous Votes

Democrats had more than a two-thirds majority. There were 347 roll calls in these years, including 39 totally unanimous votes on which coefficients undefined for unanimous variables could not be computed. Variables lacking variance are of limited interest, so the basic analysis employs only the 266 votes in which 90 per cent of the two parties were not in agreement, a common definition of nonunanimity in legislative studies.

Pearson's *r* values have been calculated between all pairs of the several measures.[14] The correlations are all positive; the lowest correlation between coefficients based on different models is .13 with nearly all correlations being above .50. The lower left part of Table 1 reproduces the correlations among the better-known statistics. (The correlations between measures based on the same model are described in a later section.)

A principal component analysis reveals that a single component accounts for 80 per cent of the total variance of the 18 measures. Only for the coefficients based on cleavage models (2d and 3d) does the first component account for less than half of the measure's total variance (and is the second component about as important as the first). With the exception of those two models, the different statistics do tap the same basic notion (though somewhat different aspects) of a relationship. Figure 1 displays the second and third dimensions of the component solution.[15] The cleavage model (2d and 3d) measures are separate from the others in this figure and particularly distant from the accord model (1a) ones, so there is a vivid contrast between the models at the top and bottom of Chart 4.[16]

Overall, measures based on different models do behave differently, but the differences are not always great, and the conditions governing them must be carefully specified.[17] Models employing

[14] The use of Pearson's *r* here is debatable, but it is consistent with the usual practice of treating party voting measures at the interval level (as by taking their mean over a series of votes). The Pearson's *r* values should be considered conservative since they measure only linear rather than monotonic relationships; for example, the Yule's *Q* and Yule's *Y* values have a correlation of "only" .94 with one another while in fact their relationship is perfectly monotonic.

The correlational approach employed in this section would not be possible in legislatures where the relationship between party and vote is identical for all votes, in which case there would be no variance in the relationship statistics. Fortunately for the analysis, the relationship between party and vote varies considerably among different votes in the American Congress. The standard deviations of the measures could range from .00 to .50 (except for the opposition index with a maximum of 1.00 since it has been allowed to take on negative values); the actual values range from .19 to .30 (with the exception of the opposition index with a standard deviation of .38).

[15] Variation on the first component is slight (all values around .65); its effect can be visualized in Figure 1 by curling the page so the bottom three points and the top point are slightly below the others.

[16] Sharp differences among the models can also emerge with respect to unanimous and near-unanimous variables. If the dependent variable is strictly unanimous when indepedent variable categories are of equal sizes, null conditions *a-c* would be satisfied while models 2d and 3d would be maximal. If there is a single dissenting vote with equal-sized independent variable categories, null condition *a* would be satisfied, models 1b and 1c would be near null, model 2c would be moderate, model 2d would be near maximal, and weak monotonicity (maximum condition 3) would be achieved. Thus models 3d, 3c, 2d and to some extent 2c are more distinct from the other models in the presence of unanimous and near-unanimous variables.

The analysis in the text was replicated with unanimous and near-unanimous roll calls retained. The correlations between the different measures (upper right of Table 1) remain positive, except for some involving the cleavage model (2d and 3d) statistics but the correlations involving model 3c and even moreso models 2d and 3d fall. The first principal component is strong, but the model 3d and especially 2d coefficients are virtually independent of it. The model 3c measures and, to a less extent, b_{xy} (a model 2c statistic) are more distinct from the remaining measures in the component solution.

[17] Measures based on different models should behave differently, at least to the extent to which their underlying models differ. This maxim specifically contradicts the view that coefficients should be stable when the variable categories are collapsed, since collapsing categories may validly alter the degree of fit with some models. Furthermore statistics should

Table 1. Representative Correlations between Measures of Party Voting Calculated on House Roll Calls, 1953–54 and 1965[a]

Model	Measure	Model 1a	1b	1c	2c	3c	2d	3d
1a	lambda-asymmetric	—	.91	.93	.87	.59	.16	.14
1b	cross-cutting	.91	—	.89	.96	.63	.14	.19
1c	$b_{y/x}$	.94	.92	—	.88	.69	−.22	−.04
2c	uncertainty$_{y/x}$	.87	.97	.91	—	.71	.34	.42
3c	Yule's Q	.67	.71	.83	.75	—	.24	.46
2d	theta	.50	.67	.46	.66	.50	—	.84
3d	maximum cohesion	.32	.51	.45	.61	.79	.66	—

[a] Correlations above the diagonal are based on all votes; those below the diagonal exclude unanimous and near-unanimous votes.

the "cleavage" interpretation of a null relationship differ most sharply from the others. Among the remaining models, models 1a and 3c are most different from one another, but their considerable covariation should not be overlooked. Finally models 1b, 1c, and 2c are moderate, and measures based on them are quite similar in their behavior.

Model selection. The different models correspond to different substantive theories of a relationship, so the analyst can choose a substantive model and then use a measure embodying that model. The choice of a substantive model will be illustrated in this section by interpreting the various models in the party voting context.

Model 1a. This is the most rigorous model, utilizing the most stringent definition of a perfect relationship along with the null relationship definition which codes the most relationships as null. This model classifies all instances of party agreement as null relationships, while it accepts only strong monotonicity as a perfect relationship. Those ideologues believing that party agreement proves the party system is a sham because the bourgeoisie and proletariat have opposite objectives would employ this model.

Model 1b. This model differs from model 1a in accepting instances of party accord as cases of a relationship so long as the party and vote cleavages are reinforcing. It is a model based on the balance between issue positions that are consistent and inconsistent with party divisions. For example, a balance between the number of Democratic doves and Republican hawks on the one hand and Democratic hawks and Republican doves on the other hand would indicate a null relationship; a relationship would exist if the Democratic doves and Republican hawks predominate (or if the Democratic hawks and Republican doves predominate).

Model 1c. This model differs from the previous ones in viewing only statistical independence as the lack of a relationship. Party would be related to vote so long as members of one party vote yes in greater proportions than those of the other party; complete party voting would require the total opposition of the two parties.

Model 2c. This model differs from model 1c only in accepting a unanimous vote as a perfect relationship, since such votes can be perfectly predicted from party.

Model 2d. Like model 2c, this model accepts a unanimous vote as involving perfect prediction from party, but it regards as a null case only those instances in which both parties split evenly on the vote. The more cohesive the parties, the more successful is the prediction that members vote with their party's majority. This model sums up the predictability of the vote for each party, while avoiding contrasts of the behavior of the parties. It tests how cohesive the parties are, not whether one is more cohesive than the other.

Model 3c. Like model 2c, this model regards statistical independence as the null value condition. It accepts, however, a single cohesive party as the definition of a perfect relationship. It is a weak ideological model in which a perfect relationship holds so long as proponents of a motion can identify one party as their friend (because it unanimously favored the motion) or their enemy

not be judged according to whether they are sensitive to the Pearson's r correlation "underlying" the cross-tabulation because they may be purposefully based on relationship models different from that of Pearson's r. An interesting empirical analysis of the behavior of several measures is given by Brent M. Rutherford, "The Accuracy, Robustness and Relationships Among Correlational Models for Social Analysis: A Monte Carlo Simulation," (paper delivered at the Annual Meeting of the American Political Science Association, Washington, D.C., September 5–9, 1972). A useful discussion of intuitive notions of a relationship and of the factors underlying differences among measures is provided by A. A. Hunter, "On the Validity of Measures of Association," *American Journal of Sociology*, 79 (July, 1973), 99–109. However, neither author's interpretation of the results acknowledges sufficiently the meaningfulness of differences in the behavior of measures.

(because it unanimously opposed the motion). This model is particularly appropriate when the line separating the independent variable categories is questionable. If, for example, the actual voting division is between southern Democrats and all Republicans on one side and northern Democrats on the other side, the model 3c measures calculated between official party and vote would be maximal.

Model 3d. This is the most lenient model. A single cohesive party is accepted as perfect party voting while any degree of cohesion for either party is viewed as indicating some party voting. Only the most cohesive party's behavior is relevant for this model since the model concentrates on how united is the most cohesive party.

I would regard models 1a, 1c, and 3c as the main models for legislative voting analysis. Model 1c is the classical statistical model and the traditional legislative analysis model. Models 1a and 3c are intriguing alternative models with clear underlying ideological bases. Measures based on models 1b, 2c, 2d, and 3d are excellent illustrations of probabilisitic theoretic predictability and ad hoc measures, but they represent interpretations of a relationship which seem to be of less interest in the present case.

Different substantive situations may call for other relationship models. Also the various relationship models can have special meanings in situations where the distinction between independent and dependent variables is inappropriate. For example, MacRae has demonstrated that model 3c measures are well-suited for tapping cumulative relationships between variables, the relationships which form the basis for Guttman scaling.[18] When measuring agreement among individuals by simply counting up the number of times a pair of individuals gives the same response to a set of items, the resultant simple agreement score is based on model 1b (actually it is a linear transformation of koppa). Students of taxonomy have shown that model 1c coefficients correct such agreement statistics for the amount of agreement expected on the basis of statistical independence, while model 3c measures concentrate on cumulative differences between the individuals or objects being classified.[19]

Chart 6 re-examines the models of Chart 4 from the perspective of what each emphasizes in its measurement. Thus high values for model 1a statistics signify opposite behavior for independent variable categories. Model 1b measures calculated between a pair of individuals are high when there is agreement between that pair of individuals on most items to which they are responding. Model 1c coefficients are large when the independent variable categories are polarized on the dependent variable (which requires equal numbers of rows and columns for complete polarization), while model 2c coefficients are large when the variables are statistically associated with one another regardless of unequal numbers of variable categories. Predictability is tapped by model 2d measures, cumulation by model 3c measures, and cohesion of at least one of the independent variable categories by model 3d statistics.

Measure selection. The choice of a measure of association need be made only after the relationship model has been selected. At that time, however, it is necessary to choose among the measures based on the relevant model. This choice problem leads us to ask a series of questions: In what ways do measures based on the same model differ from one another? How should one choose between measures based on the same model, and how important is that choice?

First, there can be magnitude differences between measures based on the same model. The operations involved in the construction of some measures cause them to have lower values than other statistics. The most neutral standard for comparing coefficient values is the case with 50 per cent in each marginal category of both variables. Figure 2 shows the statistics' values and computing formulas for this case. Curves above the diagonal represent "high-valued" statistics, while those below the diagonal correspond to "low-valued" coefficients. Measures which tend to assume high or low values may have well developed operational interpretations, but they are limited in their power to discriminate between certain sized relationships. A high-valued statistic, for example, provides little differentiation between separate cases with large relationships.

In addition to the magnitude of the coefficient, there are a variety of strategies for a choice between statistics based on a single model. Reasonable considerations in choosing between statistics include how pleasing are their operational interpretations, how simple they are to compute, how well known they are, how readily they generalize to nondichotomous variables, and what measurement level they employ for nondichotomous data.

The selection can also be based on whether "sensitivity" to the choice of independent variable and/or to the independent variable's marginals are desired in the substantive setting. An asymmetric statistic takes on different values depending on which is the independent variable (which may be appropriate, for example, when measuring how

[18] MacRae, *Issues and Parties in Legislative Voting*, chapters 3 and 5.

[19] Robert R. Sokal and Peter H. A. Sneath, *Principles of Numerical Taxonomy* (San Francisco: Freeman, 1963).

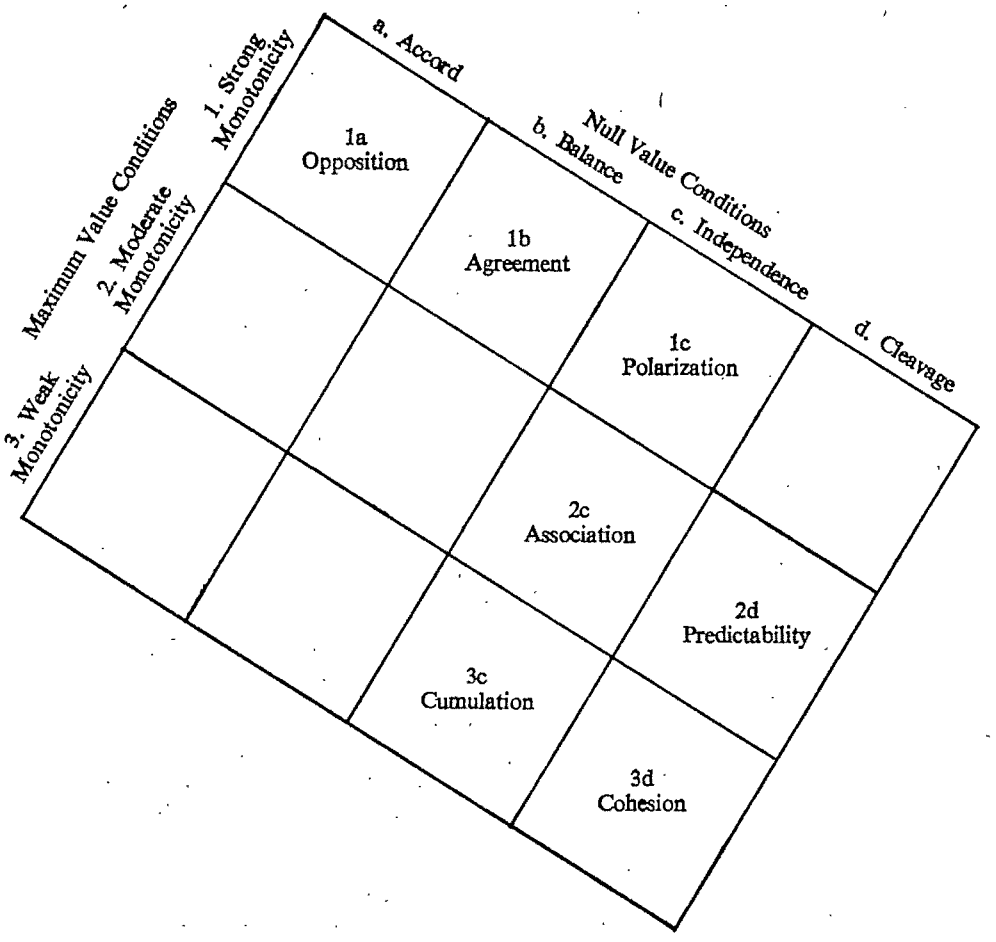


Chart 6. Models of Statistical Relationship

vell the dependent variable can be predicted from he independent one), while a symmetric coefficient is not sensitive to which variable is labeled independent." Some coefficients are also sensitive to the independent variable marginals. Consider two legislative votes, 60 per cent of Republicans voting yes on both compared to only 40 per ent of Democrats. Say these votes were taken in ifferent years with the distribution of party trength having changed in the intervening period. measure "insensitive" to the marginals would egard the extent of party voting on these two otes as identical, while a coefficient sensitive to he marginals might have different values for hese two votes.[20] The sensitivities of the measures re summarized in Chart 7.

The correlations between measures based on the same model provide a useful perspective on these many criteria. Over the set of roll calls employed for the analysis of Figure 1, the within-model correlations are uniformly very high[21]—between .89 and .99—regardless of whether the coefficients differ in their operational interpretations, symmetry, or sensitivity to variable margi-

[20] Maximum value condition 3 was described as roviding an adjustment for unequal marginals. That nvolves a contrast of independent variables and de-endent variable marginals, whereas the sensitivity escribed here is a further sensitivity to changes in he independent variable marginals.

[21] The sole exception involves the opposition index when low variance votes are retained in the analysis. This measure differs from the others in the employing negative values to assess varying degrees of party accord (its null value condition). Low-variance votes would be marked by strong party accord, so this index differs from other model la measures for such votes, which results in somewhat lower correlations.

Note that raising the measures (except the opposition index) to a positive power yields another statistic having the same maximum and null value conditions and therefore the same relationship model. Raising to a power provides a statistic whose intermediate values "look" different from those of the original measure, but they would covary perfectly according to Spearman's rho correlation of rank orders. The measures would differ meaningfully only in their magnitudes.

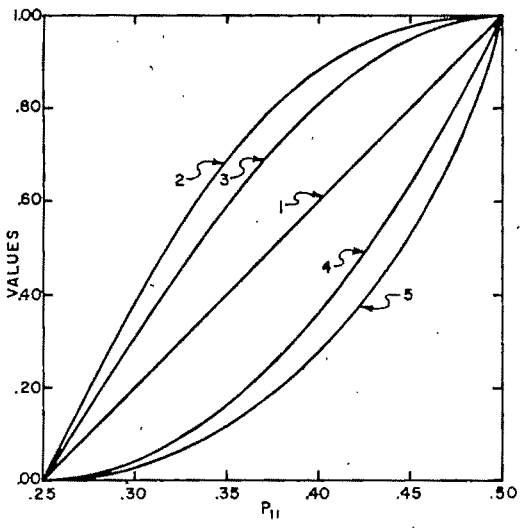


Figure 2. Measure Values as a Function of Cell Size under Equal Marginal Conditions ($p_{1.} = p_{2.} = p_{.1} = p_{.2} = .50$)

Curve 1: lambda-symmetric = lambda-asymmetric = koppa = phi = b_{yx} = b_{xy} = theta = average cohesion = phi/phi max = Yule's Y = maximum cohesion = $4p_{11} - 1$ if $p_{11} \geq \frac{1}{4}$, $1 - 4p_{11}$ otherwise.

Curve 2: Yule's $Q = (2Y)/(1 + Y^2)$ where Y is given by curve 1.

Curve 3: Tetrachoric $r = \sin(.5\pi Y)$ where Y is given by curve 1.

Curve 4: opposition index = cross-cutting = tau_{GK} = phi[2] where phi is given by curve 1.

Curve 5: uncertainty-asymmetric = uncertainty-symmetric =
$2p_{11} \log_2(p_{11}) + (1 - 2p_{11}) \log_2(.5 - p_{11})$ if $p_{11} \geq \frac{1}{4}$
$2p_{12} \log_2(p_{12}) + (1 - 2p_{12}) \log_2(.5 - p_{12})$ otherwise.

nals.[22] These high correlations were reflected in Figure 1 by the small distances among measures based on the same model. The choice among measures with such great covariation is not critical, so it can well be based on convenience.

Measures based on the same model covary, so their magnitude is the most important property distinguishing them. Minimally, the analyst should bear in mind whether the statistic being employed is high-, moderate-, or low-valued. Certain forms of analysis, such as factor analysis, will be directly affected by tendencies of the coefficients to take on high or low values. Moderate-valued statistics are desirable in that they are of the same order of magnitude as the familiar Pearson's r. When values of different measures are to be compared, it makes particular sense to employ moderate-valued coefficients so that the comparisons are not confounded by idiosyncracies of the measures themselves.

This section has considered the problem of choosing an association measure given the choice of a relationship measure. Preceding sections have emphasized the extent to which different models may be appropriate in different situations. The remainder of this paper demonstrates that more than one model can be of interest in a single substantive situation.

The Use of Multiple Models

While it is possible to choose a single relationship model on the basis of substantive considerations, it often will be useful to employ simultaneously several measures based on alternative models of a relationship. Multiple measures can be used on a given set of data to study different aspects of a relationship. Rather than postulate a single party-voting model in legislatures, for example, one could determine which of several possible models best describe the voting.

The use of multiple measures raises a new problem: How are differences in values of two coefficients to be explained? The differences can be due to the use of different relationship models, but they also can be due to the idiosyncracies of particular measures. Some coefficients are always larger than others, so an appreciation of magnitude characteristics of coefficients is required for the use of multiple models to become practical.

The previous section distinguished between "high-valued," "moderate-valued," and "low-valued" coefficients. When multiple models are used, employing only moderate-valued statistics holds constant some extraneous differences between measures. There are order relationships among these moderate-valued measures which are also important to bear in mind. Chart 8 displays the four-tiered structure of magnitude differences among moderate-valued statistics. The lambda statistics are never larger than any of the others. No model 1b, 1c, or 2c measure is ever larger than any model 2d or 3c statistic. None of these coefficients can be larger than the maximum cohesion score.[23]

This magnitude information should be con-

[22] These results seem most liable to being dependent on the empirical data being analyzed. Greater variation in the independent variable marginals may have an effect, but the effects are unlikely to be pronounced unless the marginal split betwen parties is 80–20 or 90–10.

[23] The relationships of Chart 8 were originally detected by inspection of the party voting measures for 1953–54 and 1965. Proofs have subsequently been obtained for all of the inequalities shown, while the lack of a consistent ordering with the roll-call data is sufficient to demonstrate the absence of an order relationship.

(i) Measure	(ii) Model	(iii) Asymmetry	(iv) Marginals	(v) Values
Lambda, symmetric	1a	symmetric	sensitive	moderate
Lambda, asymmetric	1a	asymmetric	sensitive	moderate
Opposition index	1a	asymmetric	insensitive	low
Koppa	1b	symmetric	sensitive	moderate
Cross-cutting	1b	symmetric	sensitive	low
Phi	1c	symmetric	sensitive	moderate
Goodman and Kruskal's tau	1c	symmetric	sensitive	low
Uncertainty, symmetric	1c	symmetric	sensitive	low
b_{ys}	1c	asymmetric	insensitive	moderate
b_{xy}	2c	asymmetric	sensitive	moderate
Uncertainty$_{y/s}$	2c	asymmetric	sensitive	low
Theta	2d	asymmetric	sensitive	moderate
Average cohesion	2d	asymmetric	insensitive	moderate
Phi/phi max	3c	symmetric	sensitive	moderate
Yule's Y	3c	symmetric	insensitive	moderate
Tetrachoric r	3c	symmetric	insensitive	high
Yule's Q	3c	symmetric	insensitive	high
Maximum cohesion	3d	asymmetric	insensitive	moderate

Chart 7. Formal Properties of Measures

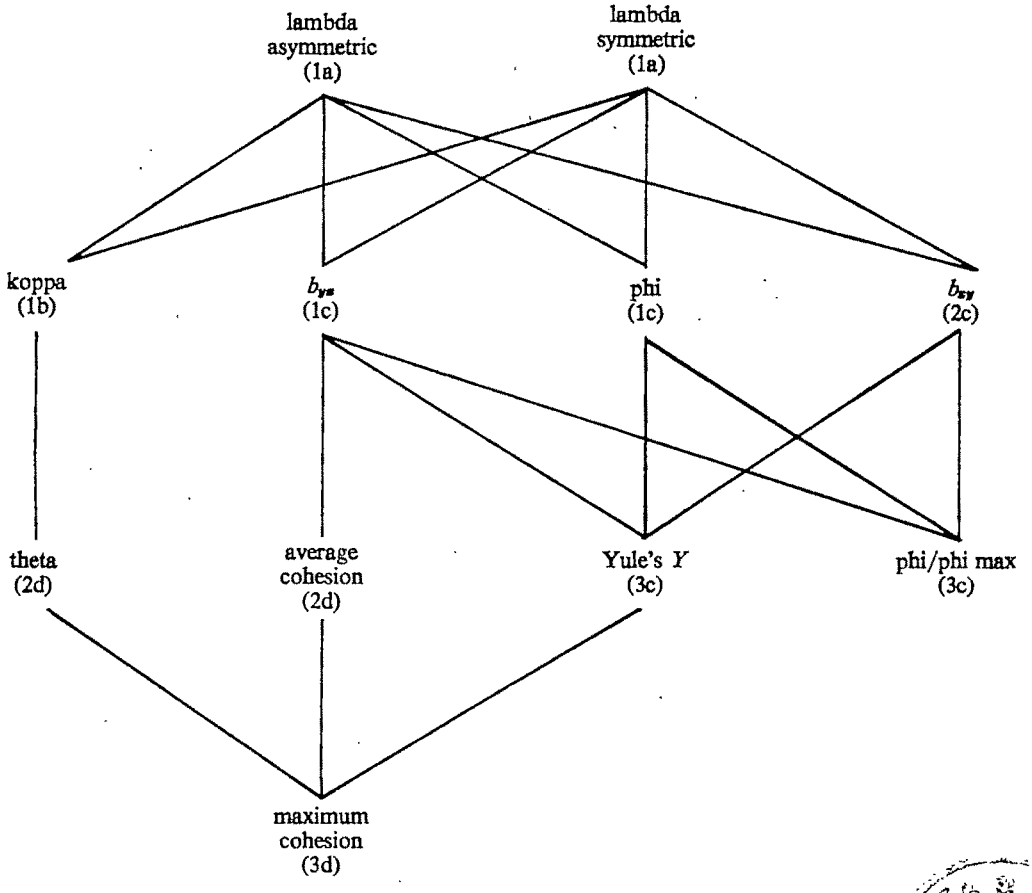


Chart 8. Order Relationships among Moderate-valued Statistical Measures[a]

[a] A connecting line indicates that the top coefficient is less than or equal to the bottom one.

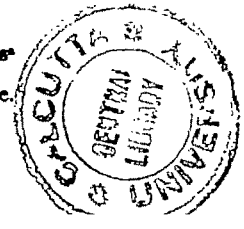


Table 2. Mean Values for Measures of Party Voting in House, 1953–54 and 1965

Measure	Model	Values*	Means without Near-Unanimous and Unanimous Votes	Means with All Votes
Opposition index	1a	low	.127	−.125
Uncertainty, symmetric	1c	low	.235	.181
Uncertainty$_{y/x}$	2c	low	.239	.216
Goodman and Kruskal's tau	1c	low	.272	.237
Cross-cutting	1b	low	.279	.230
Lambda, asymmetric	1a	moderate	.308	.266
Lambda, symmetric	1a	moderate	.337	.260
b_{yx}	1c	moderate	.455	.352
Phi	1c	moderate	.467	.414
Koppa	1b	moderate	.475	.415
b_{xy}	2c	moderate	.486	.465
Yule's Y	3c	moderate	.541	.537
Average cohesion	2d	moderate	.578	.671
Theta	2d	moderate	.580	.672
Phi/phi max	3c	moderate	.632	.626
Tetrachoric r	3c	high	.695	.679
Maximum cohesion	3d	moderate	.750	.805
Yule's Q	3c	high	.757	.737

* See Figure 2: curve 1 = moderate, curves 2 and 3 = high, curves 4 and 5 = low.

sidered when using multiple models. Say, for example, that phi (model 1c) is .49, while Yule's Y (model 3c) is .50 and Yule's Q (model 3c) is .80. It would be fallacious to compare the phi and Q values since Q is higher-valued than phi. The Y value of .5 can be legitimately compared to the phi, and making this comparison places the Y in perspective. A Y of .5 might ordinarily suggest that a model 3c relationship exists, but in this case the Y value is on par with the phi. The more restrictive model 1c suffices for description of the relationship. The relationship is effectively summarized by the strong monotonicity model; there is no additional tendency for weak monotonicity.

Party voting in legislatures. The use of multiple relationship models will be illustrated by means of three political examples. The first example returns to the roll-call data previously described. Is there party voting in the American Congress and, if so, of what type? Do the parties disagree with one another? Are there differences in the voting of the parties? Is the predominant conflict one between northern Democrats and a conservative coalition? Multiple measures must be used if one is to test the existence of various forms of party voting.

The mean values of the several coefficients over the roll calls, as reported in Table 2, can be examined to determine the extent of different forms of party voting in the House of Representatives.[24] The support for the hypothesis of party voting under restrictive model 1a is not great, but party opposition sometimes occurs. There is also evidence of party voting as tapped by the traditional model 1c (and the related model 1b). The additional support for the predictability model 2d and for the cumulation model 3c may be considered slight given their leniency. If the House were ruled by a conservative coalition of southern Democrats and a united Republican party, the moderate monotonicity (model 3c and 3d) measures would be at unity; instead significant divisions must exist within both parties. Predictability from party is far from complete even if independence is not used as the definition of predictability (model 2d).

Evidence of party voting in the House seems greater than some have charged and less than others have feared. The usual analysis of party voting using model 1c would find a moderate degree of party responsibility. The present analysis specifies that this responsibility often involves the opposition of the parties but not necessarily the total cohesion of either party. Simultaneous use of

[24] Considering only the moderate-valued measures, the restrictive model 1a statistics have the lowest means, the model 1b, 1c, and 2c coefficients have lower means than the model 3c and 2d measures, and the lenient model 3d statistic has the highest mean—an ordering of means consistent with both Charts 4 and 8. The differences in mean values in Table 2 are fairly sharp; the value of a coefficient is related to its underlying model. The means for most coefficients drop when unanimous and near-unanimous votes are included, except that the model 2d and 3d measures are higher in this instance (and the model 2c and 3c statistics fall by less than .03 with these data), since such votes are extremely predictable and cohesion is necessarily high.

multiple definitions of relationship permits more extensive analysis than the customary restriction to a single statistic.[25]

Group voting in electorates. As another example of this approach we shall consider group voting in a mass electorate. The party for which a person votes is the dependent variable for this case, while the person's group membership is the independent variable. Voting differences between classes, religions, regions, and races have been studied in several countries. The regression slope (that is, the simple percentage difference) is the measure most commonly used, with the closely related phi-coefficient also being employed.[26] The potential use of different and multiple measures to explore group voting more fully has not been exploited.

What is group voting? Does it require the groups to vote for different parties? Is it completely reflected in the difference in support for a party by two groups? Must one group be cohesive for there to be group voting? Each of these possibilities suggests a type of group voting, and we can determine which types hold for particular groups. An exhaustive review of the work on group voting would be beyond the scope of this paper, but we shall focus on a few suggestive cases. Table 3 presents the relevant data.

In Canada, the majorities of the manual and nonmanual workers did not intend to vote in opposite directions in 1960. In fact the difference in their proportions supporting the left parties is barely noticeable. Neither class was particularly cohesive in its party support. Class voting is slight in Canada under any relationship model.

By contrast, class differences are obvious in Britain. The majorities of middle- and working-class identifiers do support different parties. Defection from class occurs, but contrasting the model 1c and 3c measures does not indicate particularly greater defection in one class. Class voting in Britain is adequately described by the traditional model 1c measures.

The more lenient measures have higher values if one group is virtually united or if most of a party's support comes from a single group. Thus the 62 per cent difference in support of the Democratic presidential nominee between blacks and whites in 1968 does not adequately stress the bloc voting by blacks which is evident in the model 3d statistic. Similarly the 53 per cent difference in voting behavior of Norwegian farmers and non-

[25] Separate analysis of the two Congresses would yield similar results. The differences in measure means between the two are minimal. The largest difference is .09 for the symmetric lambda; others are .04 or less. There is no trend for higher values in one Congress than the other, though the measures' variances are consistently higher in the 1953–54 Congress in which party sizes were virtually equal.

[26] In particular, the tau-*b* used to measure "status polarization" in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960) is identical to phi since the variable are dichotomous. Rae and Taylor, *The Analysis of Political Cleavages*, also suggest the applicability of their cross-cutting measure for studying group voting. See also Walter Korpi, "Some Problems in the Measurement of Class Voting," *American Journal of Sociology*, 78 (November, 1972), 627–642.

Table 3. Examples of Group Voting Statistics

		(1a) Lambda	(1c) b_{yx}	(2d) Average Cohesion	(3c) Y	(3d) Maximum Cohesion
Canada 1960	manual workers: 60% left parties non-manual class: 53% left parties	.00	.07	.13	.07	.20
Britain 1963	working class identifiers: 72% Labour middle class identifiers: 21% Labour	>0[a]	.51	.51	.51	.58
U.S. 1968	blacks: 97% Humphrey whites: 35% Humphrey	>0[a]	.62	.62	.77	.94
Norway 1965	farmers & foresters: 60% Agrarian party others: 7% Agrarian party	.16	.53	.53	.65	.86

[a] Insufficient information is available to calculate lambda.

Sources:

Canada: Robert R. Alford, *Party and Society* (Chicago: Rand McNally, 1963), p. 354.

Britain: Donald E. Stokes and David Butler, *Political Change in Britain* (New York: St. Martin's Press, 1969), p. 76.

U.S.: Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics," *American Political Science Review*, 63 (December, 1969), 1085.

Norway: Philip E. Converse and Henry Valen, "Cleavage and Perceived Party Distances in Norwegian Voting," *Scandinavian Political Studies*, vol. 6, 1971, p. 115.

farmers does not emphasize the Agrarian party's lack of appeal outside of its occupational core which the model 3d measure captures.

These examples illustrate how the different models provide alternate perspectives on group voting. As surveys of additional countries become available, it would be fascinating to apply the full set of models in a systematic attempt to classify different forms of group voting.

Generational change in attitudes. As a final example of the considerations discussed in this paper, it is worth investigating the special situation involving time processes. Change can be measured in many ways—i.e. by estimating change parameters, gross change, net change, unidirectionality of change, and the dependence of later values on initial states. Different relationship models emphasize separate aspects of change.

Converse and DuPeux have studied generational change in politicization. In 1958 they interviewed samples of the public in France and the United States. Respondents were asked about their own and their father's partisanship (assuming the respondents knew their father and the father was a citizen eligible to vote).[27] To facilitate the discussion, the substantive example will be simplified so we can speak of whether the father's politicization level (whether or not he had a partisan affiliation) affected the respondent's. Table 4 reproduces the relevant information and gives the values for several coefficients.

Converse and DuPeux emphasize the b_{yx} percentage difference (model 1c, asymmetric) similarity in France and the United States in pointing out the common change process. The effect of higher parental politicization on respondent's politicization level is identical in the two countries, but the full range of statistics provides added insights. An interesting null finding is how little the predictor affects the dependent variable, whether in terms of the respondents moving to the high politicization level regardless of their father's politicization (lambda; model 1a) or little variance being explained (phi-squared; model 1c, symmetric). Yet the gross stability is overwhelming in the United States where 78 per cent of respondents have the same politicization level as their father (which translates to a .56 koppa; model 1b) though less noticeable in France where only 60 per cent have the same politicization level (koppa =.20). The unidirectionality of change is not striking in either country—the model 3c measure would be unity if the change were unidirectional (say from low to high but not vice versa); it is, however, actually no higher than model 1c sta tistics. The model 2d measures indicate som unity of the time 1 groups at time 2, but note tha these measures are actually equivalent in value t comparable model 1b and 1c measures (the ≈koppa and average cohesion≈b_{yx}). There definite cohesion of at least one of the t_1 group (model 3d), with the maximum cohesion score i France being much larger than any of the othe coefficients.

The Converse-DuPeux emphasis on b_{yx} is con sistent with (and one of the best political exam ples of) Blalock's advice to use unstandardize coefficients when seeking the parameters of socia science laws.[28] The actual change process, how ever, is more complex than b_{yx} suggests becaus of the low dependence of t_2 states on t_1 states an the definite change in both directions. The gros stability actually varies considerably between th two countries, though the percentage difference do not detect this divergence. As this exampl illustrates, there are many aspects of a time pro cess; stability and change have several separat dimensions, and the use of multiple models per mits an examination of the different dimensions.

Conclusions

In an era in which probabilistic interpretation of measures of relationship are in vogue, thi paper has taken pains to avoid mention of th interpretations as they are generally available else where. I have instead argued that the basic ques tion is what the statistic is actually measuring– its model of a relationship. Coefficients ofte seem more attractive when only their interpreta tions are provided than when their underlyin models are revealed. Thus while there is un doubtedly a place for models 1b, 2c, and 2d, the certainly will not "replace" traditional statistic since they actually measure different concepts Some measures and models might be more usefu in the general case than they are with binary data though greater generality does not guarantee tha the underlying model will be of interest.

Finally I would contend that analysts frequently should not seek a single measure and will neve find a perfect measure. Different measures exis because there are different concepts to measure There may be some bad coefficients, but there i some good in most of them. A bivariate relation

[27] Philip E. Converse and Georges DuPeux, "Politicization of the Electorate in France and the United States," *Public Opinion Quarterly*, 26 (Spring, 1962), 1–23.

[28] Hubert M. Blalock, Jr., "Causal Inferences Closed Populations, and Measures of Association," *American Political Science Review*, 61 (March, 1967) 130–136.

[29] See also Hernes's interpretation of several mea sures of relationship in the context of James Cole man's effect parameters for time-continuous Marko processes in Gudmund Hernes, "A Markovian Ap proach to Measures of Association," *American Jour nal of Sociology*, 75 (May, 1970), 992–1011.

Table 4. Generational Change in Politicization, U.S. and France*

Respondent's Politicization (t_2)	Father's Politicization (t_1): High (knows father's party) France	High (knows father's party) U.S.	Low (does not know father's party) France	Low (does not know father's party) U.S.
High (has partisan self-location)	79.4%	81.6%	47.7%	50.7%
Low (no partisan self-location)	20.6	18.4	52.3	49.3
Total	100%	100%	100%	100%
Per cent of sample	29%	91%	71%	9%

Model	Measure	France	U.S.
1a	lambda-asymmetric	.07	.00
1b	koppa	.20	.56
1c	b_{ys}	.31	.31
1c	phi	.29	.18
2d	theta	.20	.57
2d	average cohesion	.32	.32
3c	Yule's Y	.35	.35
3d	maximum cohesion	.59	.63

* Data are from Philip E. Converse and Georges DuPeux, "Politicization of the Electorate in France and the United States," p. 14.

ship can tell us much more than any single measure can summarize. If we are unwilling to examine the complete bivariate relationship, at least we should attempt to cope with statistics summarizing different aspects of a relationship. It is time to stop acting embarrassed about the supposed surplus of measures and instead make the fullest possible use of their diversity.

Appendix I

Polytomous Variables

While the text of this paper deals mainly with dichotomous variables, the distinction among the different relationship models remains relevant with nondichotomous variables. The exact considerations depend on the level of measurement.

Because the traditional statistics have seemed inadequate for multiple category nominal-level variables, probabilistic measures have been developed to cope with them. These measures—including lambda, the opposition index, the cross-cutting statistic, Goodman and Kruskal's tau, uncertainty statistics, and theta—are, however, generally not based on the classic relationship model (model 1c). Thus it should be recognized that these coefficients alter the definition of a relationship. The models implicit in these new measures may be valuable, but the statistics are not analogous to conventional interval-level statistics.

By contrast, there are very few measures at the interval level of measurement. Pearson's r is the classic model 1c coefficient and there are some related statistics. Yet the development of interesting relationship models with nominal measures has not been carried through to devising interval-level counterparts. Ironically measure development may now be required at the interval level.

The ordinal level of measurement is marked by a combination of these effects. There are not measures for all the potential models, but the available coefficients differ substantially in what they measure. For example, Kendall's tau-b is based on model 1c, Kendall's tau-c is the comparable model 2c measure,[30] and Goodman and Kruskal's gamma is a model 3c statistic. The issue involved in choosing among them is thus whether unequal numbers of variable categories and disparate variable marginals should detract from the magnitude of the relationship.

The conventional wisdom is that tau-c should be used rather than tau-b when dealing with variables having different numbers of categories. In considering the effects of social class on vote, for example, say there are three classes but only two parties with each class totally cohesive in its voting (Chart 9i). Strong monotonicity is lacking since class voting would be greater if each class voted cohesively for its own party in a three-party system. (Identically, if as in Chart 9iii there are two classes and three parties with one class voting for the first party and the other class splitting its

[30] Actually tau-c attains its maximum value only under moderate monotonicity with a uniform distribution of cases on the variable with fewer categories.

Chart 9 i
Moderate Monotonicity
($\tau_b < 1, \tau_c = 1, \gamma = 1$)

3 × 2 Tables:

Class	Party Rep.	Dem.
Upper	.20	.00
Middle	.30	.00
Lower	.00	.50

$p_{12} = p_{22} = p_{32} = 0$
Also maximal if:
$p_{12} = p_{22} = p_{31} = 0$
Or: $p_{11} = p_{21} = p_{22} = 0$
Or: $p_{11} = p_{22} = p_{32} = 0$

Chart 9 ii
Weak Monotonicity
($\tau_b < 1, \tau_c < 1, \gamma = 1$)

Party Rep.	Dem.	Class	Pa Rep.
.20	.00	Upper	.20
.20	.10	Middle	.30
.00	.50	Lower	.10

$p_{12} = p_{31} = 0$
Also maximal if:
$p_{11} = p_{22} = 0$
Or: $p_{11} = p_{21} = 0$

p_{12}
Also ma
p_{22}
Or: p_{21}

Chart 9 iii
Moderate Monotonicity
($\tau_b < 1, \tau_c = 1, \gamma = 1$)

2 × 3 Tables:

Class	Party Tory	Lib.	Lab.
Middle	.40	.10	.00
Working	.00	.00	.50

$p_{13} = p_{21} = p_{22} = 0$
Also maximal if:
$p_{12} = p_{13} = p_{21} = 0$
Or: $p_{11} = p_{12} = p_{22} = 0$
Or: $p_{11} = p_{22} = p_{23} = 0$

Chart 9 iv
Weak Monoton
($\tau_b < 1, \tau_c < 1,$

Party Tory	Lib.	Lab.	Class
.45	.05	.00	Middle
.00	.05	.45	Working

$p_{13} = p_{21} = 0$
Also maximal if:
$p_{11} = p_{23} = 0$
Or: $p_{21} = p_{23} = 0$

Chart 9. Ordinal Maximum Value Conditions for 3×2 and 2×

vote between the remaining two parties, strong monotonicity would not hold, since class voting would be greater if each class voted cohesively for its own party in a two-party system.) This is a perfect relationship under moderate monotonicity, whereas the strong monotonicity model exacts a penalty for the unequal number of categories. If perfect class voting means that each class votes cohesively for a different party, the strong monotonicity model should be used. Thus the choice between tau-*b* and tau-*c* is not a technical issue relating to the number of categories but a substantive decision about what is intended by a perfect relationship. The class polarization of the vote may best be measured by tau-*b* even when the numbers of parties and classes differ.

Many researchers feel that gamma overstates the size of a relationship while tau-*b* values are comparable to those of the familiar Pearson's *r*. The weak monotonicity model actually determines whether the relationship between the variables is maximal *given their marginals* (Charts 9ii and 9iv). Given the cla electorate and the party judges whether the classe possible. The leniency of fect relationship partly ex gamma. This model is of terest so that gamma shou of hand because of its lar whether or not the variabl critical decision made by tionship model to employ

This appendix has reli standings of the purposes value conditions rather t of them for the polyto definitions are as follow requires that (1) all inde gories be homogeneous or and (2) every increase in t from any category to the accompanied by a change value. Moderate monoto

the categories of the variable with the greater number of categories are homogeneous on the other variable and (2) every increase from any category of the variable with more categories to the next higher category results in the other variable's changing or remaining constant, but not both. Weak monotonicity assumes that (1) the categories of the independent variable are as homogeneous on the dependent variable as possible given differences in variable marginals and (2) an increase in the value of the independent variable results in the value of the other variable's changing, staying constant, or both. For ordinal and interval relationships, increases in one variable would always induce change in a single direction in the other variable; a positive relationship would entail an increase in one variable's inducing an increase in the other.

Individual and Contextual Variables in British Voting Behavior: An Exploratory Note*

C. NEAL TATE
North Texas State University

This note reports the results of an initial exploration into the significance of social contexts in the shaping of individual political behaviors. Several "individual-level" socioeconomic variables are selected to serve as one set of predictors of individual voting choices in Great Britain. Several constituency-level or "contextual" variables are selected to serve as a second set of predictors of voting choices. The relative potency of the two sets of predictors is then investigated using a well-known multivariate search strategy, "tree analysis" or AID, developed by Sonquist and Morgan.[1]

"Individual-level" variables are variables whose values are determined solely by the characteristics of individual respondents. Examples include the familiar demographic and social variables like age, sex, ethnicity, occupation, class, and religion, as well as attitudinal variables such as subjective class, party preference or identification, and issue-related opinions. The values of "contextual" variables may be determined in either of two ways: (1) by averaging an individual characteristic across all the individuals occupying a certain (usually geographically-defined) area or context;[2] or (2) by directly measuring for the area or context some characteristic which is *not* affected by the distribution of individual characteristics within the area or context. Examples of the first type of contextual variable are plentiful in the aggregate data reported in censuses: per capita income, proportions in various occupations, mean educational achievements, etc., etc. for states, provinces, districts, constituencies, *departments, gemeinde,* etc. Examples of the second type of contextual variable include such environmental traits as the relative presence or absence of geographical features such as mountains, lakes, rivers, or seacoasts; the relative presence or absence of human-created phenomena such as roads, canals, harbors, cities, schools, universities, and many other potentially relevant traits.

Many scholars have argued that social scientists should pay more serious attention to contextual variables when they go about constructing social theories.[3] Unfortunately, many such urgings have been merely hortatory, or else have been supported by relatively unsystematic citations to the scattered literature purporting to demonstrate the existence of "breakage,"[4] "structural,"[5] "compositional,"[6] or "contextual"[7] effects on a variety of individual behaviors. Methodological guidelines for the study of such effects have been proposed

* An earlier version of this paper was presented to the Annual Meeting of the International Studies Association, New York, March 16, 1973. The data utilized in this analysis were made available by the Inter-University Consortium for Political Research and the *Zentralarchiv für empirische Sozialforschung,* Cologne. The data were originally collected by David Butler and Donald Stokes and by William Miller. The original collectors and the archives supplying the data bear no responsibility for the uses made of it here.

[1] A brief discussion of the uses and limitations of this technique is given in the AID program description in Inter-University Consortium for Political Research, *OSIRIS II: OS Users Manual* (Ann Arbor: Institute for Social Research, University of Michigan, mimeo, 1971), pp. 375–388. For a more detailed discussion of the method, see John A. Sonquist and James N. Morgan, *The Detection of Interaction Effects: A Report on a Computer Program for the Selection of Optimal Combinations of Explanatory Variables,* Monograph No. 35 (Ann Arbor: Institute for Social Research, University of Michigan, 1964).

[2] An exception is the effort of scholars associated with Columbia's Bureau of Applied Social Research to develop socially defined contexts, especially through the use of "snowball" and "sociometric" sampling techniques. See Allan H. Barton, "Bringing Society Back In: Survey Research and Macro-Methodology," *The American Behavioral Scientist,* 12 (November-December, 1968), 1–9, and the works cited therein.

[3] See, for example, Erwin K. Scheuch, "Social Context and Individual Behavior," in *Quantitative Ecological Analysis in the Social Sciences,* ed. Mattei Dogan and Stein Rokkan (Cambridge: MIT Press, 1969), pp. 133–135; Tapani Valkonen, "Individual and Structural Effects in Ecological Research," in Dogan and Rokkan, pp. 53–68; and *European Politics: A Reader,* ed. Mattei Dogan and Richard Rose (Boston: Little, Brown, 1971), pp. 144–145.

[4] The term "breakage effect" is developed in Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting: A Study of Opinion Formation in a Presidential Campaign* (Chicago: University of Chicago Press, 1954), pp. 98–101.

[5] Peter M. Blau, "Structural Effects," *American Sociological Review,* 25 (February, 1960), 178–193.

[6] James A. Davis, Joe L. Spaeth, and Carolyn Huson, "A Technique for Analyzing the Effects of Group Composition," *American Sociological Review,* 26 (April, 1961), 215–225.

[7] Phillip H. Ennis, "The Contextual Dimension in Voting," in *Public Opinion and Congressional Elections,* ed. William N. McPhee and William A. Glaser (New York: Free Press, 1962), pp. 180–211.

and debated.[8] But so far, there have been few systematic efforts to demonstrate empirically the overall significance of contextual variables as predictors of dependent individual behaviors, especially relative to the significance of individual-level predictors.[9] This note serves as a first report of a cross-national investigation in which I am engaged which does attempt to confront directly the question of the relative importance of individual and contextual variables in explaining individual political behaviors. The project design calls for the investigation of the impact on several types of political behavior of a specific set of socioeconomic and political characteristics measured at both the individual and contextual levels in Great Britain, West Germany, the Netherlands, and Norway. Here only the initial findings for the British case are presented. The data utilized include the Butler-Stokes 1964 sample survey and a set of constituency-based ecological indicators from the 1966 1 in 10,000 Census survey of Great Britain.[10]

Since this note focuses on the relative importance of individual and contextual variables as predictors of individual behavior, and not on particular variables or theoretical concepts, a "search strategy" is adopted. A search strategy allows the investigator to seek out the best predictors of his dependent variables from a prespecified set of predictors. To some, the search strategy is atheoretical, a "fishing expedition."[11] But other scholars argue that rather than being "atheoretical," it is "pretheoretical," because it helps the investigator discover basic relationships in his data which may lead him to more explicit theoretical formulations.[12] I do not wish to enter this debate here. I will only note that for this investigation, an initial search strategy seems quite appropriate, since it will allow me to confront the methodological issue of the relative utility of individual and contextual variables directly, with a minimum of attention to which variables at which levels are *theoretically* significant. Later research can then focus on the question of theoretical significance of certain contextual and individual variables, provided these variables prove worthy of attention as predictors of dependent phenomena of interest.

The most widely used search strategy (other than trial-and-error table building) is undoubtedly stepwise multiple regression. The method which is used here is "tree analysis," as performed by the AID (Automatic Interaction Detector) program developed by the staff of the Institute for Social Research at the University of Michigan.[13] AID has analogies to both stepwise regression and analysis of variance. It has two major advantages over stepwise regression which make it the more appropriate search strategy for this analysis: (1) it more readily accepts the data at nominal or ordinal levels of measurement; (2) it does not require the assumptions of linearity and additivity characteristic of ordinary stepwise regression. A disadvantage of the technique is that it is usually necessary to categorize interval level data if one uses several predictors measured at this level in a single analysis.[14]

The predictor variables utilized in the AID analysis are listed in Table 1. This is not the place for an extensive discussion of the reasons for the selection of the particular individual- and contextual-level variables listed there. Essentially, two criteria governed the selection of these variables. The first was theoretical: the predictors were intended to tap the most important lines of social cleavage which, Lipset and Rokkan suggest,[15] are the basis for the formation of political

[8] See, for example, Blau; Valkonen; Davis, Spaeth, and Huson; Arnold S. Tannenbaum and Jerald G. Bachman, "Structural Versus Individual Effects," *American Journal of Sociology*, 69 (May, 1964), 585–595; Robert D. Putnam, "Political Attitudes and the Local Community," *American Political Science Review*, 60 (September, 1966), 640–654.

[9] One such effort, however, is Leo Meltzer, "Comparing Relationships of Individual and Average Variables to Individual Response," *American Sociological Review*, 28 (February, 1963), 117–123.

[10] The survey data were originally collected by David Butler and Donald Stokes. See *Political Change in Britain: Forces Shaping Electoral Choice* (New York: St. Martin's, 1969). They were provided by the Inter-University Consortium for Political Research. The census data were originally collected by William Miller and were made available through the *Zentralarchiv für empirische Sozialforschung* of the University of Cologne. The original collectors and the archives supplying the data, of course, bear no responsibility for the uses made of it in this analysis.

[11] For arguments which, at least by implication, condemn such atheoretical approaches, see Robert T. Holt and John M. Richardson, Jr., "Competing Paradigms in Comparative Politics," in *The Methodology of Comparative Research*, ed. Holt and John E. Turner (New York: Free Press, 1970), pp. 21–71.

[12] See the comments of Lawrence C. Mayer, *Comparative Political Inquiry: A Methodological Survey* (Homewood, Ill.: Dorsey Press, 1972), pp. 279–280 for a discussion of the research strategy appropriate for a pretheoretical discipline such as political science.

[13] Sonquist and Morgan; John A. Sonquist, "Finding Variables That Work," *Public Opinion Quarterly*, 33 (Spring, 1969), pp. 83–95; and John A. Sonquist, *Multivariate Model Building: The Validation of A Search Strategy* (Ann Arbor: Institute for Social Research, University of Michigan, 1970).

[14] Inter-University Consortium for Political Research, p. 375.

[15] Seymour Martin Lipset and Stein Rokkan, *Party Systems and Voter Alignments* (New York: Free Press, 1967), pp. 1–64. The basic cleavages discussed by Lipset and Rokkan are social and economic in nature. Consequently, the individual and contextual indicators in Table 1 are all social and economic variables. It is possible that more variance would be

Table 1. Predictor Variables Utilized in AID Analyses

Individual-Level Variables

1. Subjective Social Class
2. Trade Union Membership
3. Employment Status
4. Age in Decades
5. Number of Children in Home
6. Head of Household's Occupation
7. Head of Household's Income
8. Respondent's Occupation
9. Respondent's Income
10. Whether Respondent is Dependent Person
11. Urban/Rural Residence
12. Religion
13. Frequency of Church Attendance
14. Residential Mobility
15. Length of Residence in Home
16. Number of Sources of Political Information
17. Type of Housing

Contextual Variables

1. % Aged 0–4 Years
2. % Aged 65 and Older
3. % Dependent Persons
4. Private Houses per Capita
5. Council (Public) Houses per Capita
6. Owner-Occupied Houses per Capita
7. Private Cars per Capita
8. % Northern Ireland Immigrants
9. % Irish Republic Immigrants
10. % Commonwealth Immigrants
11. % Irish plus Commonwealth Immigrants
12. % Professionals, Managers, and Employers
13. % Manual Workers
14. % Unemployed
15. % With No Job During Last Year
16. Nationality (English, Scots, Welsh)

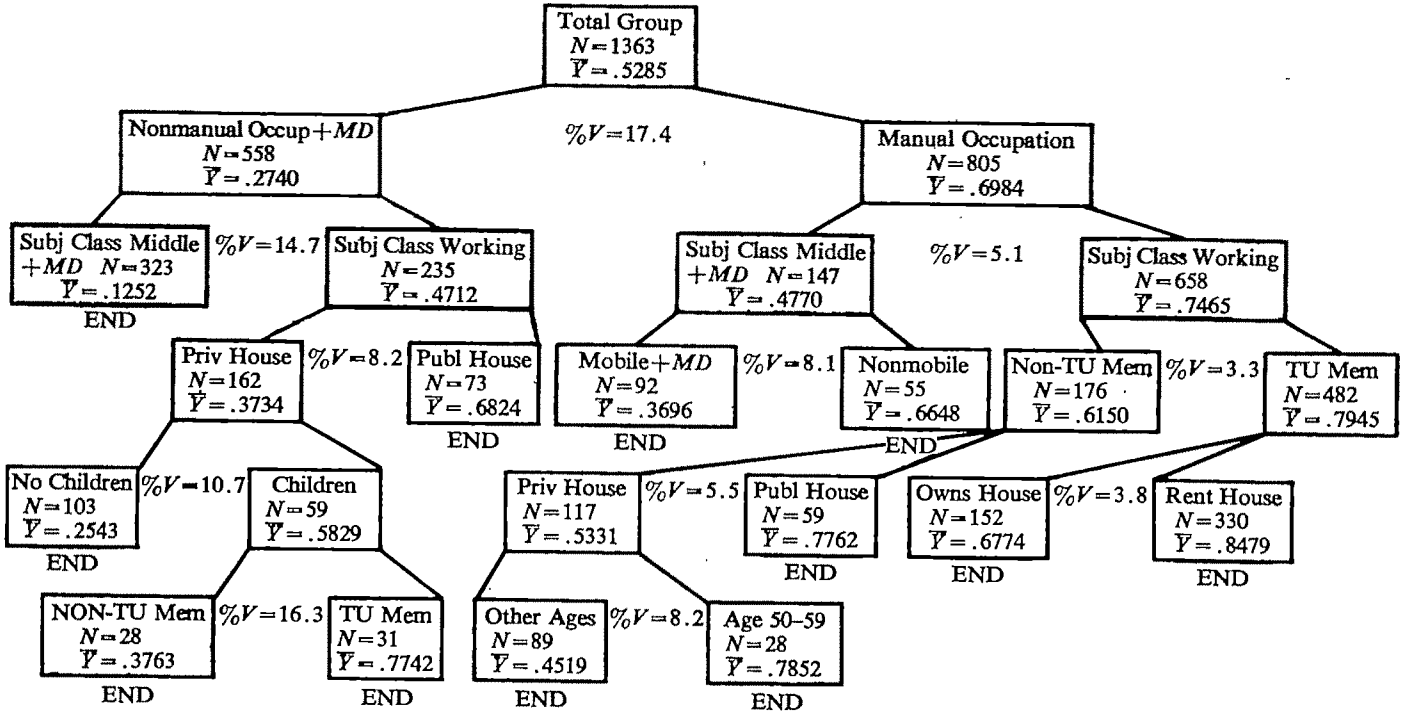
party followings. The second was the pragmatic criterion of availability in the data sets at my disposal.[16]

Figures 1–3 present the results of three AID analyses using the individual and contextual predictors listed in Table 1 to predict Labour party voting in the 1964 British national election for the 1363 respondents who voted either for Labour or the Conservatives. Figure 1 is the "tree" which resulted from an AID analysis utilizing only the *individual-level* indicators in Table 1 as predictors. In each box of the "tree," N represents the number of respondents having the set of characteristics represented by the box, and $\bar{Y}$ is the proportion of these respondents reporting a vote for Labour in 1964. The "per cent V" which appears at each "branching" or "split" in the tree is the proportion of the variance in the preceding or "parent" group *accounted for by that branching*. Groups in the tree were split into further groups whenever the resulting split fulfilled two criteria: (1) the "Between Sum of Squares" produced by the split was at least .6 per cent of the "Total Sum of Squares" for the whole sample of 1363; (2) the N in each of the resulting groups was greater than or equal to 25. Figure 2 is the tree resulting from an AID analysis using only *contextual* variables as predictors of Labour vote. Figure 3 is the tree produced when *all* predictors—individual and contextual—were utilized to predict Labour vote in 1964.

The structure of Figure 1 demonstrates the overwhelming importance of class and class-related variables among the set of individual indicators in Table 1. The first-level split divides the sample on the basis of head of household's occupation into manual and nonmanual workers, a division which evokes the classic "middle class-working class" dichotomy. The second-level split demonstrates the importance of "subjective social class," as it divides each occupational group on the basis of the self-described class statuses of the respondents. By this stage, the difference of proportions for Labour in the two extreme groups—the consistently middle class and consistently working class categories—is .6213, a formidable figure. At the third level, it proved impossible to split the consistently middle-class group in any useful way. The other three categories are split on two additional class-related indicators (trade union membership and type of housing) and on residential mobility. At this point the tree is no longer symmetrical, indicating that a linear and additive model no longer applies to the data. But the increase of .0480 in the maximum difference of proportions is not negligible. The overall maximum difference of proportions (.7227) is achieved with the fourth-level branchings. Two of the three splits at this level are made on the basis of type of housing (each split involves a slightly different dichotomization of the housing variable), while the third is based on number of children in the home, a variable which significantly affects standards of living. The final (fifth-level) splits merely produce some further differentiations in the proportions for Labour for two intermediate categories. Yet even at this level class-related indicators continue to be significant, as the more potent of the splits is based on trade union membership.

explained if individual psychological orientations (such as issue opinions) and/or contextual political indicators (such as past party voting) were utilized in the analysis. Other studies might find it worthwhile to include such alternative predictors. They were omitted here to maintain manageability of data in an openly exploratory analysis.

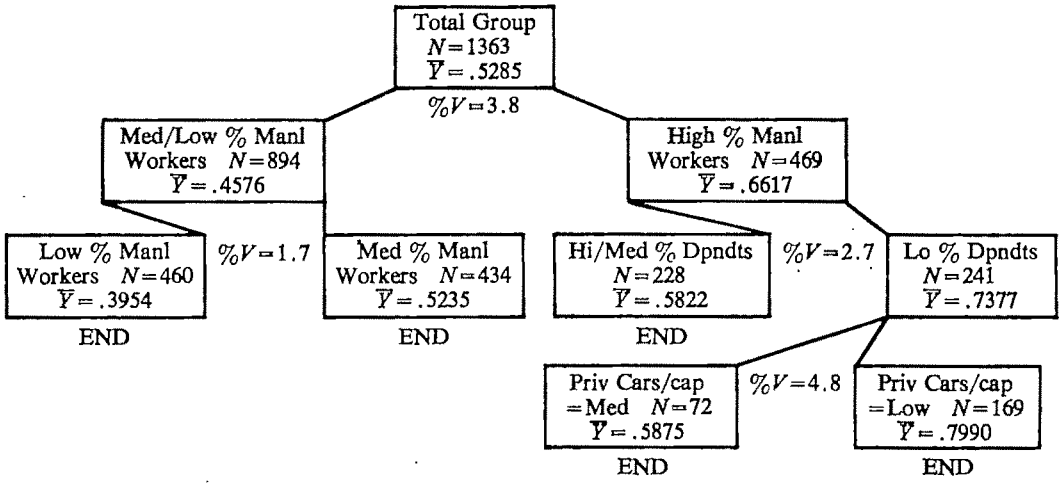
[16] Since the independent variables were restricted to indicators of the basic socioeconomic cleavages in Western Europe, there were in fact few problems of availability. All the relevant cleavages could be operationalized by at least one individual and one contextual indicator in the available data sets.



$\bar{Y}$=Proportion of group voting Labour in 1964; %V=% of Total Sum of Squares of "Parent" group explained by this split.

Percentage of Total Sum of Squares accounted for by this tree=32.35%.

Figure 1. Summary of AID Analysis Using Only Individual-Level Variables



$\bar{Y}$ = Proportion of group voting Labour in 1964; $\%V$ = % of Total Sum of Squares of "Parent" group explained by this split.

Percentage of Total Sum of Squares accounted for by this tree = 6.4%.

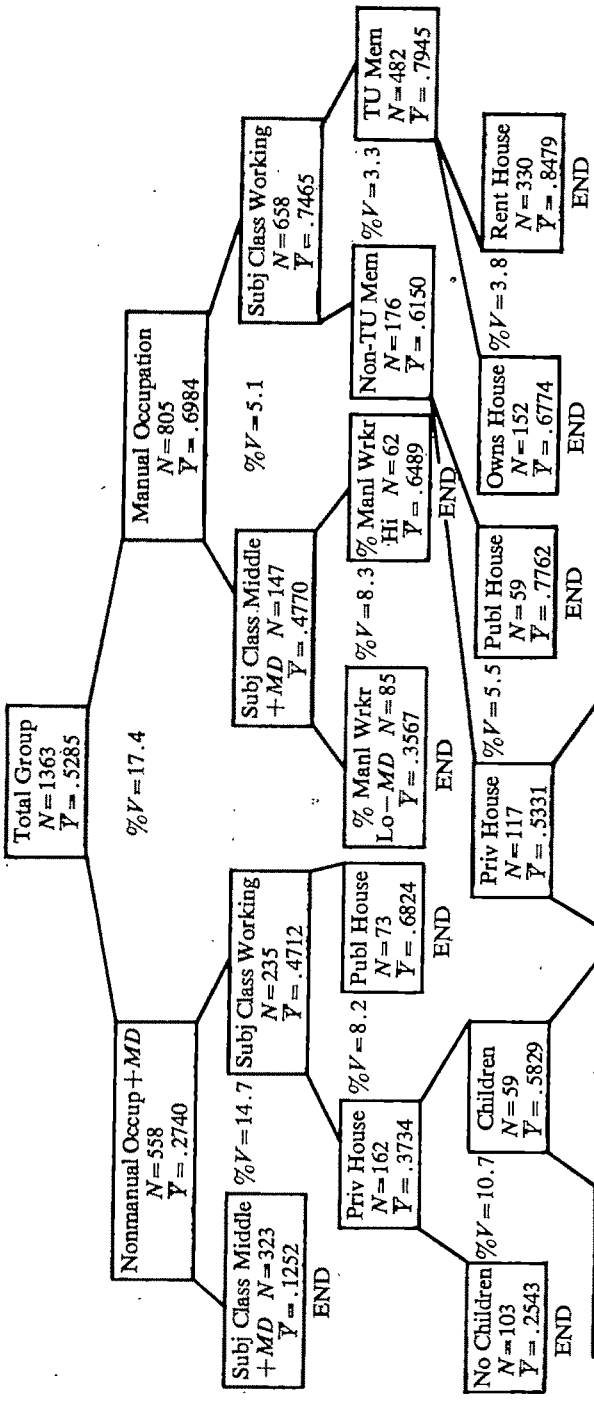
Figure 2. Summary of AID Analysis Using Only Contextual-Level Variables

Even the casual reader will easily notice the striking differences in complexity and potency between the trees in Figures 1 and 2. The individual variables tree (Figure 1) has five levels of splits; the contextual variables tree has only three. The *first* split in Figure 1 accounts for a greater proportion of the variance in Labour-Conservative voting (17.4 per cent) than does the entire tree in Figure 2 (6.4 per cent). Overall, the tree in Figure 1 explains 32.35 per cent of the variance in Labour-Tory voting in 1964, more than five times as much as the contextual tree in Figure 2. Nevertheless, the contextual tree does deserve some limited discussion.

In order to simplify and make manageable the contextual AID analysis and to make possible the combination of individual and contextual variables in the succeeding joint AID analysis, the interval scale contextual indicators were trichotomized. For each variable, roughly equal-sized groups were created representing "High," "Medium," and "Low" levels of the contextual variable. Obviously this procedure "wastes" a good part of the information provided in the original indicators. But it seems unlikely that it would destroy any extant relationship between any contextual variable and the dependent variable, unless that relationship was exceedingly weak to begin with. At this early stage of the analysis, I felt the loss of information could be justified by the gains in ease of computation and interpretability.

The most important variable among the contextual indicators is the proportion of manual workers in the constituency. It is utilized in two of the four splits in the tree and accounts for more than three-fourths of the total explained variance associated with the tree (4.9 per cent out of a total 6.4 per cent). The other two splits involve the percentage of dependent persons (i.e., persons in the very young and very old age brackets) in the constituency and the number of private cars per capita. The latter is—in the British case, at least—an indicator of the class context of the constituency just as is the percentage of manual workers. The dependency percentage is not directly a class-related indicator, but the other class-related indicators (mostly per cent manual workers) are clearly as dominant in the contextual tree as they were in the individual tree.

The difference in potency between the individual and contextual trees immediately suggests that contextual variables are far less useful as predictors of individual voting choice than are individual variables, or, more precisely, that the contextual variables included in *this* study are less useful than the individual variables included in *this* study. But in order to achieve a more definitive judgment on their relative usefulness, the two sets of variables were combined in a third AID analysis. Every predictor was free to "compete" with every other predictor, and only the "best" predictors were utilized in constructing the AID tree. That tree is presented in Figure 3. An examination of the tree reveals startling evidence of the relative insignificance of contextual variables compared to individual variables. To all but the most careful eyes, Figure 3 must look exactly like Figure 1. In fact, it *is* exactly like Figure 1 with one small exception: the middle, third-level split which was made on the basis of residential mobil-



Ȳ = Proportion of group voting Labour in 1964; %V = % of total sum of Squares of "Parent" group explained by this split.
Percentage of Total Sum of Squares accounted for by this tree = 32.37%.

Figure 3. Summary of AID Analysis Using Individual and Contextual Variables

ity in Figure 1 is made on the basis of a contextual variable, percentage of manual workers, in Figure 3. *No other contextual indicator appears at any place in the tree.* The one which does appear does not affect the difference in proportions voting Labour for the most extreme groups. The split in which percentage manual workers appears produces a slightly smaller difference in that proportion between the two resulting categories than the corresponding split in Figure 1 on residential mobility, but it does increase the total amount of variance explained by the tree from 32.35 per cent to 32.37 per cent (!).[17]

[17] Another way of evaluating the trees in Figures 1–3 is by ascertaining the reduction in the number of prediction errors for individual respondents made when using the information provided by the individuals' final group classifications in the trees rather than the overall mode. Without knowledge of individual characteristics, one's best prediction for each individual voter would be the mode: a Labour vote. Predicting the overall mode for all 1363 respondents leads to 641 errors in prediction. Predicting the mode of each final group for all individuals in the group produces the following numbers of prediction errors: Figure 1, 315; Figure 2, 548; Figure 3, 315. For Figures 1 and 3 the proportionate reduction in error is .51 [(641–315)/641]. For Figure 2, it is only .15. Using these measures rather than the proportion of variance explained leads to identical conclusions.

Overall, this analysis suggests a remarkably clear conclusion: contextual variables have little or nothing to add to explanations of individual political behavior based on individual variables—at least for these variables, voting choice, and this sample of the British electorate. Nor do the contextual variables yield satisfactory alternative explanations of British voting behavior in 1964 even if individual variables are ignored. Despite the limitations of this study, it does not take a great deal of courage to suggest that its findings may well be duplicated in other investigations of the utility of contextual indicators as predictors of individual behaviors. Yet British voters may be atypical; their voting choice may be a relatively idiosyncratic dependent variable; the contextual variables taken into account here may be singularly ill-chosen. All these possibilities suggest that further careful research into the overall utility of contextual variables as molders of individual political behaviors is needed.

Community and Control: Critical Democratic Theory in the Progressive Period*

DAVID E. PRICE
Duke University

Current political reflection and criticism in the United States, replete with pronouncements as to the moral and intellectual "poverty" of the liberal tradition and its imminent (or hoped for) demise, has decisive antecedents and striking parallels in the Progressive era. It is the purpose of this paper to explore the critique mounted by American intellectuals in this earlier period—the indictments brought against the liberal capitalist order, the arguments made and the means chosen in the dual quest for fraternity and order, the implications for how "politics" is to be conceived and practiced—and briefly to assess its continuing reverberations.

It has often been noted that the challenges to liberal thought and practice that arose in chorus toward the end of the nineteenth century characteristically asserted the need for a rediscovery of *community* in American life, although the terminology and the specific prescriptions varied. Acquisitive individualism seemed to have run rampant in what Parrington aptly termed America's "great barbecue"; thoughtful men perceived not the survival of the fittest but a rising tide of human misery, economic instability, narrowed paths of opportunity, shocking concentrations of wealth, the oppressive exercise of private power, a debasement of moral and cultural standards, and the erosion of traditional patterns of deference and loyalty. Robert Wiebe points to a resultant desire for community "self determination, purity, and unity," the expressions of which ranged from anti-monopoly and anti-alien political movements, to the growth of farmer and labor alliances, to prohibitionism and the single-tax panacea.[1] Progressive publicists and social theorists took up the community theme, often consciously employing it as a corrective to what John Dewey termed "the liberalism of the school of Locke [and] the economists."[2]

It is the argument of this paper that, while the idea of "community" is undeniably central to the social thought of the period, its use as an overarching rubric may complicate more than it simplifies the tasks of those who would critically analyze or appropriate that thought. In the Progressive period, as in our present-day social criticism, the "quest for community"[3] carries divergent meanings and projects a wide variety of social forms. This paper will highlight two quite different, and not always complementary, notions of community which continue to play an important role in our political discourse. These two usages, it should initially be noted, roughly correspond to two primary assumptions of the ubiquitous and seemingly monolithic liberal system that is under attack. The first and most basic assumption, that the "free" individual is the basic unit of the political order and the protection of his life, liberty and property is the state's basic task, is questioned to varying degrees by the thinkers we shall term "communitarians." This "individualist" assumption has classical and Christian roots but dates, in its modern guise, from the rise to political ascendance of the bourgeoisie and receives its definitive articulation in the writings of Hobbes and Locke. The second assumption, the particular target of Progressives whom we shall term theorists of "social control," finds ambiguous support in Locke (and none in Hobbes) but owes more to *laissez-faire* economics. A natural harmony or equilibrium is assumed among "free" individuals (or groups); the safeguarding of the life, liberty and property of each is regarded as a sufficient protection of the *common* good; an extensive role for govenment in this connection is viewed as at once unnecessary and dangerous. It is to thinkers particularly critical of these latter dogmas, which of course were elevated and rigidified in the nineteenth century by way of Social Darwinism, that we first turn.

Ward, Ross and Social Control

Among late nineteenth-century intellectuals, it was Lester Frank Ward who leveled the most telling single blow at the second of American liberalism's cardinal assumptions—at what Reinhold Niebuhr later termed its "soft utopianism."[4]

* For valuable criticisms and suggestions I am indebted to Jane Flax, John Hallowell, David Mayhew, Marcella McDonald, Carey McWilliams, Andrew Scott, and Thomas Spragens.

[1] Robert H. Wiebe, *The Search for Order, 1877–1920* (New York. Hill and Wang, 1967), chap. 3.

[2] *Liberalism and Social Action* (1935; reprinted, New York: Capricorn, 1963), chap. 1.

[3] R. Jackson Wilson uses this theme to tie together his suggestive sketches of five thinkers of the period. *In Quest of Community: Social Philosophy in the United States, 1860–1920* (New York: Oxford, 1968). See also Jean B. Quandt, *From the Small Town to the Great Community: The Social Thought of Progressive Intellectuals* (New Brunswick: Rutgers University Press, 1970).

[4] "*Soft utopianism* is the creed of those who [in contrast to the Marxist 'hard utopians'] do not claim

Ward was far from abandoning the analogy perceived by the Social Darwinists between the laws of natural selection as they operated in the biological realm and in society, but he insisted that in both realms, "nature" left untended is wasteful and destructive. "I regard society and the social forces as constituting just as much a legitimate field for the exercise of human ingenuity as do the various material substances and physical forces," wrote Ward in his first major treatise. "The latter have been investigated and subjugated. The former are still pursuing their wild, unbridled course. . . . Why cry '*Laissez faire*!' as if society would ever work out its own progress? As well say to all inventors: Cease trying to control nature, let it alone and it will control itself. . . ."[5] Ward's "dynamic sociology" thus purports to establish a scientific basis for social intervention and control.

Ward appears at first glance to be challenging the first of liberalism's basic tenets as well, for he stresses that the fetish of "abstract" individual right must be transcended if society is ever to become "the arbiter of its own destiny." But it soon becomes clear that his objection is not to the way "liberty" is conceived—in individualistic, utilitarian terms—but rather to the current operationalization of the concept in terms of absolute legal immunities. Such immunities simply preserve the status quo, prohibiting government "interference" and enabling men to defend policies as "principled" which in reality concentrate power and effective liberty in the hands of the few. Ward in fact quickly assures his readers that he is denying abstract rights only "in theory," for his intention is to increase the sum of human happiness, which is, after all, "the only conceivable object of any right."[6] Nor is he guilty of that "false pride" which would grant to human feeling and aspiration a basis apart from the fundamental impulse to seek pleasure and avoid pain. Finding "no generic distinction" between the basic desires of man and the lower animals, Ward goes on to portray the happiness of society as "nothing else than the happiness of its individual units." The diversity of men's appetites makes their direct satisfaction by government impossible; still less does sociology or politics have to do with subjective, "moral" utilities. What "scientific utilitarianism" concerns itself with is rather the "correspondence between organism and environment," so as to devise means whereby a larger proportion of individual actions aimed at happiness may succeed in securing their object. Ward does not seek, in short, to assess or shape individual ends or modes of fulfillment; his aim in attacking *laissez-faire* is rather to facilitate their realization. "The problem of dynamic sociology is the *organization of happiness*."[7]

E. A. Ross, a disciple (and nephew by marriage) of Ward, and an acknowledged pioneer in American social science after his publication of *Social Control* in 1901, carried forward the spirit of reform "within the very discipline that Spencer had hoped would teach men to let things alone."[8] Ross has a more Hobbesian sense than Ward of the way the capitalist system "violently thrusts men apart," creating the need for "artificial frames and webs that may hold the social mass together."[9] Appropriating the terminology of Ferdinand Tönnies, Ross points to the powerful forces that are "more and more transforming *community* into *society*, that is, replacing living tissue with structures held together by rivets and screws." But like Ward, Ross shows little sense of loss. Nor does he anticipate the establishment of control through the renovation of communal institutions and sentiments. Modern cities and nations have, he acknowledges, assumed certain community functions, but they remain preeminently "societies." "It is perhaps safe to say that we are relying on artificial rather than natural supports to bear the increasing weight of our social order, and that a return to the natural basis of social partnership is about as likely as a return to raw food or skin garments."[10]

In fact, while Ross has no doubt that liberal man must be harnessed, he is dubious about not only the possibility but also the desirability of transforming the individual's self-seeking instincts.[11] As communal associations lose their hold on man, human nature displays its "heights" as well as its Hobbesian "depths," Ross asserts, while friendship and association assume their "sweetest" form as they become no longer inherited but voluntary.[12] It is thus not merely out of pessimism that Ross chooses the negative and apparently modest definition of social order as

to embody perfection, but expect perfection to emerge out of the onging process of history" (Harry R. Davis and Robert C. Good, eds., *Reinhold Niebuhr on Politics* [New York: Scribner's, 1960], p. 13).

[5] Lester F. Ward, *Dynamic Sociology* (New York: Appleton, 1883), Vol. I, pp. 35, 53.

[6] *Ibid.*, p. 32.

[7] *Ibid.*, Vol. II, pp. 122, 133, 153–156. See also Ward's later *Psychic Factors of Civilization* (Boston: Ginn, 1892), chap. 2, 16.

[8] Richard Hofstadter, *Social Darwinism in American Thought* (Boston: Beacon, 1955), p. 161.

[9] Edward A. Ross, *Social Control: A Survey of the Foundations of Order* (New York: Macmillan, 1904), p. 53.

[10] *Ibid.*, pp. 432–436.

[11] See the discussions in Wilson, *Quest of Community*, pp. 102–113; and Christopher Lasch, *The New Radicalism in America, 1889–1963* (New York: Random House, 1965), pp. 170–177.

[12] *Social Control*, p. 405.

the "absence of collisions" and assures his reader that "the goal of social development is not, as some imagine, a Perfect Love, or a Perfect Conscience, but better adaptation; and the more this adaptation is artificial, the less need it be natural."[13] Ross reveals his fear that "the art of domesticating human beings may succeed only too well;" he can only hope that whatever "adaptation" need be imposed will leave individual variety and autonomy of purpose unimpaired.

Both Ward and Ross harbor certain utopian expectations for their new order, despite its artificial and instrumental character. Ross answers his own apprehensions about "the coalescence of physical and spiritual forces in the modern state" with an uneasy anticipation that diversity can be preserved through the continued vitality of the press, religious, professional, and cultural associations, and the universities—and that the uniquely "energetic, self-assertive, and individualistic" Aryan peoples will discover modes of coalescence and control that, while reinforcing the "humane" and suppressing the predatory instincts, will preserve individual distinction and excellence.[14] Serious and unresolved tensions remain in Ward's work; his anticipation of a classless, impartially administered "sociocracy" seems to reintroduce the liberal notion of natural harmonies, or else to presuppose a program of education and socialization which will radically transform individual "utilities."[15] But however problematic their assumptions might be, the fact remains that Ward and Ross do not think of reform as providing a new social and moral setting for the individual or transforming the self-interested, utilitarian quests the liberal state was supposed to protect. Theirs is a critique of means, not ends; their point is not that the ends of the liberal state are impoverished, but rather that *laissez-faire* is rendering them contradictory and destructive.

The notion of community represented by Ward and Ross—that of a society free of "collisions," managed so as to maximize the realization of individual "happiness"—was an enduring legacy of the Progressive period. The old juxtaposition of governmental power and individual liberty was overcome, at least in theory; there was, as Herbert Croly was fond of saying, a new realization that Hamiltonian means could serve Jeffersonian ends.[16] In the end, Robert Wiebe reports, the early Progressive quest for an "organic city" of brotherhood and compassion gave way to a vision which "accepted the impersonal flux of the city and anticipated its perfect systematization."[17] But the alternative visions generated during the period should perhaps not be so easily dismissed, in terms of either their social and intellectual significance or the echoes to be found in the present. It is, then, to ideas about liberalism and community considerably divergent from those of Ward and Ross that we now turn.

Cooley's Alternative Vision

Lester Ward was to be admired for his challenge of those who would misuse Darwin to defend the *status quo*, wrote the young sociologist Charles Horton Cooley in his journal in 1897. But Ward's relegation of the individual to utilitarian ends and of society to physical laws represented, he thought, almost as great a betrayal of human dignity as that perpetrated by the Social Darwinists. "Society," Cooley declared, "is spiritual, but current philosophy does not see this, it sees only the shell."[18]

Cooley's critique of Spencer and Sumner focused not so much on their *laissez-faire* conclusions as on the materialism and determinism of their basic premises: "The physiological aspect of life is expounded and assumed, apparently, to be the only aspect science can consider."[19] Cooley challenged the conceptualization of social evolution in terms of mere physical "survival" and questioned whether, in any case, it was to be primarily explained in terms of biological selection. Man's capacity to "adapt" and develop depends rather on his own unique powers of imagination and intellect. Cooley, like Ward and Ross, demands that these powers be utilized for the purposive control of social forces, but he goes on to insist that progress must be seen as something *more* than the mere "growth of intelligent control."[20] Cooley, later to be heralded as "more than any other single person" the founder of social

[13] *Ibid.*, pp. 1, 437.

[14] *Ibid.*, pp. 172–179, 437–440.

[15] Ward, *Dynamic Sociology*, Vol. II, chap. 14.

[16] See Samuel H. Beer, "Liberalism and the National Idea," *The Public Interest*, no. 5 (Fall, 1966), 70–82.

[17] Wiebe, *Search for Order*, p. 170.

[18] Quoted in David W. Noble, *The Paradox of Progressive Thought* (Minneapolis: University of Minnesota Press, 1958), p. 107. See also John Dewey's early review of Ward's *Psychic Factors of Civilization* in *Psychological Review*, 1 (July, 1894), 400–411. Dewey focuses on the incongruity of Ward's attempt to ground society's purposive, creative "action" in a mechanistic, reflexive individual psychology. In order adequately to support his recommendations, Dewey asserts, Ward should have stated "the mental life in active terms, those of impulse and its development, instead of in passive terms, mere feelings of pleasure and pain" (p. 405).

[19] Charles Horton Cooley, *Human Nature and the Social Order* (New York: Scribner's, 1902), p. 92.

[20] Charles Horton Cooley, *Social Process* (New York: Scribner's, 1918), p. 405. See also his earlier critique of Social Darwinism: "The Process of Social Change," *Political Science Quarterly*, 12 (March, 1897), 63–81.

psychology in America,[21] vigorously insisted that students of society "cease circumscribing and testing ourselves by the canons of physical and physiological science." Those undertaking the tasks of social analysis and reconstruction must rather "answer to the organic character of the social process;"[22] this means understanding a society's potentialities and aspirations from *within* and charting the path of advancement and the ends of social control in qualitative, non-material terms. "Society is not a machine, controlled chiefly by the powers of darkness, but an expression of human nature, capable of reflecting whatever good human nature can rise to."[23]

Cooley's view rests on the conviction that the individual cannot properly be viewed apart from the social relationships in which he is enmeshed, "in living unity with social wholes." Modern society, to be sure, is characterized by a plurality of groups and often by voluntaristic modes of association, but it is a delusion to suppose that the individual's desires and valuations spring from "the inscrutable depths of the private mind" and hence may be abstracted from or set in opposition to those of "society." The point is not simply an academic one for Cooley, though of course his exploration of the collective formation of "mind" and of the role of primary groups in particular was central to the agenda he laid down for his discipline. His writings are permeated with a passion for ethical regeneration and social reform, the key to which lies in the reconstituting of the social environments where persons form and implement their ideal ends. The values characteristic of American society, while "individualistic" to an extreme, are no more "private" than those of more traditional societies; Cooley parallels Veblen in his description of how the modern market economy "largely dictates the valuations which it afterwards equilibrates."[24] But those values do bespeak a breakdown in community life, a breakdown which, far from freeing man to pursue his "basic" needs and desires, has instead impoverished him, denying his sympathetic feelings and his spiritual and aesthetic impulses. The environments which once sustained men in their interpersonal relationships and their devotion to transcendent ideals have too often succumbed before a commercial system that in its ideology and social consequences has dragged "human nature . . . by the hair of the head."[25]

[21] Talcott Parsons, "Cooley and the Problem of Internalization," in Albert J. Reiss, Jr., ed., *Cooley and Sociological Analysis* (Ann Arbor: University of Michigan Press, 1968), p. 67.

[22] Cooley, *Social Process*, pp. 397–401.

[23] Charles Horton Cooley, *Social Organization: A Study of the Larger Mind* (New York: Scribner's, 1909), p. 205.

[24] Cooley, *Social Process*, p. 299.

[25] *Ibid.*, p. 235.

Cooley's work thus in an important sense is a brief for human freedom, defined as the "opportunity for right development"—and in that very definition is implied a critique of "freedom" as late nineteenth-century American liberalism had come to define it: "cold, harsh, and spiritually poor, and hence not really free." In some strikingly modern passages, Cooley suggests that the individual utilities generated by the market economy and perpetrated by its dominant classes are not an authentic expression of either social or individual needs and hence are an unreliable *datum* for the social reconstructionist:

> So strong is the individualist tradition in America and England that we hardly permit ourselves to aspire toward an ideal society directly, but think that we must approach it by some distributive formula, like 'the greatest good of the greatest number.' Such formulas are unsatisfying to human nature, however justly they may give one aspect of the truth. . . . No aggregation of merely individual good can satisfy the need of the imagination for a unitary conception. . . .
>
> It would be fatuous to assume that the market process expresses the *good* of society. The demand on which it is based is a turbid current coming down from the past. . . . To accept this stream as pure and to reform only the mechanism of distribution would be as if a city drawing its drinking-water from a polluted river should expect to escape typhoid by using clean pipes.[26]

What is needed is not merely renovated structures of economic "opportunity" or the redistribution of the material requisites of effective "freedom," but rather a purification of the wellsprings of valuation and motivation—polluted in an atomistic, competitive society—in terms of which freedom and opportunity are defined and sought. That Cooley should find Ward's projected sociocracy spiritually unsatisfying is hardly surprising.

The key to this moral regeneration and the individual fulfillment it promises is for Cooley the communities in which modern man finds himself. Chief among these, both in strength of impact and in humanistic potential, are the "primary" face-to-face groups—families, work groups, neighborhoods—where "what is universal in human nature and human ideals" is nurtured and men's sympathetic impulses are cultivated and directed to appropriate objects. "All the abuses which are the objects of reform," Cooley quotes Thoreau as saying, "are unconsciously amended in the intercourse of friends."[27] The task is to maintain or recreate viable primary associations amid social and economic forces that have undermined the form they once took and the allegiance they once commanded; "there is nothing upon which the

[26] *Ibid.*, pp. 316, 417–418.

[27] Cooley, *Social Organization*, p. 34.

conscience of the time is more determined than restoring them to health."[28]

In *Social Process* (1918) Cooley turns explicitly to the form such groups might assume under modern conditions and how their functions might be performed. Particular emphasis is placed on the work group, which may assume face-to-face form but which, on another level, will incorporate dispersed persons and groups performing the same or related social functions. Cooley aims at a kind of professionalization that will foster "*esprit de corps*, emulation and standards . . . all animated with a spirit of loyalty and service to the whole. To achieve this would call for no change in human nature, but only in the instigation and direction of its impulses; it would mean chiefly firmer association and clearer ideals of merit among those pursuing the several functions. . . . Freedom, self-expression, and the competitive spirit would be cherished, but could not degenerate into irresponsible individualism."[29] And what of the state, either as an integrating mechanism or as a community in its own right? Cooley is uninspired by the liberal view—"Our picture of the republic is mostly a child's sketch, without beauty or form of depth and harmony of color"—and cautiously announces his own Hegelian proclivities. He cites Ross approvingly, insisting that a purely private view of morality will not suffice for those who would protect the community from the depredations of the "engines of politics or commerce." He attempts to go beyond Ross, however, in giving that public morality a positive substance that will nourish the individual and direct his sympathetic impulses. Cooley says a great deal about how new modes of communication and economic interdependence have made a more inclusive community both possible and necessary. "We need a collective life which, without repressing individuality, personal or local, shall afford central emblems that all may look up to and a discipline in which all may share. A deeper community spirit is needed throughout our society . . . leading up from the neighborhood to the nation."[30]

In the end, however, Cooley echoes many of Ross's misgivings concerning a holistic view of the national community—misgivings grounded not in Ross's suspicion of attempts to institute "Perfect Love" but rather in a concern for the *quality* of the "deeper community spirit" he anticipates. Cooley almost never mentions the Hegelian view of the state without deploring its corruption through "militarism and bureaucracy" in contemporary Germany. And he continually warns against "formalism" in the state—against imposed ideals or bureaucratic coercion which, no less than the "disorganization and disintegration" of *laissez-faire*, divorce human nature from social institutions and take hold of the individual only "by the outside, his personality being left to torpor or to . . . ruthless self-assertion."[31] On the one hand, Cooley doubts that the state can ever engage and socialize the individual after the fashion of the more particularistic group; on the other hand, he fears the conformity and coercion that might result if the state took on a unitary, organic character. His projected model of society is thus pluralistic:

> Our discipline needs to be as diverse as our society. . . . Democracy should not mean uniformity, but the fullest measure of differentiation, a development everywhere of special spirits—in communities, in occupations, in culture groups, in distinctive personalities.[32]

And when Cooley turns to the problem of integrating these loyalties and resolving conflicts among them, his tendency is not to think of ideals and institutions at the national level so much as to invoke a higher religious loyalty which transcends and reconciles all.

Viewed, then, as a critic of nineteenth-century liberalism, Cooley appears far more radical and penetrating than those who simply took aim at the Social Darwinist version of the theory of the invisible hand. He rather focused on the failure of life, liberty and property in their contemporary incarnation to do justice to men's social and sympathetic impulses. But when he attempted to portray the communal contexts in which individuality could find a fuller development and expression, he vacillated between nostalgia for a kind of familial and small-town interaction that, if it ever existed, was not in any case to be restored, and the projection of a professionalization around social functions that hardly warmed the heart and was far more restricted in terms of occupation and social stratum than he acknowledged. And when he considered political entities, particularly the nation, as communities, he displayed a profound ambivalence. On all these counts, as we shall see, Cooley was far from atypical of the social critics of the Progressive period.

Variations on the Communitarian Theme: Royce, Croly, Dubois, Dewey

Considerations of space forbid developing at any length the explicit or implied critiques of liberalism to be found among Cooley's contemporaries. What can be demonstrated, however, is that the Michigan sociologist was not alone either in his preoccupation with the individual's "wholeness" or in his conflicts and uncertainty as to what sort of community might provide moral sustenance and develop sympathetic relationships

[28] *Ibid.*, p. 27.
[29] *Social Process*, p. 143.
[30] *Ibid.*, p. 419; Compare *Social Organization*, part VI.
[31] *Social Organization*, p. 349.
[32] *Social Process*, p. 148.

under modern conditions. We will consider briefly the ideas of four thinkers—Josiah Royce, Herbert Croly, W. E. B. DuBois, and John Dewey—who shared Cooley's central perceptions but who differed from him and from one another in the positive community ideals they projected.

Royce, like Cooley, draws heavily on the pioneer psychologist James Mark Baldwin in portraying the psychological dependence of the individual on his social environments. The keystone of his social thought, however, is a prior notion of the nature of moral man. Invoking Kant, Royce declares the "first principle of ethical inquiry" to be that "my duty is simply my own will brought to my clear self-consciousness." But a second principle turns man to those "superindividual unities" in which he finds himself:

> I, left to myself, can never find a plan of life. I have no inborn ideal naturally present within myself. By nature I simply go on crying out in a sort of chaotic self-will, according as the momentary play of desire determines.[33]

Royce can call himself a "voluntaristic idealist" or an "absolute pragmatist," because in his portrayal of the absolute and of man's dependence on the communities which embody transcendent ethical ideals, he can claim to be doing no more than developing notions of truth and responsibility which do justice to human need and experience. Man's ethical impulses *require* for their satisfaction and integration the apprehension of an ideal which stands over against the self and is seen to possess its own intrinsic value. This apprehension is neither passive nor abstract; Royce rather subsumes "the whole moral law" under the principle of *loyalty*, defined as "the willing and practical and thoroughgoing devotion of a person to a cause." And he continually stresses that the practical result of loyalty is to relate men to one another in purposive human communities.

The implications of this view for the liberal democrat are profound, although Royce rarely gives his discussions an explicitly political cast. In *The Philosophy of Loyalty* (1908), which can be read as a brief against a presumption of autonomy and sovereignty on the part of the individual will, Royce also makes it clear that he regards such objects and examples of loyalty as American society does provide to be narrowly utilitarian and spiritually impoverished. "Narrow loyalties, side by side with irrational forms of individualism and with a cynical contempt for all loyalty—these are what we too often see in the life of our country."[34] Royce is particularly apprehensive of the loyalties to "mutually hostile partisan organizations . . . sects, or social classes" that he sees arising, and he regards allegiance to the emerging bureaucratic state as an antidote that is, if not ineffectual, then dangerous. He thus calls for a renewed loyalty to the *local* community, a "new and wiser provincialism" in American life. The province, in Royce's terms, is "any one part of a national domain, which is, geographically and socially, sufficiently unified to have a true consciousness of its own unity, to feel a pride in its own ideals and customs, and to possess a sense of its distinction from other parts of the country."[35] Loyalty to the province thus has a certain immediacy and concreteness; at the same time it is qualitatively different from—and indeed helps one transcend—loyalties grounded in class or partisan interests.[36]

A concern to distinguish among loyalties qualitatively and to provide for their peaceful integration in larger communities led Royce to develop "loyalty to loyalty" as an overarching moral principle: one should choose and pursue one's cause in such a fashion as to honor and enhance the loyalties of one's fellows. It is more immediate loyalties that will "engross and possess" men most strongly, but Royce does not doubt that loyalty at this more abstract level, "making loyalty prosper amongst men," is itself compelling.[37] His later book, *The Problem of Christianity* (1913), makes it clearer yet that loyalty to loyalty does not merely entail liberal tolerance; it is in fact concretized in terms of a "universal community" and clearly elevated to ethical superiority.[38] But Royce did not follow his idealist mentors in turning to the state as the community through which the moral transformation of men was to be accomplished. At the national level Royce rather counseled mutual tolerance and sustenance among provinces and other communities of the loyal. He partially eschewed the liberal-pluralist expectation of a natural harmony among the communities to which men gave their allegiance (the moral dilemma of Robert E. Lee fascinated him), but he still fastened on a renewed provincial life as the only hope for relief from the "harassed mediocrity" and spiritual "estrangement" that he perceived around him. He did not regard the nation as a viable object of men's primary loyalties,

[33] Josiah Royce, *The Philosophy of Loyalty* (New York: Macmillan, 1908), pp. 25–31.

[34] *Ibid.*, p. 229.

[35] "Provincialism," in *Race Questions, Provincialism, and other American Problems* (New York: Macmillan, 1908), p. 61.

[36] See *Ibid., passim; Philosophy of Loyalty*, chap. 5; and Royce's extended discussion of the characteristics of a "true community" in *The Problem of Christianity* (New York: Macmillan, 1913), II, chap. 9–10.

[37] *Philosophy of Loyalty*, pp. 125–126, 180–181.

[38] See Peter Fuss, *The Moral Philosophy of Josiah Royce* (Cambridge: Harvard University Press, 1965), chap. 8; and Bruce Kuklick, *Josiah Royce: An Intellectual Biography* (New York: Bobbs-Merrill, 1972), chap. 11.

and the functional roles Ward and Ross were urging on the state were peripheral to his central ethical concerns.[39]

Herbert Croly, a student of Royce and eventually the best known of the Progressive political publicists, shared his mentor's ethical sensitivities, but the ameliorating community he projected was based on shared values and purposes rather than personal interaction and a localized identity. Moreover, the institutional changes he prescribed were directed not only to Royce's affective and integrative concerns, but also to the practical achievement of economic redistribution and social control. What was needed, Croly declared in *The Promise of American Life* (1909), was *both* "centralization" and "nationalization"—the former a means to Ward's utilitarian ends, the latter a "formative and enlightening political transformation" fostering an idea and an experience of community commensurate with "the whole complex of human powers and interests." Croly categorized "individualist" and "provincial" democracy together as reactionary ideas, which merely served to perpetuate "a policy of inaction enveloped in a cloud of sacred phrases."[40]

"I shall not disguise the fact," Croly tells his readers, "that, on the whole, my own preferences are on the side of Hamilton rather than of Jefferson."[41] By that he does not so much mean to denigrate Jeffersonian ideals—he admires Jefferson as a "radical and unqualified democrat"—as to stress that affirmative and concerted national efforts will be necessary to the continued realization of those ideals. Under changing economic and social conditions, Croly warns, the American promise "instead of being automatically fulfilled, may well be automatically stifled;" if Americans wish to maintain the viability of their "traditional vision," they must be willing to reconsider "the traditional American ways of realizing it."[42] But Croly does not leave it at that: he frankly asserts that the new instrumentalities of collective purpose will require that individual goals be tempered and the "indiscriminate scramble for wealth" restrained:

> The substance of our national Promise has consisted of an improving popular economic condition, guaranteed by democratic political institutions, and resulting in moral and social amelioration. These manifold benefits were to be obtained mainly by liberating the enlightened self-interest of the American people. . . . [But] if the fulfillment of our national Promise can no longer be considered inevitable, if it must be considered as equivalent to a conscious national purpose instead of an inexorable national destiny, the implication necessarily is that the trust reposed in individual self-interest has been in some measure betrayed. . . . The Promise of American Life is to be fulfilled—not merely by a maximum amount of economic freedom, but by a certain measure of discipline; not merely by the abundant satisfaction of individual desires, but by a large measure of individual subordination and self-denial.[43]

Lurking behind such passages is something more than the utilitarian's calculation that individual pursuits must be controlled for the sake of a more equitable overall distribution of goods and opportunities. Croly rather reveals a *qualitative* distaste for American goals in their present incarnation and the resulting "blight" that afflicts the culture.[44] Croly continually asserts that the traditional "Promise" must still be redeemed in urban, industrial America, but, like Royce, he anticipates that self-aggrandizing, materialistic conceptions of that promise will be transcended in the process. Similarly, Croly's support for the pragmatist program of social reorganization and experimentation aims not only at the relief of specific deprivations but also at the cementing of a sense of "joint responsibility" and "brotherhood" which is to be valued in its own right. To foster this quest for "moral" as well as "material" improvement, for a "better society" and a "worthier set of men," Croly turns to a community which Royce saw as holding little ethical promise: the nation.

Croly's positive assessment of the nation as an object—and a "school"—of loyalty reflects both his basic historical determinism and his sense of the historically unique character of the American national spirit. On the one hand, Croly feels that efforts to build communities apart from the concrete ties through which men are already organized and united are futile—and just as consolidation and bigness are not to be escaped in the economic realm, so is *nationality* to be accepted as "the great contemporary political fact." At the same time, nationality is a "political and social *ideal*," and Croly takes particular comfort in the

[39] Royce characterized contemporary agitation for "social control" as an extension of the quest of the willful individual—apart from any loyalty to "the cause of the community"—for power and enrichment (*The Problem of Christianity*, I, pp. 148–159). See also his personalistic prescription for the failure of the "thoughtful public" in America effectively to implement their ideals: "Reform in such matters, must come, if at all, from within." "On Certain Limitations of the Thoughtful Public in America," in *Race Questions*, p. 137.

[40] Herbert Croly, *The Promise of American Life* (New York: Capricorn, 1964), chap. 9.

[41] *Ibid.*, p. 29.

[42] *Ibid.*, pp. 5, 17.

[43] *Ibid.*, p. 22.

[44] On the expression of this distaste in Croly's early editorial work for the *Architectural Record* see Charles Forcey, *The Crossroads of Liberalism* (New York: Oxford, 1961), pp. 18–25.

expectation that *American* nationalism, unlike its continental counterparts, will institutionalize and protect the democratic heritage, preserving, not submerging, "individual distinction" and the free interplay of diverse interests. Despite his yearning for a "higher social relation" and a patriotism of "sufficient substance and organization," Croly in the end reintroduces individual autonomy and social diversity as key values and is left with a rather uneasy synthesis.[45]

Croly's second major work, *Progressive Democracy* (1914) de-emphasizes the theme of nationalism; he gives considerable emphasis, in fact, to the ways in which smaller communities, particularly a democratized workplace, might foster the intellectual and ethical growth and the combined senses of efficacy and interdependence which he deems necessary to national regeneration.[46] Democratic themes are also more prominent in the second book: while Croly is no less convinced that executive and administrative leadership must be underwritten with popular "disinterest" and discipline, he is considerably more concerned to develop means of enlarged participation and democratic control. But Croly's central perception remains constant and in fact underlies his concern for democratization: the crisis which Progressivism must answer is fundamentally one of the spirit:

> Although [democratic political organization] is designed to attain a certain administrative efficiency, its organization for efficiency is subordinated to the gathering of an educational popular political experience. . . . It assumes an intrepid and inexhaustible faith in the value to humanity of an ideal of individual and social fulfillment.[47]

The society's animating faith is to a degree "transcendentalized,"[48] detached from specifically national traditions and symbols, but Croly still sees its propagation as Progressivism's chief goal: "The progressive democratic faith . . . finds its consummation in a love . . . which is at bottom a spiritual expression of the mystical unity of human nature."[49] What continues to distinguish Croly from Ward and his disciples is his sense that control *per se* does not hold the solution to the crisis of liberal society; new structures of human inter-relatedness and "ideal life" are required—to humanize social control, make it work, and give it an ever-expanding sense of possibility, but also to restore the senses of identity and fraternity that have eroded in industrial society.

Relatively subdued in Croly, but more prominent in the thought of John W. Burgess and other nationalists, is the notion of America as a *racial* community, embodying a Teutonic spirit which fosters in men a distinctive identity and a presumably unique set of active virtues. Considerably more interesting and more sensitive in terms of the crisis of liberal society, however, is the use made of the notion of race by an American black trained in German literature and philosophy, W. E. B. DuBois. In *The Souls of Black Folk* (1903), the young DuBois mounts an attack on the leadership of Booker T. Washington and his program of "adjustment and submission." Some of DuBois's criticisms fall well within traditional liberal canons; he deplores, for example, the way Washington has been willing to soft-pedal the question of suffrage and other legal and political rights of the black man for the sake of securing sympathy and support among whites for his program of "industrial and intellectual development." In fact, DuBois insists, progress in the one realm is necessary to safeguard gains in the other. DuBois also shares the Progressive conviction that national power can and must be positively employed to serve liberal ends. "The Freedman's Bureau died, and its child was the Fifteenth Amendment"—and DuBois makes clear that this was a bad bargain for the American Negro. An abstract set of rights, increasingly unenforced, were substituted for what might have been an affirmative program to give the "freedom" of the former slaves meaning and substance. DuBois is quite candid about the Bureau's concrete failings, but he still insists that what was really needed was a *permanent* Freedman's Bureau, "with a national system of Negro schools; a carefully supervised employment and labor office; a system of impartial protection before the regular courts; and such institutions for social betterment as savings banks, land and building associations, and social settlements."[50] Instead, liberal political thought and practice have abandoned the black underclass to

[45] Louis Hartz finds in Croly a "pathetic clinging to Americanism" whereby the vision of a solidary, rationally ordered community was compromised for the sake of assurances that "perfect individualism" would result; his rhetoric became "practically unintelligible when the crucial questions were posed" (*The Liberal Tradition in America* [New York: Harcourt, Brace, and World, 1955], pp. 233). Compare the exposition in Mason Drukman, *Community and Purpose in America: An Analysis of American Political Theory* (New York: McGraw-Hill, 1971), pp. 281–282, 286–293.

[46] Herbert Croly, *Progressive Democracy* (New York: Macmillan, 1914), pp. 264–265, 378–405. But compare p. 197: "centres of association" in society tend to seek not only "mutual accommodation and adjustment" but also "the realization of common purposes. . . . Society comes to be conceived as a whole, with certain permanent interests and needs, into which the different centres of association must be fitted."

[47] *Ibid.*, p. 378.

[48] See Noble, *Paradox of Progressive Thought*, pp. 66–77.

[49] *Progressive Democracy*, p. 427.

[50] W. E. B. DuBois, *The Souls of Black Folk*, in J. H. Franklin, ed., *Three Negro Classics* (New York: Avon, 1962), chap. 2.

he overwhelming forces of economic domination nd racial prejudice.

DuBois's kinship with the particular group of heorists that here concern us, however, rests upon he more fundamental ethical case he wishes to nake against the liberal capitalist order. Its probems are not merely those of distribution and ontrol. DuBois is of necessity concerned that his eople obtain the staples of life, but his most undamental criticism of Washington's program onetheless has to do with its "economic cast"—'a gospel of Work and Money to such an extent s apparently almost completely to overshadow he higher aims of life." DuBois is repelled by the ineteenth century's "deification of Bread," and n an eloquent chapter on Atlanta—who, in her ursuit of prosperity and grandeur, he likens to he mythical Atalanta—he warns the South of the lelusion that "golden apples are the goal of acing and not mere incidents along the way."

Vhat if the Negro people be wooed from a strife for ighteousness, from a love of knowing, to regard dolrs as the be-all and end-all of life? What if to the Mammonism of America be added the rising Mamonism of the re-born South, and the Mammonsm of this South be reinforced by the budding Mamonism of its half-wakened black millions? Whither, hen, is the new-world quest of Goodness and Beauty nd Truth gone glimmering?[51]

While DuBois decries the circumstances that have leprived the Negro of his sustenance and his afety, he finds that this very deprivation has urtured a certain strength of spirit in the race nd, thus far, has made for a degree of immunity o the gospel of wealth. The supreme threat of Vashington's program lies precisely in its willingess to forego the nurturing of this spirit for the ake of material advancement.

In DuBois's subsequent work as editor of *The Horizon* (1907–1910) and of the NAACP's *The Crisis* (1910–1934), he naturally becomes more reoccupied with day-to-day threats to the safety nd dignity of the Negro community. The case gainst lynching, disenfranchisement, and Jim Crow he is generally content to make in the amiliar terms of Christian morality and the merican creed. But when he turns to longerange plans for amelioration, the distinctive ethial hope he holds out for the Negro community eappears. His advocacy of public control of arge industry and of the organization of coperatives among blacks, for example, does not im merely at economic gains:

We must awake! Not in a renaissance among ourelves of the evils of Get and Grab—not in private oarding, squeezing and cheating, lies our salvation, ut rather in that larger ideal of human brotherhood, equality of opportunity, and work not for wealth but for Weal—here lies our shining goal.[52]

Similarly, DuBois's early "separatism," while it represents a strategy for attaining strength and solidarity in the face of continued white hostility and exclusion, also has more positive cultural aims:

If the Negro is to develop his own powers and gifts; if he is not only to fight prejudices and oppression successfully, but also to unite for ideals higher than the world has realized in art, in industry, and in social life, then he must unite and work with Negroes and build a new and great Negro ethos.[53]

DuBois is rather vague concerning the form this ethos might take or how distinctive it would remain under more favorable social and political conditions, but he leaves little doubt that it would continue to represent a negation of acquisitive individualism. The chief mechanisms of its transmission are to be a highly trained black intelligentsia, an educational program that places "the ideal and the practical in workable equilibrium," and economic institutions that provide everyday training in cooperation and social service. In the long run, DuBois hopes, the Negro ethos will transcend racial boundaries, serving as a leaven in the national community. Thus might the future interaction of the races make for the survival in American life of that which is truly "fit," preserving "for future civilization . . . not greed and impudence and cruelty [but] all that is really fine and noble and strong."[54]

It was John Dewey who gave to these various themes of Progressive political thought their most detailed and integrated exposition. While it is impossible to do justice to the subtlety and complexity of Dewey's thought in brief compass, it is a relatively simple matter to place him in the broad tradition under discussion here, for he, more than any of the other communitarians, developed his social theory in direct and explicit response to contemporary liberal thought and practice.

Dewey echoes Royce in regarding the erosion of the *loyalties* which once gave individuals "support, direction, and unity of outlook on life" as the age's central moral problem and in judging national chauvinism, mass conformity, and the selfish pursuit of class interests to be pathological substitutes for the solidarity, meaning, and common effort men once experienced in their "daily communities." Nor is Dewey completely free of Royce's nostalgia: "To 'restrain' or turn back the industrial revolution and its consequences," he acknowledges, "would be efficacious if only it

[51] *Ibid.*, pp. 246, 264, 266.

[52] *The Horizon*, February, 1907, p. 8. Compare *The Crisis*, 2 (June, 1911), 64–65; and 9 (April, 1915), 310–312.

[53] *The Crisis*, 17 (January, 1919), p. 112.

[54] See *Souls of Black Folk*, chap. 9.

could occur." But in the end he asserts that the traditional mix of provincial and other-worldly allegiances is no longer viable, that the moral life must find new substance and a new setting. This conclusion is reinforced by the judgment—which eventually overshadows the other elements in his critique of liberalism—that the problems of the age are of a new order, *requiring* new modes of social organization, intelligence and control. The conditions rendering the individual spiritually and too often materially impoverished are "general, not local. They concern complex forces that are at work throughout the whole country. . . . Traditional ideas are more than irrelevant. They are an encumbrance." Dewey, like Croly, takes contemporary economic and technological forces as given: "Political *control* is needed, but the *movement* cannot be arrested. . . ."[55]

It is for the sake of such control that Dewey prescribes the collective self-discovery of the "public"—defined in orthodox liberal fashion as those who have a shared interest in controlling the side-effects of otherwise private transactions—and the concomitant strengthening of its agent, the state. Dewey is anxious lest the state transcend this instrumental role; he distinguishes his view from the Hegelian in much more decisive terms than do Cooley and Croly. But Dewey also insists that there be no predetermined set of "inherent" and absolute limitations—such as the common view that the state is merely "an umpire to avert and remedy trespasses of one group upon another."[56] His insistence is prompted in part by a growing sense of the extensiveness of the measures which any effective "diking and channeling" of economic and social forces will require. But never far from Dewey's mind is his perception of the threat which the new order poses to the human agency and efficacy presupposed by the democratic idea. It is not enough simply to erect structures that will prevent rapacious behavior and provide the material necessities, for the crisis of the public runs deeper than that. What is crucial is that men have lost the *capacity* they possessed in more provincial settings to perceive and responsibly to shape the forces acting upon the community. "The local face-to-face community has been invaded by forces so vast, so remote in initiation, so far-reaching in scope and so complexly indirect in operation, that they are, from the standpoint of the members of local social units, unknown."[57] The forces of "electricity and steam" have created the "Great Society," characterized by economic interdependence, nationwide organizations and systems of communication, and cultural homogeneity. What remains is the distinctly human, purposive task of making this a "Great Community"—"the perfecting of the means and ways of communication of meanings so that genuinely shared interest in the consequences of interdependent activities may inform desire and effort and thereby direct action."[58] It will be a massive undertaking, and it is important that human vision and agency not be artificially restrained in advance.

Dewey, like Rousseau, thus tends to telescope the meanings of "community" and "democracy"; in seeking to recapture the essence of community life, he seeks first of all a sense of efficacy, mastery, and self-direction for its constituents. His attempts to develop a "method of intelligence" adequate to purposive social intervention and the kind of education that could underwrite and guide its application have this qualitative end in view: to make of the Great Society a truly democratic, self-aware, self-determining community. Dewey thus goes considerably beyond his contemporaries in recognizing the importance of personal responsibility and political efficacy—as well as "spiritual" solidarity—to the life of the provincial community and in thinking imaginatively about how they might be restored within and among the distended "publics" of industrial society.

He does not anticipate, however, that individuals and "individualism" will remain unchanged in the process. The Great Community will not merely give men the means of securing antecedent interests; participation will also have a "reactive" effect, increasing their sense of mutual responsibility, broadening their horizons, "clarifying their aims and purging their activities."[59] "Renascent" liberalism, Dewey makes clear, will aim at a personal "liberation" from merely utilitarian quests; economic activity, he asserts, is the "least significant manifestation" of human "initiative, vigor, and independence." The social order, in contrast to Lockean "society," will be "possessed of a spiritual authority that will nurture and direct the inner as well as the outer life of individuals."[60] Dewey remains rather

[55] See John Dewey, *Individualism Old and New* (1929); reprinted, New York: Capricorn, 1962), pp. 37, 52, 68, 93, 119, 158. Italics added. On the criteria for distinguishing "desirable" from inferior forms of community life, see *Democracy and Education* (1916; reprinted, New York: Free Press, 1966), chap. 7.

[56] John Dewey, *The Public and its Problems* (New York: Henry Holt, 1927), p. 73. But note Dewey's own earlier likening of the state to an orchestra conductor "who makes no music himself but who harmonizes the activities of those . . . producing it" (*Reconstruction in Philosophy* [New York: Henry Holt, 1920], p. 203).

[57] Dewey, *Public and its Problems*, p. 131.

[58] *Ibid.*, p. 155.

[59] *Ibid.*, p. 71.

[60] Dewey, *Liberalism and Social Action*, pp. 30, 38, 88.

vague about the criteria the Great Community might employ in seeking "growth" for all and in placing a "discount" on the "injurious";[61] he clearly remains close to liberal assumptions both in the ends he posits for the new order (a "flowering of human capacities") and in the harmony and complementarity of interests he anticipates among the "liberated." But his conceptualization of individuality in terms of self-development rather than "negative" autonomy and privacy enables him to see the community as a nurturing rather than threatening force, to see the system bred by *economic* individualism as a threat to individual "freedom" rather than its logical extension, and thus to begin to transcend some of the familiar antinomies that have constricted liberal thought.[62]

It is also Dewey who, among the thinkers we have considered here, comes closest to perceiving the multiplicity of values touched by the loss of community and the multiplicity of institutions that might be necessary to their recovery. Dewey, unlike Croly, sees that the "Great Community," however successful it may be in turning private pursuits to social benefit, however it may increase the efficacy and enrich the interaction of its constituent persons and groups, can never fully replace the local, face-to-face community. "Vital and thorough attachments are bred only in the intimacy of an intercourse which is of necessity restricted in range. . . . The local is the ultimate universal, and as near an absolute as exists."[63] For Dewey, no less than for Royce and Cooley, the crisis of liberal society is a moral one, and it is only through a revitalization of contiguous relationships that men can achieve the rootedness and fellow-feeling necessary to overcome their isolation and narrowness of purpose. Nor can any truly liberating *social* reconstruction proceed unless it is accompanied by a renewal of these wellsprings of identity and common aspiration: the two levels of community, while speaking to different impulses and needs, are also highly interdependent. Dewey, to be sure, is ambivalent and not entirely consistent on this question: his references to communal "stagnancy," "conformity," and "isolation" leave little doubt that the Great Community's work will aim at more than mere sustenance or restoration; it rather "will do its final work in ordering the relations and enriching the experiences of local associations." But Dewey, quoting Cooley, remains convinced that it is the face-to-face community that "really counts" in forming durable "emotional and intellectual dispositions" and that apart from the restoration of community at this level, the public cannot hope adequately "to find and identify itself."[64]

[61] See the rather general discussion in *Reconstruction*, pp. 197–198.

[62] The conceptual distinctions developed by Steven Lukes are helpful here: *Individualism* (New York: Harper, 1973). For an argument that Dewey, in effect, overcompensated, reducing "meaning to its public context," failing "to do full justice to man's inner life," see John E. Smith, *The Spirit of American Philosophy* (New York: Oxford University Press, 1966), p. 129. Considerably more sensitive on this score is Royce, whose "whole philosophy could be interpreted as the story of the cooperation and tension between individual and community" (*Ibid.*, pp. 91–98).

[63] *Public and its Problems*, pp. 212, 215.

Contemporary Reverberations and Implications

Louis Hartz and other historians have too often maintained their monolithic portrayal of America's "liberal tradition" by ignoring the communitarian strains in Progressive political thought.[65] It must be acknowledged, however, that the contradictions and difficulties of the Progressives and other theorists of the period in a sense bear Hartz out; that is, they demonstrate how difficult it is to apply the insights of Burke or Hegel or Tönnies in America. That the environment provides little in the way of ideological or social-structural reinforcement is also suggested by the subsequent fate of the communitarians' ideas. Ward's and Ross's notion of the national community is the one that has proven most durable; Croly's estimation of the impact of his ideas on Theodore Roosevelt was considerably exaggerated,[66] and he seemed positively mystifying to the architects of the New Deal. More telling yet is the fate of the pluralism of Royce, Cooley, and Dewey. "Group theorists" such as Arthur Bentley and David Truman have drawn heavily on Baldwin's and Cooley's insights about the group-relatedness of individual articulations of right and interest, and many analysts have followed Dewey in regarding group organization as a crucial means to broadened participation and political efficacy in the American system. But the "interest group" is hardly what the Progressives had in mind. Subse-

[64] *Ibid.*, pp. 97, 211, 216. For an argument that Royce, in his later writings, was approaching a "multitiered theory of the interpretational community as a promising solution . . . [to the] time-honored problems of political thought," see Peter Fuss, "Interpretation: Toward a Roycean Political Philosophy," *Revue Internationale de Philosophie*, XXI, 79–80 (1967), pp. 120–131.

[65] See Hartz, *Liberal Tradition*, ch. 9. A needed corrective is provided by Wilson Carey McWilliams, who finds a continuing, if often frustrated, search for "fraternity," with the sense of emotional security and "assurance of identity" it brings, in American political thought and movements. McWilliams's portrayal of the Progressives, however, is hardly more differentiated than Hartz's; they remained fixated, he suggests, on "the fraternity of race and blood and the fraternity of national 'mission.'" See *The Idea of Fraternity in America* (Berkeley: University of California Press, 1973), chap. 4, 17.

[66] See Forcey, *Crossroads of Liberalism*, ch. 4.

quent American pluralist thought has tended to ignore the communal functions of the political "province" or association—its nurturing and extension of men's moral awareness, its awakening of loyalty and existential commitment, its furthering of social solidarity, discourse, and common effort. The tendency has rather been to transfer private material quests from the individual to the "group." Moreover, pluralist analysis has sometimes gone on to undermine the legacy of Ward and Ross as well. Liberal assumptions have been reintroduced, in other words, not only with regard to the *ends* of political association but also through expectations of harmony, equity, and adaptive "adjustment" among contending groups. Political leadership and social control have thus been reduced to brokerage functions: "balancing" interests, maintaining consensus, safeguarding "the rules of the game."

It is thus arguable that insofar as the realities and the aspirations of American political life conform to the pluralist paradigm, the critical thrusts of Progressive political thought still confront it in a dialectical fashion. Certainly present-day critiques are often remarkably close in substance and in spirit to the commentaries of the Progressive period. Equally striking, however, is the extent to which the ambivalences, contradictions, and lacunae of Progressive thought also remain. In conclusion, then, it will be useful to take a summary view of some of the problems on which the social critics of the turn of the century focused and to point up some of the continuing dilemmas which their treatments raised.

Implications for Politics. What, in the first place, marks the liberal political community as a defective one? We have suggested that social critics of the period tended to focus *either* on the conflicts and inequities and social drift attributable to the theory and practice of *laissez-faire or* on the individual isolation, acquisitiveness, and "homelessness" that supposedly flowed from the liberal-capitalist sanctification of life, liberty, and property as the primary ends of social and political life. Critics such as Croly, DuBois, and Dewey bridge the dichotomy in various ways, and *within* each cluster of theorists there are important distinctions to be drawn. Communitarians differ, for example, in the relative stress they give to particularized *identities* and common *purposes* as the bases of community life, in their willingness to see "utilitarian" quests as compatible with (or even as a source of) communal loyalties, and so forth. Surely further analytical refinements are in order. But the basic distinctions drawn here amply demonstrate that efforts to understand or appropriate theories coming out of the Progressive period are poorly served by treatments of "community" as a panacea or an undifferentiated concept. To understand its critical thrust and the ameliorative structures and policies it implies requires a discerning examination of the presuppositions and contextual elements governing its use.

To what extent are social maladies to be perceived and solutions projected in *political* terms? Such a question is bound to be vexing, given the disparities and inhibitions within the liberal tradition as to the scope and character of politics. If one construes politics in the Weberian mode, focusing on the statewide "enforcement of its order" by a government which "successfully upholds a claim to the monopoly of the legitimate use of physical force,"[67] it is Ward and Ross and —insofar as Croly and Dewey stress the policies and techniques the "Great Community" will underwrite—the nationally oriented communitarians whose "political" thrust is most conspicuous. This despite the fact that all play down the Weberian theme of coercion and assume an ultimate harmony among the interests upon which the state will impose its "order."

The political implications of the communitarian critics become clearer in light of the less formalistic definitions which modern theorists give to politics. When one's conception of the "political" encompasses relationships of power and authority that transcend the realm of government,[68] the political importance of community organization and collective action becomes manifest. Today, amid cries for "community control" and widespread perceptions of the isolation and impotence of mass man in the face of "remote power,"[69] it is common to think of the community as an instrument of individual and group empowerment, providing means for control of the economic and political environment and for effec-

[67] Max Weber, *The Theory of Social and Economic Organization*, translated by A. M. Henderson and Talcott Parsons (New York: Free Press, 1964), p. 154.

[68] Robert A. Dahl, *Modern Political Analysis* (Englewood Cliffs: Prentice-Hall, 1970), p. 6.

[69] Note the intermixture of themes in such otherwise dissimilar works as Robert A. Nisbet, *Community and Power* (New York: Oxford University Press, 1962) and Stokely Carmichael and Charles V. Hamilton, *Black Power: The Politics of Liberation in America* (New York: Random House, 1967). Nisbet's mode of social analysis of course has easily discernible nineteenth-century roots, but it is nonetheless instructive to note the tendency of Royce, whose analysis of mass society is easily the most prescient among the American communitarian theorists of the period we have examined, to translate the "crushing" of the individual by "economic and industrial developments" and other "leveling tendencies" in modern civilization into a problem of *spiritual* estrangement, the loss by the individual of "all sense of his unique moral destiny" (Royce, "Provincialism," pp. 74–76 and *passim*).

tive representations (or resistance) to "higher" authorities. The earlier communitarian thinkers, reacting to a political culture that reduced the politically relevant ends of man to the utilitarian and the crudely chauvinistic, often countered with an equally exaggerated "spiritual" view of fraternization. Particularly in Dewey and DuBois, however, one finds the beginning of a synthesis—a recognition that the pursuit of power and interest play a critical role in community formation and maintenance, that association is a means as well as an end in itself.

Similarly, it is in Dewey that one finds the closest approximation of that view of political life recently articulated anew by Hannah Arendt: politics is that realm of *collective* power where free men join together publicly for "speech" and creative "action" directed at the achievement of common ends.[70] Most of our theorists, it is true, share in what Arendt regards as the typically modern view of politics as the domain of *animal laborans*, meeting man's "needs" as producer and consumer, more akin functionally to the Aristotelian "household" than to the *polis*.[71] This is made explicit in the utilitarianism of Ward and Ross, and no less in communitarians such as Royce and Cooley, although it becomes grounds for the disparagement of contemporary politics in the latter cases. But Dewey, Cooley, and occasionally others among the communitarians also capture a glimmer of the Greek ideal, whereby the public space is reclaimed from the powers of production and consumption and men enter politics, not simply to safeguard particularistic power and interest, but to debate collective goals, undertake common enterprises, and distinguish themselves in seeking freedom and vitality for the whole.

If the social critics of the Progressive period frequently caught only a glimmer of the political implications and possibilities of their own work, they nonetheless loosened the bonds that constrained—and still constrain—the liberal discussion of politics. It was the theorists of social control who pointed up the ubiquity of power and conflict in Lockean "society" and the potential of *governmental* power, not as a threat *sui generis*, but as a countervailing and ameliorating force. But it was the communitarians who penetrated more deeply, questioning the conceptual and ethical adequacy of a notion of society's foundations in terms of autonomous individuals and a voluntaristic contract. Liberalism had assumed that men's articulations of their rights and interests, and their divising of means of association and exchange as well, were "natural," i.e., prepolitical. Political organization merely put a seal on these pre-existing ends and arrangements, safeguarding them but adding no substantive ends of its own. The communitarians rightly saw that the theorists of social control took too much of this for granted—equating the common good with new modes of aggregating and implementing "pre-existent" interests, ignoring the kinds of policies and structural arrangements that might put human goals and relationships on a less individualistic and materialistic footing. Cooley and those who shared his perceptions, by contrast, saw that the "wholeness" of man as he came to politics was not a "given" but a problem, and that even if a generalized desire for life, liberty and property could be assumed, these hardly exhausted the ends of community life. But the temptation was strong to avoid the theoretical reconstruction to which these perceptions might have led: while some sought to "enrich" liberal institutions virtually by fiat (Croly), others, having perceived the impoverishment of public life, turned to means of spiritual and social compensation that, in effect, encouraged the abandonment rather than the transformation of politics (Cooley, Royce).

If such escapes fail to do justice to men's stake in and responsibility for the commonweal, the current tendency to regard all of life as "politicized" is likewise oversimple. The liberal view of autonomous man and instrumental communities is inadequate, but thought must still be given to how men's needs for loyalty, solidarity, and collective purpose relate to the traditional rights, interests, and immunities the liberal state has set out to protect (or has simply left alone).[72] The

[70] Hannah Arendt, *The Human Condition* (Chicago: University of Chicago Press, 1958). See also Joseph Tussman, *Obligation and The Body Politic* (New York: Oxford University Press, 1960), and Robert Paul Wolff, *The Poverty of Liberalism* (Boston: Beacon Press, 1968), chap. 5.

[71] See Arendt, *Human Condition*, pp. 28–37, 305–325. Feminists of the Progressive period argued that the participation of women in politics was necessary and justified precisely because their traditional household concerns had become the business of the state. "If women follow only the lines of their traditional activities here are certain primary duties which belong to even the most conservative women, and which no woman or group of women can adequately discharge unless they join the more general movements looking toward social amelioration through legal enactment." Jane Addams, "Why Women Should Vote" (1910), in Christopher Lasch, editor, *The Social Thought of Jane Addams* (New York: Bobbs-Merrill, 1965), p. 145.

[72] For a compelling argument as to the continuing moral force of these claims and the concrete forms they might take in the "administrative state," see Charles Reich, "The New Property," *Yale Law Journal*, 63 (April, 1964), 733–787. Benjamin R. Barber's fascinating study of the history of a Swiss mountain canton suggests that the view of "freedom" as an absence of constraints, the assumed opposition of the power of the individual and of the collectivity, and an instrumental and defensive view

American communitarians felt this acutely and thus presented their theories as a means of preserving and enhancing the freedoms and institutions of civil society rather than "transforming" or "transcending" them. They felt enormous constraints not to succumb to an organic, statist (and often misperceived) "Hegelianism," though it was their common intent to transcend the "antithesis . . . between society and the individual" posited by liberalism.[73] Similarly, the communitarians were reluctant to divorce political leadership and public policy from the perceived and articulated "wants" of the populace, although several of their arguments set them apart from "want-regarding" theory in its conventional liberal guise: they did not regard all wants as equally legitimate or deserving of public support; they were aware that wants, far from being autonomous, were in fact dependent on economic and political circumstances; and they welcomed the prospect of men debating and implementing their publicly-oriented "ideals" in the political arena.[74]

The problem, of course, is not merely the supposed threat community poses to liberal "wants" and rights; there is also the question of their interdependence. As noted above, the communitarians dealt rather imprecisely with the positive relationship of the quest for power and interest to community formation. But it is important not to take one's frame of reference entirely from those modern theorists who identify politics as the realm of power and domination, for the communitarian theorists raise questions that are all too easily missed in current discussion. There are, after all, as Royce, Cooley, and Dewey variously insisted, crucial qualitative differences between a "group" seeking to secure particularistic power and interest and a community of the "loyal." The needs and aspirations they address—and the notions of "politics" they embody—diverge and often conflict. If the communitarians tend to sentimentalize the association and thus to neglect its potential relevance for liberal politics, they nonetheless have something to say to those contemporary thinkers, whether from the radical left or the pluralist "center," who in their secularizing and instrumentalizing of the community would concede far too much to the liberal-utilitarian view of association and political life. It is a mistake which Dewey and DuBois would not have made, although they admittedly provided only scattered suggestions as to how one might begin to sort out and interrelate the ends of community life and the associational and administrative arrangements required for their realization.

The Ends of Community: Lingering Dilemmas. The question of the ends and "levels" of community thus remains an important and difficult one. Present-day critics, like those of the Progressive period, tend to define the crisis of liberalism in radically divergent terms and in the process to talk past one another. Theodore Lowi, for example, who might be regarded as a modern heir of Ward and Ross, urges an abandonment of the pluralistic reincarnation of market politics and a return to the "formalism" whereby social goals are clearly specified and law and administration are authoritatively directed to their achievement.[75] Across the chasm, meanwhile, Philip Slater portrays less individualistic and achievement-oriented modes of child-rearing and socialization as the only means to free the society of debilitating and socially destructive "loneliness."[76] Some worthy attempts at bridge-building have recently appeared, but their success is mixed. Robert Dahl, for example, posits a multi-tiered model of democratic organization as an antidote to the governmental illegitimacy and unresponsiveness that are the targets of current protest. The welfare-state liberal's fixation on the central government and the radical's quest for direct democracy via neighborhood "control" are both fatally inflexible; what is needed is a frank realization that all-purpose modes of political association and organization are not to be found, that the scope and character of participation and

of political participation are not written in the stars, but neither are they likely to be completely transcended in the thought and experience of men shaped by the history and political culture of liberalism (*The Death of Communal Liberty* [Princeton: Princeton University Press, 1974], pp. 3–9, 134–138).

[73] Dewey, *Public and its Problems*, p. 191; see also his discussion in *Reconstruction*, chap. 8. For an interesting modern treatment, which draws heavily on Royce in its grounding of liberal freedoms not in the individual's "autonomy" but in his overlapping (and concentric) "faiths," see H. Richard Niebuhr, *Radical Monotheism and Western Culture* (New York: Harper and Brothers, 1960), chap. 5.

[74] The terminology is adapted from Brian Barry, *Political Argument* (London: Routledge and Kegan Paul, 1965), chap. 4. For a discussion of the dilemmas posed for Dewey as a social reconstructionist by his liberal-democratic scruples and his philosophical antidogmatism, see Drukman, *Community and Purpose*, pp. 342–352.

[75] Theodore Lowi, *The End of Liberalism* (New York: W. W. Norton, 1969). Lowi's follow-up volume, *The Politics of Disorder* (New York: Basic Books, 1971), pays more explicit attention to the "social movements" that underwrite the regime of "formalistic democracy and the rule of law." The focus, however, is on participation as an *instrument* for inducing political change in accordance with preexisting wants; even his discussion of the hypothesized *gemeinschaft* "stage" of organizational development refers to individuals "finding," "discovering" common interests and irritants (pp. 36–43).

[76] Philip Slater, *The Pursuit of Loneliness* (Boston: Beacon Press, 1970).

authority must depend on the specific needs and functions being addressed. "Stages of government fitting together rather like components of a Chinese box are necessary," Dahl says, "if 'the people' are to 'rule' on matters important to them, whether a neighborhood playground, water pollution, or the effective prohibition of nuclear war."[77]

Dahl's apparent synthesis of the communitarian and social-control perspectives is incomplete, however, for the criteria he would apply to community formation and organization have little to do with the affective and ethical concerns expressed by the contemporary critics of liberalism and their communitarian forebears. Dahl rather adopts a radically individualistic posture, assuming "personal choice" and preference to be autonomously articulated, prior to politics. The mode of participation and authority obtaining at any level is to be assessed merely in terms of its effectiveness in implementing these preferences. Thus Dahl assumes that the crisis of liberalism is one of preferences frustrated and "choices" unmet; his discussion of levels of community simply talks past those whose primary concern is to broaden and humanize those preferences through new modes of communal identity and interaction.[78]

Wilson Carey McWilliams, by contrast, makes the fundamental human need for "fraternal relations" the basis of his critique of liberal thought and practice. He is aware of the danger that the quest for fraternity may leave broader questions of right and justice unattended—"that men might create a place for themselves within the existing order without changing it, especially a nook of private gratification which helps conceal the knowledge of public indignity."[79] But his own treatment of fraternity tends to widen the gap between the quests for justice and for community and to remove the latter from politics. Fraternity, McWilliams insists, is based on *both* "intense interpersonal affection" and "shared values and goals considered more important than 'mere life'."[80] This means that "civic fraternity" is impossible in the modern state; in fact, McWilliams echoes the familiar liberal fear that efforts artificially to induce it are dangerous. "The best, or rather the safest, approximation of justice is procedural rather than substantive, limited to external conduct, and leaves the development of man's justly fearful spirit to others than the state."[81] But to see the tasks of justice as "procedural," related to the "external" man, is to declare them peripheral in terms of McWilliams's core values. "Compassion will probably forbid" *discarding* modern society, he acknowledges, crucial as its "power and resources" are to the well-being of millions. But surely McWilliams's willingness not to "discard" mass society and its organs of distribution and control offers little comfort to those deprived and disadvantaged by the current workings of the system or, for that matter, to those who would seek for new standards of political responsibility and purpose.[82]

[77] Robert A. Dahl, *After the Revolution? Authority in a Good Society* (New Haven: Yale University Press, 1970), p. 93. Dahl's argument for consolidated jurisdictions and delegated central authority, while issue-specific and anything but doctrinaire, still partakes of the spirit of Ward and Ross and represents something of a departure, it would seem, from the celebration of "minorities rule" to be found in his earlier *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956). "Our debates about economic institutions," he laments, "nearly always leave some major alternatives—chiefly 'socialist' alternatives—unexplored" (*After the Revolution?*, p. 119). Yet he still seems to conceive of the public interest in terms of a straightforward implementation of private preferences and to anticipate a harmonious equilibrium between narrowly-based groups and communities—the instruments of "individual effectiveness" (p. 99)—and the instrumentalities of the broader public. The compatibility of pluralist politics with "collective effectiveness" is thus not fundamentally questioned.

[78] Dahl's suggestion that a matter "best dealt with by democratic association" should be handled "by the smallest association that can deal with it satisfactorily" seems to be partially rooted in a sense that human interaction and socialization are qualitatively different in "units of human proportions" (*Ibid.*, pp. 102, 165). Yet his discussion of the individual's "wish to live among others" (p. 9) is devoid of the language of dependence or identification, and he defends his preference for smaller units of association mainly in terms of "individual effectiveness" and economy vis-à-vis the attaining of preferences.

[79] McWilliams, *Idea of Fraternity*, p. 621.

[80] *Ibid.*, p. 7.

[81] *Ibid.*, p. 74. *Local* groups and governments, McWilliams acknowledges, are "small enough to be meaningful to the individual" and thus might play "a far more immediate role in our security and emotional life than those to whom we are tied by abstractions." However, McWilliams foregoes any extended examination of the potential of these units as organs of fraternity, conceding more than one would anticipate, given the balance of his argument, to the "functionalist" contention that their viability is severely reduced because "all the great events of political life cross the boundaries of local areas and defy control by them" (*Ibid.*, pp. 69–70). Compare Dahl, *After the Revolution?*, pp. 64–67, 85–98.

[82] McWilliams, *Idea of Fraternity*, p. 622. It is not, of course, McWilliams's *intention* to "offer comfort" to a society where he feels the discovery of fraternity on any large scale is impossible and where, at the same time, attempts to return to a simple social order are subject to serious moral and practical objections. Neither, however, does he wish to belittle the "decencies and humanities" of the New Deal and other welfare-state reform efforts, however deficient their "compassion" and "impersonal condescension" are in terms of the fraternal ideal (*Ibid.*, chap. 19).

McWilliams insists that the quest for fraternity must take place in the interstices of modern society. His suggestion that "communities and fraternities" should be made "more possible" is not without policy implications, but he portrays the rediscovery of fraternity as primarily a matter of individuals "setting examples," making the most of "chance events of meeting and moment," "recognizing one's fellow citizens when chance casts them in the way."[83]

The Progressives' dilemmas are thus still with us. These dilemmas, however, while genuine and persistent, are perhaps not totally intractable; certainly it is important that contemporary critics of liberalism pay renewed attention to how their perceptions intersect and to the relationships their projected patterns of community organization and interaction bear to one another. To clarify the meanings the "quest for community" assumes in our present discourse would be to understand more fully the hard times upon which liberal politics has fallen, and perhaps to conceive of the directions from which renovation and renewal might come. Equally valuable, however, would be a renewed sense of what politics *cannot* accomplish—by virtue either of incompatibilities among our critiques and goals or of the character of the goals themselves. For if our vision needs to be lifted, if we must go beyond liberal notions of the ends and means of political life, we also need a fresh sense of the limitations and contradictions of that realm and of whatever renovations we undertake. The creative and constructive potential of the idea of community in American political thought is enormous, but it still does not promise salvation.

[83] *Ibid.*, pp. 93–94, 622–624. "The book," McWilliams writes, "is really concerned to open an area for discussion—to 'set a stage' for policy suggestions, rather than to make them [itself]. . . . Certainly, measures for greater 'community control,' neighborhood stabilization, etc. would rank very high on the list" (personal communication, February 27, 1974). For further hints along these lines, see McWilliams's discussion of the ill-fated Subsistence Homestead and Farm Security programs of the 'thirties (*Idea of Fraternity*, p. 550).

COMMUNICATIONS

TO THE EDITOR:

Professor Bruell's thesis regarding "Thucydides' View of Athenian Imperialism" (*APSR*, 68 [March, 1974], 11–17) can be summarized in the following two propositions:

(1) wrong does not properly exist for the mighty in matters of foreign policy;
(2) while necessity is the mighty's proper concern in foreign policy, this necessity must derive from a consideration of interest or advantage rather than from anger or revenge.

The impact of these propositions generates the following synthesis: "Justice is the advantage of the stronger," with the qualification "in matters of foreign policy." The significance of these considerations involves the loss of a status for justice separate from interest or advantage in matters of foreign policy. Professor Bruell is correct: if this transformation in the normal (i.e., conventional) meaning of justice occurs, then (in Thucydides' view) we can no longer speak sensibly of the mighty doing "wrong" in foreign policy. Such a statement to be used at all must only apply when the mighty are stupid or make a mistake; then the mighty merely do "wrong" by improperly calculating utility. The necessity of the mighty remains to do whatever is in their interest whether that violates conventional justice or not. I contend that such an understanding of Thucydides' view of foreign policy is too simple.

I agree with Professor Bruell that comprehension of Diodotus' speech (III, 41–48) is central to unraveling this problem. Diodotus' name does mean "Gift of Zeus"; moreover, Thucydides introduces him as the son of Eucrates, a name which means "one who is properly strong" or "he who prevails well" (III, 41).[1] Thucydides here also tells us that Diodotus in the earlier meeting of the *ecclesia* had been the principal speaker against serving the death penalty upon the Mytilenians. At that time he had lost to Cleon. Professor Bruell is correct, the Athenians were angry, they acted in haste. However, only after the Athenian *demos* had begun to reconsider that their action had been savage and brutal were the Mytilenian envoys in Athens and their Athenian supporters able to again put the question before the Athenians (III, 36.4–5). Diodotus is not specifically credited with having introduced this repentant feeling among the Athenian *demos*. Rather, he is able to reintroduce thoughtful discourse only after the Athenians of their own accord have calmed down. More is suggested than merely that the Athenians were ready to make the mistake urged by Cleon. The sequence of these meetings leads to the conclusion that the necessary compulsion of anger and revenge was stronger than the necessary compulsion of interest and advantage. The Athenian *demos* does not want to reconsider the question because they have found a more advantageous solution to the problem, rather they are experiencing shame, that traditional psychological concomitant of justice. The consideration of genuine interest and advantage does not cause the anger of the *demos* to dissipate, rather these considerations depend upon that anger subsiding. If the necessary compulsion of anger and revenge is the stronger of these two principles, then it seems most paradoxical to argue that foreign policy predicated upon the mighty should depend for its correctness upon the weaker principle.

Nor should we necessarily conclude that the question of interest and advantage is the total point of Diodotus' argument. Professor Bruell properly notes that Diodotus alerts his audience to his intention to deceive them. But what is the nature of this deception? At the point in his speech where Diodotus claims the concern in the argument about the Mytilenians must be *advantage*, he distinguishes between this type of deliberation and a lawsuit where the guiding principle is justice (III, 44.4). Diodotus suggests, then, that justice's proper sphere involves legal suits and domestic matters. Diodotus' major argument of utility is the following: If the Athenians impose the death penalty upon the Mytilenians and leave no lesser alternative of penalty, then why should any city in revolt from Athens capitulate until the last extremity of resistance has been exhausted. Athens in the process of stopping revolts will be left with ravaged cities which can neither provide revenue nor arms in the future (III, 46.2–3). He ends this point by returning to the judicial metaphor: the Athenians must not be such strict judges as to bring harm upon themselves, rather the Athenians should here punish moderately (III, 46.4). The deception seems to be that Diodotus has introduced the conventionally just solution under the guise of advantage.

But there is still more significance here. Diodotus has considered the good citizen, the good speaker, and the wise city (III, 42.5); he has discussed the question as it should be discussed by men who are being wise (III, 44.2). The wise course, the good counsel, seems not to treat allies as enemies as Cleon has suggested (III, 40.8), but

[1] For a comparative instance of the word "κράτος" but without the prefix "εὖ," see V, 105.2.

to treat enemies as allies whenever possible. The last point of Diodotus' argument of utility emphasizes the extent to which the populace of Mytilene remained allies in an oligarchic revolt. The point of the undifferentiated death penalty Cleon advocated would have been to push the populace to the oligarchs in any revolt, making every city in revolt a city of enemies (III, 47). Justice, then, in its most conventional sense must temper the wise foreign policy even when that policy is suffering the agonies of internecine warfare during full-scale war. An Athenian foreign policy which failed to include any moderation by justice if carried to its logical extreme would entail not only the destruction of the Athenian alliance but also perpetual warfare until all enemies submit. Subjugation by one side or the other must ensue. Peace which would require a sort of domestic policy of international law would be impossible. Yet while Thucydides refuses to call genuine the Peace of Nicias (V, 26.2–3), in which no genuine arbitration of disputes between Athens and Sparta took place, he makes no similar claim for the thirty-year truce.

If justice is only the advantage of the stronger, as the Athenians in Sparta assert (I, 76.2–4) and the Athenians at Melos insist (V, 105.2), then the slaughter at Melos is required once the Melians refuse to submit. The precise point of the Melian dialogue is that the Athenians so arguing cannot have the friendship of neutrals in foreign policy (V, 95). If these admittedly weak islanders[2] do not submit, the Athenians fear the breakup of their island empire (V, 97). The Athenians do not listen to the Melian argument that refusal to grant a neutral status to Melos will strengthen present enemies and generate new enemies for Athens (V, 98), an argument which strongly resembles Diodotus' argument concerning the original Mytilenian decree. The Athenians on Melos are even more extreme (or, perhaps only more blatant) than the Athenians in Sparta; there is no gentleness to their foreign policy (compare I, 77.1–5 with V, 89). At Melos, the Athenians demonstrate that only power and fear hold together their alliance; Thucydides himself has discussed the dire consequences of governing by these principles (compare II, 8.4–5 with III, 82.8 and III, 84).[3]

[2] Here compare the Athenians' typification of the Melians relative weakness (V, 97) with Thucydides' description of Mytilene as a relatively weakly controlled allied *polis* (III, 36.2). Neither Cleon (III, 39.2) nor the Athenians at Melos (V, 94 and 95) can consider independence or neutrality of the relatively weak *polis* in contention. Is this because of the relative weakness of the Melians and the Mytilenians or because of the relative weakness of the Athenians at Melos and of Cleon? I contend the former is only a threat because of the latter.

[3] It is also interesting to compare the reversals of conventional meanings in the Melian dialogue (e.g., V, 111.3 with the word "*αἰσχρόν*") with Thucydides' discussion of the conditions for and the process of perversion of normal language meanings (III, 82.4).

Diodotus is properly strong because he understands how conventional justice not only can continue to exist even under an imperial foreign policy, but more importantly how conventional justice must continue to play an essential role in the wise imperial foreign policy. To advance this point he must engage in deception precisely because Athens is not the wise city. Thus, necessity and justice do not function as contraries, i.e., they *can* both be true at the same time. Where one is properly the guiding principle, i.e., necessity in foreign policy, justice in domestic policy, the other continues to operate in the generation of a *μέτριος*. To typify Thucydides' teaching as Machiavellian is to miss the actual reversal intrinsic to each. Machiavelli makes all policy, including domestic policy, operate on the principle of foreign policy; Thucydides demonstrates how justice even in warfare remains essential.

JEROME R. CORSI
Franconia College

TO THE EDITOR:

While making observations of genuine interest and importance in themselves, and correcting me on a significant point, Professor Corsi has not fully grasped the argument of my article. In order to indicate some of our agreements and disagreements so far as I understand them, I am obliged to place that argument in a somewhat broader context.

Professor Corsi believes, erroneously, that the argument of my article is that "justice is the advantage of the stronger," to which he adds, also erroneously, the qualification, "in matters of foreign policy." Furthermore, he appears for the most part to believe that I understand that thesis in the following way: the "mighty," in foreign affairs, never have need of what he calls conventional justice; or perhaps, they must always act selfishly, and to act selfishly means never to act in accordance with conventional justice even when such justice happens to be in one's best interest. To this he responds, quite properly, that even the strongest, even in foreign affairs, cannot dispense completely with such justice. I agree with the general statement of this important point; but my saying that "those who are able to do wrong do not refrain from doing wrong" never excluded this, to say nothing of the fact that there is no necessity for selfishness to be stupidly pursued.[1]

[1] Nor do the Athenians either at Sparta or Melos assert that "justice is only the advantage of the stronger." (See I, 76.3 and V, 89, and cf. V, 105.4.)

But what Professor Corsi writes suggests further that the love or concern for justice, which exerts some power over the souls of men, may sometimes be necessary to secure or prepare popular agreement to a policy which prudence by itself might require. He makes the observation that it was only after the Athenians had begun to regret their action of condemning *all* the Mytilenians rather than only the guilty to death and to consider it to be savage that Diodotus could advance his prudential arguments. This correct and valuable observation makes necessary some modification of my account of Diodotus' speech. But I would express the needed correction tentatively as follows: the highest deceit of Diodotus is to advance the prudential (and also gentler) policy by arguments which, after proper preparation by appeal to interest, appeal also to the Athenians' somewhat ambiguous sense of justice (III, 47.3; cf. 46.5).

On the basis of such lines of thought as have been indicated, Professor Corsi would be inclined, I believe, to say that Thucydides asserts a strength of justice, where I argue (albeit penultimately) that Thucydides is impressed with its weakness. But it may be that such an assertion of strength would not be so incompatible with that assertion of weakness as one might wish. One must therefore ask whether this strength, or either of its grounds, constitutes an adequate defense of justice against the attack which Professor Corsi believes to have been directed against it, when all the elements of the argument are taken into account. What would constitute such a defense? On these questions, I believe we would disagree. However that may be, it is only in the light cast by a consideration of them that the concern for justice, which Professor Corsi writes of and thereby shows, and which is a pervasive if implicit theme of my article as well, can be properly appreciated.

CHRISTOPHER BRUELL

Boston College

As for the latter, it is not absolutely clear that in their view, "the slaughter at Melos is required once the Melians refuse to submit," that they would not, had the decision been theirs, rather have imposed banishment, for example. And their original terms, as they indicate in passing (V, 111.4), were moderate, so that one cannot fairly say that "there is no gentleness to their foreign policy."

TO THE EDITOR:

Jack Walker has already commented on the substantive issues emerging from the paper by Virginia Gray ("Innovation in the States: A Diffusion Study," *American Political Science Review*, 67 [December, 1973], 1174–1185). This letter draws attention to some methodological problems.

Gray's models are estimated against time series data, which are likely to be auto-correlated, and include lagged dependent (or endogenous) variables on the right-hand side of the regression equations. This presents very messy estimation problems, which are not considered in detail in many econometric text books.

Let us, for simplicity, consider her linear model

$$A_t = bA_{t-1} + c + e_t$$

although the quadratic equation, also a function of A_{t-1}, contains the same difficulties. As Gray notes, serial correlation of the error terms can indeed lead to distorted results. But problems of serial correlation themselves, if not compounded by other matters, can generally be accommodated by use of Generalized Least Squares.[1] It is for other reasons that the regression results presented in Gray's paper are unreliable.

The major inaccuracies arise from the use of a lagged endogenous variable (in this case, A_{t-1}). If A_t is highly correlated with A_{t-1} (as is, in this case, true), then one of the 'independent variables,' A_{t-1}, will be correlated with the disturbance term e_t whenever e_t is autocorrelated.[2] But Ordinary Least Squares (OLS) regression specifically assumes that this is not the case, i.e., that the disturbance is uncorrelated with the right-hand-side variables. Therefore, much of the explanation of variance, which should in fact be assigned to the disturbance term e_t, will instead be assigned to the correlated variable A_{t-1}, resulting in overstating

[1] It should be recognized that the Durbin-Watson test (DW), which Gray appears to have employed, is not valid for this case, primarily for reasons related to the distortion of the residuals, and detailed in the text above. In addition, however, DW tests only for the statistical significance of the coefficient of a first order auto-regressive process, which need not be the appropriate serial dependence model. Thus, it is generally preferable to examine the correlograms of the residuals in order to ascertain the model generating the serial correlation, as opposed to relying upon DW alone. Douglas A. Hibbs, Jr., "Problems of Statistical Estimation and Causal Inference in Time Series Regression Models," in H. L. Costner, ed., *Sociological Methodology 1973–74* (San Francisco: Jossey-Bass, 1974), is an excellent reference on this point and related complexities.

[2] For example, if

$$A_t = bA_{t-1} + c + e_t \qquad (1)$$

while e_t is a first order auto-regressive process

$$e_t = \rho e_{t-1} + v_t \qquad (2)$$

then, substituting in (1)

$$A_t = bA_{t-1} + c + \rho e_{t-1} + v_t \qquad (3)$$

But, since A_{t-1} is a linear function of e_{t-1} and e_t is a linear function of e_{t-1} then A_{t-1} is correlated with the disturbance term e_t. Similar results can be proved for e_t generated by alternate disturbance structures, as long as e_t is auto-correlated. (i.e., a function of e_{t-1}).

its size and significance. At the same time, the disturbance term will be understated, and the residuals accordingly distorted. The Durbin-Watson statistic is accordingly inappropriate in cases with lagged endogenous variables: It is not intended to, and does not, address the crucial problem of correlation between independent variable and disturbance, and it is no longer even a measure of serial correlation of the disturbances, since the residuals no longer reflect the disturbance. Thus, any serial correlation of the 'true' residuals may be masked.[3]

In addition, high enough correlation between A_t and A_{t-1} makes the regression equation functionally equivalent to regressing A_t upon itself; so it is not surprising when high and statistically significant R^2's result from such a tautology.

From the results presented by Gray, this indeed appears to have been the case. None of Gray's R^2's was below 0.93, and most were well over 0.99. (Contrary to her statement, an R^2 of 0.928 is not generally described as "low.") Unfortunately, insufficient data are presented for a fuller evaluation—in particular, as to why a change in R^2 from .9950 to .9951 (as for the example of Board of Education) is "a significant increase in the proportion of variance explained" or proves that "innovations seem to diffuse through interaction."

Although footnote 25 refers to the "proper adjustments . . . for using a lagged dependent variable," it is unclear what procedure was in fact followed. In theory, the best method involves either the substitution of carefully chosen instrumental variables for the lagged endogenous term, or, alternately, previous knowledge of the precise nature of the underlying process which generates the disturbances, followed by application of some nonlinear least squares algorithm with the appropriate constraints applied to the coefficients (usually impossible in practice.) But it must be recognized that theoretical solutions are not always optimum in a particular applied context.[4] In practice, Gray's equation is probably not testable by any statistical method.

The problems with this regression analysis may have little effect upon the substantive findings, which appear quite cogent. Nonetheless, this brief discussion may point out the perils of application of statistical techniques without full understanding of the often complex assumptions involved.

RAISA B. DEBER
Massachusetts Institute of Technology

TO THE EDITOR:

Ms. Deber's excellent communication quite correctly calls attention to the potential, though not the necessity, for erroneous results when the regression model includes a lagged endogenous variable, i.e., when the dependent variable is some function of its value in the past. As she points out, the messy estimation problems, particularly the tests for auto-correlation and the practical solution to the joint occurrence of lagged variables and auto-correlation, are not discussed fully in many standard econometric texts, especially the one[1] I was consulting for auto-correlation tests when writing the article in early 1971.[2]

Nor do more recent texts provide much help: Wonnacott and Wonnacott discuss the lag problem, though not the solution, in two pages.[3] The second edition of Johnston devotes more space to the problem but leaves open the question of how misleading the use of the Durbin-Watson statistic may be in the lag situation: first, it is noted that the *d* statistic is often applied "for want of anything better";[4] then an experiment is reported showing that the Durbin-Watson test on the OLS residuals is quite powerful;[5] finally, it is reported that a new test has been developed by Durbin for samples larger than 30 and this statistic *h* can be computed in a routine fashion.[6] The latter test is the appropriate one for me to have used; it was first published in an econometric journal in 1970.[7]

Clearly Ms. Deber has hit upon a diffusion problem: if the diffusion of information from technical economics journals to substantive fields of political science were rapid, a more definitive test for auto-correlation at least would have been conducted for my data. I hope that the Hibbs article of this year will speed up the diffusion of

[3] Indeed, Gray's Figure 1 strongly suggests that auto-correlation is present.

[4] More complete discussion of these complexities, plus an appended bibliography, can be found in Raisa Deber and Nazli Choucri, Appendix B ("Note on Methodology,") and Raisa Deber, Appendix C ("The Results of an Experiment,") in Nazli Choucri and Robert North, *Nations in Conflict: Population, Expansion, and War* (San Francisco: W. H. Freeman, 1974).

[1] Carl F. Christ, *Econometric Models and Methods* (New York: John Wiley & Sons, 1966).

[2] Virginia Gray, "Innovation in the States: A Diffusion Study," *American Political Science Review*, 67 (December, 1973), 1174–1185.

[3] Ronald J. Wonnacott and Thomas H. Wonnacott, *Econometrics* (New York: John Wiley & Sons, 1970), pp. 146–147.

[4] J. Johnston, *Econometric Methods* (New York: McGraw-Hill Book Co., 1972), p. 309.

[5] *Ibid.*, pp. 311–312.

[6] *Ibid.*, pp. 312–313.

[7] J. Durbin, "Testing for Serial Correlation in Least-squares Regression When Some of the Regressors Are Lagged Dependent Variables," *Econometrica*, 38 (May, 1970), 410–421.

such information to political scientists as well as sociologists.[8]

Apart from testing more extensively and more appropriately for the presence of auto-correlation, Ms. Deber's communication is a plea for me, and indeed for anyone working with incremental data, to use an estimation procedure other than OLS because its assumptions *may be* violated for any particular data set in the manner she demonstrates for the general case. It should be noted that some econometricians, such as Theil, argue that OLS and GLS can be justified for a number of data sets which violate the basic assumptions in this manner, provided the sample is large.[9] However, Ms. Deber concludes, as do so many of the econometric texts, that one can't make bricks without straws;[10] specifically, she notes that "theoretical solutions are not always optimum in a particular applied context" and that the solutions she suggests are "usually impossible in practice."

Now what is the substantive political scientist, interested in testing a theory, to do in this situation? If he or she proceeds to make bricks, i.e., to conduct a scientific inquiry, he or she may stumble into problems which worry statisticians but for which they, at present, have no practical solution, i.e., they have not yet assembled the straws to their satisfaction. If, on the other hand, one is paralyzed into inaction by the fear of making bricks which later turn out to be faulty because of the quality of the straws, then we political scientists are at the mercy of statistics.

My own answer to this dilemma is to use whatever statistics are at hand at the moment and to put one's effort into developing an adequate theory which can be tested statistically, logically, against prior empirical findings, and so forth. In the case of innovation, diffusion theory as expressed in formal models of varying complexity is well-established and the empirical evidence is impressive.[11] In a recent synthesis of social change research 174 sets of binary adoption data are reviewed; the time path of all look like an S-curve, which can be described by the cumulative normal or the logistic curve, and the median goodness of fit is high ($R^2 = .98$), regardless of whether the innovations being diffused are technological or social.[12] Thus, I would argue that there is every reason to accept the results reported in my article: the theory is well-grounded, the various findings (graphs as well as regression results) agree with the extensive prior empirical results in other fields of social science, and the statistical methods, though by no means perfect, were reasonably sophisticated among those used by political scientists three years ago.

VIRGINIA GRAY
University of Minnesota

[8] Douglas A. Hibbs, Jr., "Problems of Statistical Estimation and Causal Inference in Time Series Regression Models," in H. L. Costner, ed., *Sociological Methodology 1973–74* (San Francisco: Jossey-Bass, 1974).

[9] In answer to "how large is large?" Theil reports sampling experiments consisting of 20 observations where the true value of the lagged coefficient was underestimated 75% of the time, thus leading to a conservative bias: Henri Theil, *Principles of Econometrics* (New York: John Wiley & Sons, 1971), pp. 425–427.

[10] The statement appears in the first edition of Johnston where he is discussing the solutions to multicollinearity: J. Johnston, *Econometric Methods* (New York: John Wiley & Sons, 1963), p. 207.

[11] For theory see: James S. Coleman, *Introduction to Mathematical Sociology* (London: Free Press of Glencoe, 1964), chapter 17; David J. Bartholomew, *Stochastic Models for Social Processes* (London: John Wiley & Sons, 1967), p. 206.

[12] Robert L. Hamblin, R. Brooke Jacobsen, and Jerry L. L. Miller, *A Mathematical Theory of Social Change* (New York: John Wiley & Sons, 1973), p. 4. The universality of the S-curve is also supported by many studies reported in Everett M. Rogers and F. Floyd Shoemaker, *Communication of Innovations* (New York: The Free Press, 1971), pp. 161–164.

TO THE EDITOR:

Professor A. James Gregor's review of my book, *Mussolini and Fascism: The View From America* ("On Understanding Fascism: A Review of Some Contemporary Literature," *APSR*, 67 [Dec., 1973], 1332–47), is a good example of a political scientist asking answers to ultimate questions that humble most historians, and asking, at the same time, and paradoxically, that historians steep themselves in "hard," "controlled" quantitative data where behavioral answers can be found only because ultimate questions have been forsaken. Gregor takes me to task not only for expressing my disappointment with the Roper polls of the '30s (my one effort at empirical inquiry), but, more importantly, for not doing the impossible—offering a definition of the "reality," "nature," and "essence" of Fascism. In the book I deliberately and emphatically avoided such an exercise, and in the concluding chapter, discussing the various views of Marxists, humanists, liberals, Catholics, etc., I stated clearly: ". . . if one cannot penetrate to the essence of reality there is still considerable satisfaction in viewing it from all perspectives. And if the meaning of Fascism continues to elude historians there is still something to be gained by studying the meanings Americans gave it; indeed there is much more to be gained by comparing American interpretations with those of European *emigres*."

It is no more necessary to define the essential nature of a political phenomenon before one writes about it than it is necessary to define the essence of a book before one reads it—and understands it. My book dealt with not the reality of Fascism but with American attitudes toward both Mussolini as a political personality and Fascism as an ideological curiosity. Nevertheless, Gregor questions, and rightly so, how I can say that Americans "misunderstood the essence of the Fascist movement" when I myself venture no interpretation of its ultimate historical meaning. Admittedly it is a question that plagued me while writing the book, and one that has as many implications as Melville's solipsistic nightmare. But from a scholar of Gregor's stature, I expected profound political and historical criticisms, not useless methodological quibbles. I also expected that he might understand that there is "reality," and there is "Reality," and that the historian can deal with the former without defining the inner essence of the latter. The historian, that is, can deal with people's perception of the reality and meaning of a given phenomenon, and he can judge critically their views to the extent that that phenomenon developed into something quite different from what was perceived. "This kind of retrospective omniscience," I pointed out in the Introduction, "comes cheap to students of history." Yet it is legitimate to criticize, without claiming to know what Fascism "is," American attitudes and illusions about its historical significance and political direction. Hence, in discussing the views from the White House, I concluded:

> In general, and with some variation in emphasis, both the Republican and Democratic administrations accepted these assumptions: that Mussolini was generating economic and social progress; that although a dictator he enjoyed the overwhelming support of the people; that with the rise of Nazism he would prove a moderating foil to Hitler; and that despite his quixotic temperament he was good for Italy and good for the United States. Doubtless these assumptions contain a modicum of truth, but beginning with the Ethiopian War they collapsed one by one. The real rub, however, is not that history proved these attitudes wrong but that they were based on so shallow an understanding of Fascism in the first place. Roosevelt's hope that Mussolini would restore "democratic processes," for example, betrayed a complete ignorance of Il Duce's scorn for liberal democracy. Similarly, the general belief so prevalent in the State Department under Coolidge and Hoover that Mussolini had exterminated Bolshevism and was rapidly advancing Italy economically, appears, in light of Italy's subsequent economic troubles and the upsurge of Communist strength after the fall of Fascism, only to compound ignorance with illusion.

Throughout the review Gregor roasts me not only for criticizing opinions about a reality without explaining the ultimate nature of that reality, he also burns me for making "judgments," particularly those that "satisfy [my] moral sensibilities," and thus for writing a book in which there is "little . , . that can pass as warranted social science generalization or cognitively supported explanation" (p. 1336). Gregor then concludes his own review of the nine books on Fascism by confessing that "one doesn't really know what to make of all this. Contemporary social science clearly has not provided us with a clear definition of Facism" (p. 1346). Perhaps it is Gregor's need for a "clear definition" that is the problem. Since he does not know what to make of the conflicting interpretations, he seems to suggest that no one has the right to pass judgment on Fascism. Enthralled by the symmetrical beauty of untarnished theoretical classifications and "clear" taxonomic categories, he is uncomfortable with the messiness of historical experience. Demanding a flawless definition, he complains that my judgments and generalizations lack "cognitive" support, which may be only another way of saying that they betray normative concern. Indeed my own historical judgment of American attitudes toward Fascism is both ironical and moral, that is to say, Niebuhrian. Social scientists who listen to Gregor may reject it, but I hope some statesmen will read it:

> . . . the irony is that the attitude of the American government was not so much "reactionary" as "progressive." Most public officials sincerely believed that an Italy under Mussolini would improve economically and that, once the pace of modernization increased, political liberalization would eventually follow. This concept of democracy through industrial progress has been one of America's most venerable assumptions; applied internationally, it has been one of America's most vulnerable illusions. For the doctrine of political salvation by economic growth evolves from America's own national experience, and therein lies its limitations. The lesson of Fascism for American diplomacy is basically this: if there is one historical experience rarer than a Right-wing dictatorship achieving genuine social progress, it is a Right-wing dictatorship transforming itself into a genuine democracy.

Gregor's review is replete with contrived and extracted quotes and phrases taken out of context and crammed together, one gathers, to prove to the reader that I fail to explain what Gregor, and not I, claims that I intended to explain. Two examples will suffice. "It was 'right-wing,' in Diggins' judgment, at least in part because it exploited 'national humiliation—the vital mental germ of almost every right-wing movement' " (p. 1335). Gregor then cites what every sophomore knows, that the left-wing also exploits wounded nationalism, and he follows this up with the wonderful *non sequitur*, that on these grounds

"Castro's and Mao's communism seem to qualify as 'right-wing' " (p. 1335). The problem of distinguishing Mao's nationalism, rooted in immediate historical experience, from Mussolini's, fired by romance and mythos, I'll leave to Gregor, who is as weak with distinctions as he is clever with distortions. For the point is that I was *not* attempting to define, let alone explain, Fascism. On the contrary, in that very sentence he quoted I was describing the appeals of Fascism to the Italo-Americans, who had suffered the humiliation of Caporetto and the Versailles settlement. Small wonder that a reviewer who cannot keep in mind what is being explained cannot himself distinguish between what can and what cannot be explained.

Another example of Gregor's sloppiness occurs when he states that "Diggins is prepared to admit that Fascists felt that a 'dreadful misunderstanding' about Fascism was 'prevalent' in the United States" (p. 1335). Had Gregor carefully read the book, instead of skimming it for isolated phrases like the "political psyche" and the "American mind," which he seized upon to advertise his misspent "analytical" power, Gregor would see that he cannot even report facts accurately, not to mention interpretation. Actually, the "dreadful misunderstanding" was felt, not by Fascists, but by one man, the journalist Charles Sherrill, who was upset because Walter Lippmann and several other newsmen wrote critically of Mussolini's Italy. What point is Gregor trying to make by claiming I am "prepared to admit" that, in respect to press attitudes toward Fascism, not all was sweetness and light? Apparently he wants American opinion to be all of one piece, either favorable or unfavorable toward the Mussolini regime. And when he finds me describing a divided and continuously changing public opinion, it is I, and not public opinion, that he finds inconsistent. "The fact is," Gregor writes, "that there is no public way of choosing between these apparently incompatible generalizations" (p. 1335). Why not? When two or more viewpoints are being described, in what sense can they be said to be "incompatible"? And why does one have to "choose" between varying and contradictory opinions on Fascism, which itself was nothing if not ambiguous? Gregor wonders, after reading my book, "how, in fact, the historian proceeds to formulate and support generalizations" (p. 1335). This author wonders, after reading his review, how one who calls himself a political scientist can continually fail to understand what it is that is being discussed. Thus Gregor appears unable to distinguish Sherrill's statement, as a specific utterance made in a personal letter, from more general attitudes relating to different groups at different moments in time. Not only does Gregor not know what is being explained, he confuses private expression for public opinion. And while he shows no capacity for understanding that "opinion" can be as ambivalent as "reality" is complex, he waxes eloquent about "social sciences generalizations" and "cognitively supported explanation"—always a bad sign.

Similarly, for all Gregor's professed awareness of the distinction between "opinion" and "reality," he completely loses sight of this distinction when he tries to refute my historical generalizations. Specifically, he denies that Fascism was the "first anti-democratic political experiment of the twentieth century," claiming that the Bolsheviks deserve such dubious honor. But is Gregor dealing with "reality," his fetish, or "opinion," my subject? "Whatever else it was," he tells us, "Bolshevism doesn't seem to qualify as a 'democratic political experiment'." Here Gregor falls into the same trap he accuses me of being mired in—judging a political phenomenon not by how it was conceived but by what it became. The point I was trying to make is that the intellectuals' perception of Fascism and Bolshevism in their early stages was quite different. There were thousands of writers in the West for whom Bolshevism did "seem to qualify" as a popular, mass movement and scientific "experiment" in the "new democracy." True, they should have paid more attention to Lenin's *What's To Be Done* than to his *State and Revolution*, but it is worth recalling that the scores of intellectuals who soon broke with Bolshevism did so because its democratic (and anarchist) promises had been betrayed. How many ideologues broke with Fascism out of sense of democratic disenchantment? Finally, Gregor denies that Fascism was "the first modern political movement to blare the noise of ideology into concert halls, art galleries, and literary salons," again claiming that I ignore the "Bolshevik insistence that all culture serve the revolution." This point is debatable, but here, as elsewhere in the review, Gregor conveniently shifts from the subject under discussion: the cultural repercussions of Fascism in the early twenties. Indeed, the unity of art and politics was precisely what was debated, and ultimately rejected, in the early twenties by Bolshevik enthusiasts like Barbusse, Dell, Eastman, Rolland and others. Perhaps not until the thirties did communists *effectively* make "culture serve the revolution." Meanwhile, Pirandello, Pound, Puccini, Santayana, Shaw, and other writers and artists discovered Mussolini long before their Left counterparts were forced to choose Stalin against Hitler, "the bad against the worst."

One is puzzled why Gregor took up so much space in the *APSR* to demonstrate that I was working with a "vague and suspect methodology" (p. 1335). I would be the first to concede this, and indeed admitted it in my Introduction and

stressed it again and again throughout the book. If Professor Gregor is in possession of a methodological windfall, if he has the epistemological key that will solve the problem of studying, not only an unfinished and amorphous reality like Fascism, but that "slippery beast" of public opinion, I would appreciate hearing about it in his reply. Students of history need all the assistance and advice we can get from political scientists. What we don't need is more empty rhetoric that predictably calls for methodological precision, particularly in a review that is itself a model of misrepresentation and an escape from the demands of historical explanation and moral judgment.

JOHN P. DIGGINS
University of California, Irvine

TO THE EDITOR:

Professor John Diggins's letter affords me the occasion for restating some very simple methodological suggestions. That Diggins misunderstood my original remarks is much to be lamented, and restating them might serve some purpose.

But first, let me say that Diggins's book accomplished what he says it did—and in my review I clearly granted as much. His book tells us "something" about prewar American views concerning Fascism. In my review I indicated that the book did that "quite well" (p. 1335). Diggins agrees with me that his book does not tell us anything coherent about the "real nature" or the "reality" of Fascism. At this point we seem to part company. Diggins seems to think that it was I who charged him with the responsibility of talking about "essences" and "real" natures. But it was Diggins, not me, who insisted that Americans "misunderstood the essence of the Fascist movement . . . " (p. xv of his Introduction). I simply reminded him that to insist that Americans misunderstood Fascism logically requires that Diggins be prepared to argue that *he* understands it. Since Diggins admits that he does not understand it he might have avoided the dilemma that apparently "plagued" him throughout the writing of his book by simply reporting American opinions without trying to identify it with understanding *or* misunderstanding. My basic objection to Diggins's book has little to do with what he does—report on the collection of confused and sometimes contradictory opinions Americans entertained about Fascism. My objection turns on what he pretends to do: to talk about how that opinion understood or misunderstood the "essence," "reality," or "real nature" of Fascism. Since Diggins seems to believe that it is "impossible" to address himself to the task of defining that "essence" and that "reality," I suggest he stop referring to it.

I am sorry that Diggins seems to feel that this is a methodological "quibble." I am sure he will find my further objection no less of a quibble. My quibble is that Diggins's "rough content analysis" cannot identify the modal traits of a population if the only data he alludes to are drawn from some purposively collected sample of literature. One simply cannot inversely project findings drawn from a potentially biased sample of literature over the attitudes of an entire population (even if that population were better defined than those dealt with by Diggins). This is a very simple rule governing the interpretation of findings. It is common knowledge among undergraduates. It may, therefore, be commonplace, but I don't think it qualifies as a quibble.

Diggins seems content to confuse several issues. I do not expect the world, history or public opinion to be all of a piece. I simply attempted to remind him that he could not talk about complex constructs like "the American mind," or the "American psyche" when his data refer only to his personal insights into some ill-defined, nonrandomly selected collection of literature. To know what some Catholic authors, public officials, or Italo-Americans were saying at any particular time is interesting and perhaps important. But all that one can say about that kind of information is that that was what some Catholic authors, public officials or Italo-Americans were saying at that particular time. One cannot, on the basis of such information (however credible), generalize over all Catholics, all public officials or all Italo-Americans. Still less does it license talk about "the American mind," "public opinion," or "the American value system."

The rest of Diggins's objections turn largely on substantive, but tangential, issues that it would serve little purpose to pursue. I suggest, however, that Diggins write a book about some of the subtle distinctions he wishes to argue. He sees a distinction between Mao's nationalism—"rooted in immediate historical experience"—and Mussolini's nationalism—which was apparently not rooted in historical experience, but "fired by romance and mythos." He seems to think that at some vaguely specified time some vaguely specified number of "intellectuals" saw Bolshevism as an experiment in "new democracy"—while everyone apparently agreed that Fascism was intrinsically and inherently "anti-democratic." I suggest that to support these rather quaint distinctions would require far more than Diggins's confident assertions. But to pursue all this here would hardly serve any useful purpose.

The point of my review was very simple: neither historians nor anyone else can generalize, with any confidence whatever, on the basis of the kind of data Diggins had at his disposal. He could neither generalize over an omnibus "American

public opinion," "America's apologia for Fascism," or lead us "to the heart of American political values." Still less can his data, reports on confusing and confused American opinions about Fascism, support his Niebuhrian moral judgments.

If there is an "epistemological key" to be found in all this it is the following: if one is dealing with the reports about the fragmentary, episodic, confusing and sometimes contradictory opinions held by intellectuals who are select members of populations and subsets of populations, and one is employing an admittedly vague and suspect methodology, one should not attempt to talk about complex constructs like "the values of America's business culture," the American "mass mind," or the "pervasive values" entertained by Americans.

A. JAMES GREGOR
University of California, Berkeley

TO THE EDITOR:

Voters in the William Riker and Steven Brams article, "The Paradox of Vote Trading,"[1] do not sell their votes in the best market. As is normal, if you sell your product for less than the best you can get for it, you do not do very well. Consider the first bargain on their Table 5. Voter 2 and voter 3 exchange votes so that each acquires a positive value of 1. However, suppose that voter 2, after having heard voter 3's proposal, approached voter 1 and asked him if he could do better. Voter 1 could agree to vote for $\bar{Y}$ in return for voter 2 voting for X. This would give voter 2 a positive value of 3 points and reduce the loss suffered by voter 1 from -4 to -1. Clearly, both would be made better off by this bargain.

Of course, at this point voter 3 might approach voter 1 and offer to vote for X in return for voter 1 voting for Y. This would lead to a value 2 for voter 1, -1 for voter 3, -1 for voter 2. Again, both the members of the bargain, 1 and 3, can be made better off. As in most logrolling cases, there is a set of bargains of this sort that form a cycle. This is probably the reason that Riker and Brams ruled out recontracting; but the same cycle can occur in negotiation for a contract rather than after the contract or logrolling trades have been completed.

If there is nothing but redistribution involved, such cycles are essentially endless. When there are not only redistributional considerations but also efficiency considerations—i.e., there are different total payoffs to different possible coalitions in real terms, then the cycles are not necessarily unending. This is particularly so if there is a rather small number of possible trades available, as is true in the Riker-Brams article.

In the example given above, like Riker and Brams I did not assume that the individuals sold their votes in the best market because I confined the trading possibilities to trading on X and Y. If the parties seek out optimal trades, the possibility of an agreement that voter 2, regardless of counteroffers, will vote for X and that voter 1, again regardless of counteroffers, will vote for V, provides the highest payment that these two members can obtain from the sale of their votes on those issues. A consistent set of such bargains by all

[1] *American Political Science Review*, 66 (December, 1973), 1235–1247.

Table 5. Utilities of Members for Outcome of Voting

	Preferred Outcome Col 1	Motion x $u_i(X)$ Col 2	$u_i(\bar{X})$ Col 3	Preferred Outcome Col 4	Motion y $u_i(Y)$ Col 5	$u_i(\bar{Y})$ Col 6
Member 1*	X	1	-2	Y	1	-2
Member 2	X	1	-1	$\bar{Y}$	-2	2
Member 3	$\bar{X}$	-2	2	Y	1	-1
		Motion w $u_i(W)$	$u_i(\bar{W})$		Motion z $u_i(Z)$	$U_i(\bar{Z})$
Member 1	W	1	-1	$\bar{Z}$	-2	2
Member 2	$\bar{W}$	-2	2	Z	1	-1
Member 3*	W	1	-2	Z	1	-2
		Motion t $u_i(T)$	$u_i(\bar{T})$		Motion v $u_i(V)$	$u_i(\bar{V})$
Member 1	$\bar{T}$	-2	2	V	1	-1
Member 2*	T	1	-2	V	1	-2
Member 3	T	1	-1	$\bar{V}$	-2	2

* Nontrader on pair of motions.

members with each other would lead to the solution of exactly the same value as is obtained with what Riker and Brams call "sincere voting." Indeed, all the parties would be voting in the same manner as in sincere voting. At the same time, they would be selling their votes for the highest price.

Riker and Brams have inadvertently assumed that in vote trading you must of necessity vote against your preferences in order to get a payment for your vote. This is not necessarily true; you may secure "bribes" for not departing from your preferences. Indeed, if the people who talk about logrolling as improving the situation were defining logrolling as accepting bargains which are not the best you can do, they could hardly argue for the optimality of the process. Of course, it is true that in the real world information conditions may lead people to make mistakes; but this is just as true in deciding which "sincere" vote is in your best interest as in deciding which bribe to accept.

It has been known for some time that, if the preferences are appropriately arranged, then the logrolling outcome is the same as that which would be obtained under simple majority voting.[2] Further, Riker and Brams are wrong when they say "external costs have generally been ignored in the writings of those who extoll the benefits of vote trading."[3] Such external costs are part of the foundation of the reasoning in *The Calculus of Consent*, where they were discussed in great detail with special emphasis on explicit logrolling and on the case where majority voting leads to the same pattern of votes as "sincere voting." They were also treated in "Entrepreneurial Politics" and in my article, "A Simple Algebraic Logrolling Model."[4] The particular case they have designed, which I find mathematically most interesting, has not previously been discussed but the general principle is well-known. In such cases, logrolling leads to the same outcome that would be obtained without it and there are arguments in terms of reducing transactions costs for not having logrolling, but this is not the Riker-Brams argument.

In sum, Riker and Brams have demonstrated that those who sell their votes for less than they are worth do badly. In the particular case in which they discuss this, doing badly means that they do worse than they would have done had they not sold their votes at all. The problem, however, lies in the lack of skill on the part of the vote traders, not in the vote-trading process itself.

GORDON TULLOCK
Virginia Polytechnic Institute and State University

TO THE EDITOR:

Gordon Tullock, who is one of the chief advocates of the universal desirability of logrolling, has committed a number of errors of analysis and interpretation in discussing our article:

(1) There is no way that the paradox we set forth can be avoided by the methods Tullock suggests. In our example (p. 1241), on any particular pair of motions the following kinds of outcomes are possible, using x and y as an illustration:

	(x, y)	$(x, \bar{y})$	$(\bar{x}, y)$	$(\bar{x}, \bar{y})$
Member 1	2	−1	−1	−4
Member 2	−1	3	−3	1
Member 3	−1	−3	3	1

Our example consisted of a permutation of the three members with respect to three different pairs of motions (x and y, w and z, and t and v). Calculating the results for each member of the permutations among positions, we get:

	Column 1 both motions pass in all three pairs	Column 2 one motion passes, the other fails in all three pairs	Column 3 both motions fail in all three pairs
Member 1	$2+(-1)+(-1)=0$	$(-1)+3+(-3)=-1$	$(-4)+1+1=-2$
Member 2	0	−1	−2
Member 3	0	−1	−2

Defining vote-trading in the natural way, we get column 3, supra, or a value of (−2) for all three members when they trade votes. Tullock, defining vote trading as blackmail, gets column 2, supra, or (−1) for all three members when they trade votes. Since each member would get zero (i.e., column 1) if they did not trade votes, clearly Tullock has not avoided the paradox, which is simply the fact that logrolling may make all the participants worse off than if they forswore it.

(2) Tullock appears to recognize that the paradox still exists because he then proposes the members blackmail each other back to the result in column 1, which is indeed better for everyone. The whole point of the paradox is to reveal this

[2] Cf. James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962), pp. 126–130.

[3] Riker and Brams, "Paradox of Vote Trading," p. 1240.

[4] Gordon Tullock, "Entrepreneurial Politics," Research Monogram #8, Thomas Jefferson Center for Political Economy, University of Virginia, 1964; Gordon Tullock, "A Simple Algebraic Logrolling Model," *American Economic Review*, 60 (June, 1970), 419–426.

fact. Tullock apparently believes that the matter can stop there, but of course it cannot because it is in exactly that position that a normal trade is desirable. That trade leads to column 3, which is what he was trying to avoid. This is simply to say that the system forms a cycle, which we discussed at length on p. 1245 of our article.

(3) In proposing a trade between 1 and 2 on x and v, Tullock errs when he says that such a trade provides the highest payment that these two members can get from selling their votes on these issues. For member 2, the salience of x (where "salience" is the absolute value of the difference between the utility of a pass and the utility of a fail) is 2, while the salience of v is 3, giving a total salience of 5. But for motions x and y for member 2, the total salience is 6. Hence, 2 makes more on an agreement on x and y than on x and v. Clearly, as we said, x and y is 2's best market, not x and v, as Tullock says.

(4) Despite the oversights in Tullock's letter, there is one serious dispute between us. This concerns the way one is to define and understand vote trading. We have defined it as occurring when there is an obvious and immediate advantage to both parties. Tullock allows also for a kind of blackmail trading. In his procedure, person A attempts to force person B to trade by saying to B that, if you (B) do not trade with me (A), I will trade with someone else (C) in such a way that you will suffer an external cost. Person B can then decide whether A really means his threat. We agree that this is possible in that there are some people who may try anything that can be thought of. On the other hand, this is too contrived an action for most people, especially since it involves a legislator (A) negotiating with a third party (C) simply to manipulate (B). Surely A will not last long as a negotiator if he is known to behave in this way. In general, vote trading is a very difficult form of barter, hard enough to arrange without complicating it. Economists do not create a theory of a market for goods based on blackmail and other highly contrived and unlikely maneuvers. Similarly, we think the analysis of vote trading ought to be in terms of the obvious, direct and immediate advantage of the traders.

We believe it is reasonable for Tullock and other correspondents to be upset by the paradox. But the appropriate reaction is not to deny its existence. There is another way around it, as we pointed out in our essay. That is a system of two political parties with strong discipline, which is, in effect, also the game-theoretic solution of a permanent coalition of some pair of members. No vote trading can occur with such institutions and, naturally, the paradox cannot occur either.

WILLIAM H. RIKER
STEVEN J. BRAMS

TO THE EDITOR:

In a recent paper Riker and Brams put forward the hypothesis that logrolling may worsen the situation of every voter, a result which they call "The Paradox of Vote Trading."[1] They try to show, moreover, that under the assumptions postulated, this is a consequence of the instability of the possible coalitions of voters, who want to improve their situations by vote trading. For "even if the original trades are not considered binding so that traders can try to extricate themselves from the paradox, they may make new trades which result in an indefinite cycle of return to the original position, return to the paradox etc." (p. 1236). Finally, Riker and Brams believe "that the paradox of vote trading is closely related to the paradox of voting" (p. 1245).

In the following comment I intend to demonstrate

(a) that the results concerning the "Paradox of Vote Trading" are not a special consequence of logrolling, but can also follow from straight majority voting;
(b) that vote trading can only bring about a worse situation for everybody, if logrolling remains incomplete;[2]
(c) that there exists no necessary relation between the "Paradox of Vote Trading" and the Paradox of Voting.

Let us begin by looking at the first question. In doing so I will make use of the numerical example in Riker and Brams's paper. As they did, I will use cardinal utilities, but all my considerations are also valid for ordinal utilities of voters.

Consider first Table 1, where b_{ij} ($i=1, 2, 3$; $j=1, 2$) are six alternatives belonging to three issues b_i ($i=1, 2, 3$). $u_k=u_k(b_{ij})$ is the utility of alternative b_{ij} to voter V_k ($k=1, 2, 3$). Since only one of the two alternatives belonging to each issue can be selected by majority voting, total

[1] "The Paradox of Vote Trading," *American Political Science Review*, 67 (December, 1973), 1235–1247.

[2] Riker and Brams seem first to realize this: "We assume, however, that once a set of trades on particular issues is made, new trades cannot be made on these issues" (p. 1240). Later on, however, they state: "in the next section . . . we drop even this assumption and show that the paradox persists even with unrestricted trading" (p. 1241).

Table 1

	b_1		b_2		b_3		
u_k	b_{11}	b_{12}	b_{21}	b_{22}	b_{31}	b_{32}	U_k
u_1	2	−4	−1	1	−1	1	−2
u_2	−1	1	−1	1	2	−4	−2
u_3	−1	1	2	−4	−1	1	−2

utility for V_k sums up to:

$$U_k = u_k(b_{1j}) + u_k(b_{2r}) + u_k(b_{3s}),$$

with $(j, r, s = 1, 2)$. This implies, of course, that the two alternatives belonging to each issue exclude each other.

Now assume that each of the issues b_i has to be decided by simple majority voting. Then b_{12}, b_{22} and b_{32} will be selected by the group and the following total utility result for V_k:

$$U_k = u_k(b_{12}) + u_k(b_{22}) + u_k(b_{32}) = -2, \quad (1)$$

$(k = 1, 2, 3)$. Thus simple majority voting makes every voter worse off compared to the status quo, and this without any logrolling taking place.

Table 1 has been obtained from Riker and Brams's Table 5 by forming the sums $u_i(x)+u_i(y)$, $u_i(\bar{x})+u_i(\bar{y})$, $u_i(w)+u_i(z)$, etc. These sums are used by Riker and Brams to show the advantages of vote trading to different pairs of voters. They are, therefore, in their example the utilities of complex alternatives (the b_{ij}'s) which have been put together out of twelve simple alternatives for the purpose of logrolling. As a consequence, vote trading is for Riker and Brams responsible for the paradox that every voter is worse off (compare their Table 8). But the b_{ij} certainly need not be complex alternatives combined for the purpose of vote trading. Thus straight majority voting can produce the paradox just as well as logrolling.

Let us turn to the second point. Table 2 corresponds to Table 5 of Riker and Brams. It gives the utilities $u_k(a_{fg})$ $(f = 1, 2, \ldots, 6;\ g = 1, 2)$ of twelve simple alternatives a_{fg} belonging to six simple issues $a_f (f = 1, 2, \ldots, 6)$. Thus, we have among others:

$$b_{11} = (a_{11}, a_{21}) \qquad b_{12} = (a_{12}, a_{22})$$

$$b_{21} = (a_{31}, a_{41}) \qquad b_{22} = (a_{32}, a_{42})$$

$$b_{31} = (a_{51}, a_{61}) \qquad b_{32} = (a_{52}, a_{62}).$$

By voting independently on issue a_f, alternatives a_{f1} will be selected by the group, and each member gets $U_k = 0$.

Since, on the other hand, utilities are additive, it follows at once from Tables 1 or 2 that the resulting situation of each pair of voters can be improved by logrolling, as stated by Riker and Brams. Expressing a strong preference of the group by P, we get:

$$(a_{12}, a_{22})P(a_{11}, a_{21}) \quad \text{or} \quad b_{12}Pb_{11} \quad \text{and} \quad (2)$$

$$(a_{32}, a_{42})P(a_{31}, a_{41}) \quad \text{or} \quad b_{22}Pb_{21} \quad \text{and} \quad (3)$$

$$(a_{52}, a_{62})P(a_{51}, a_{61}) \quad \text{or} \quad b_{32}Pb_{31}. \quad (4)$$

Thus, if the group selects the b_{i2} $(i = 1, 2, 3)$ by logrolling, everybody will be worse off, since total utilities for voters are now:

$$U_k = -2 < 0, \quad (k = 1, 2, 3), \quad (5)$$

whereas it amounted to 0 with straight majority voting on each simple issue.

The problem with this approach of Riker and Brams is that the possibilities for logrolling are not exhausted by these three separate processes of vote trading. As a consequence their result follows only if logrolling remains incomplete. For, looking at Table 1 which describes the first round of logrolling, we notice that each pair of voters can improve their positions by additional logrolling. For example, as can be seen from Table 3, V_1 and V_3 will be better off, if they join to bring about by logrolling the even more complex alternative (b_{11}, b_{21}). In this case their total utilities would amount to

$$U_k = 2 > 0 > -2, \qquad (k = 1, 3),$$

whereas voter V_2 would get

$$U_2 = -6 < -2 < 0.$$

As a consequence, V_2 would be worse, V_1 and V_3, however, better off than with straight majority voting excluding vote trading. The group preference for the newly created complex issue is

$$(b_{11}, b_{21})P(b_{12}, b_{22}), \quad (6)$$

whereas $b_{32}Pb_{31}$ remains unchanged.

Table 2

	a_1		a_2		a_3		a_4		a_5		a_6		
u_k	a_{11}	a_{12}	a_{21}	a_{22}	a_{31}	a_{32}	a_{41}	a_{42}	a_{51}	a_{52}	a_{61}	a_{62}	U_k
*	x	$\bar{x}$	y	$\bar{y}$	w	$\bar{w}$	z	$\bar{z}$	t	$\bar{t}$	v	$\bar{v}$	
u_1	1	−2	1	−2	1	−1	−2	2	−2	2	1	−1	0
u_2	1	−1	−2	2	−2	2	1	−1	1	−2	1	−2	0
u_3	−2	2	1	−1	1	−2	1	−2	1	−1	−2	2	0

* Riker-Brams notation.

Now vote trading as described in Table 3 still does not exhaust the possibilities for logrolling. For, besides similar vote trading among different pairs of voters, three instead of two of the complex alternatives b_{ij} can be combined to even more complex alternatives. The outcome of Table 3 is given by (b_{11}, b_{21}, b_{32}) and logrolling only takes place between issues b_1 and b_2. But V_2 can certainly get a better result by proposing a trade to V_3 including all three complex issues, namely (b_{12}, b_{21}, b_{32}). This alternative offers—as can be seen from Table 1—$U_2 = -4 > -6$ to V_2 and $U_3 = 4 > 2$ to V_3, whereas V_1 gets $U_1 = -4 < 2$. From this, it follows that

$$(b_{12}, b_{21}, b_{32}) P (b_{11}, b_{21}, b_{32}). \qquad (7)$$

We note specifically that one voter, V_3, is better off than with straight majority voting, since $U_3 = 4 > 0$.

From the above considerations two results can be derived:

(1) Logrolling can only lead to worse results for every voter, if not all issues offering potential benefits to a majority of voters are included in the logrolling process.

(2) Whatever the results of logrolling may be, at least one of the three voters considered will be better off, if all relevant issues have been included in the vote-trading process.

It seems to me that these results are generally true for any number of voters and for ordinal preferences, but at the moment only a proof for two issues with two alternatives could be given. It follows from the above results that the "Paradox of Logrolling" is a consequence of incomplete and unconnected vote trading. This, however, can only happen in the presence of incomplete information, of transaction costs for logrolling or of limiting rules of the political process. All of these reasons are certainly quite realistic, but should be made explicit as the real cause of the "Paradox of Vote Trading."

Finally we turn to point (c), namely, that there is no necessary relation between the "Paradox of Logrolling" and the Paradox of Voting. It has been shown recently that under certain conditions a logrolling situation always implies circular group preferences, if all voters have full information and if no transaction costs or adverse institutional rules are present.[3] The "Paradox of Vote Trading," however, can only occur if one of these conditions is violated, i.e., if incomplete and unconnected logrolling takes place. Thus vote trading implies the Paradox of Voting, even if the "Paradox of Logrolling" is not present.

[3] Peter Bernholz, "Logrolling, Arrow-Paradox and Decision Rules: A Generalization," *Kyklos* (fasc. 1, 1974), 49–62, and the earlier paper by the same author: "Logrolling, Arrow-Paradox and Cyclical Majorities," *Public Choice*, 15 (Summer, 1973), 87–95.

Table 3

u_k	(b_{11}, b_{21})	(b_{12}, b_{22})	b_{31}	b_{32}	U_k
u_1	1	−3	−1	1	2
u_2	−2	2	2	−4	−6
u_3	1	−3	−1	1	2

It is perhaps adequate to conclude this comment by clearing up some problems which have only been dealt with in part by Riker and Brams. Looking back at Table 2 we realize that, as stated by Riker and Brams, b_{12}, b_{21} and b_{32} can be brought about by vote trading (see relations (2) to (4)). But without incomplete information, transaction costs for logrolling, or institutional limitations, there exist possibilities for improving these unwelcome results for each pair of voters. From Table 2 we get easily:

$$(a_{11}, a_{21}) P (a_{11}, a_{22}) P (a_{12}, a_{22}). \qquad (8)$$

Because of (8) and (2) one gets:

$$(a_{11}, a_{21}) P (a_{11}, a_{22}) P (a_{12}, a_{22}) P (a_{11}, a_{21}). \qquad (9)$$

Thus circular social preferences result, as had to be expected from the general proof given in the paper mentioned above. Or as Riker and Brams state correctly, the different possible logrolling coalitions are not stable, they are circularly dominated. The same result obviously holds for the other possibilities for logrolling described in (3) and (4). This, however, not only happens in cases in which the "Paradox of Vote Trading" can happen; on the contrary, it is a consequence of any logrolling situation.

Vote trading confined to a combination of two simple issues into complex ones is, as has been shown above, not the only possibility for voters to get rid of the consequences of (2) to (4). Instead of this they can try to form new coalitions by combining into complex alternatives three, four, five or even six simple alternatives. (Compare (6) and (7).) And it is worth stressing that for each of these "levels of logrolling" the phenomenon of circular social preferences is implied by the possibility to trade votes. Thus, e.g., for a combination of six issues we get:

$$(b_{12}, b_{22}, b_{32}) P (b_{12}, b_{21}, b_{32}) \cdot P (b_{11}, b_{21}, b_{32}) P (b_{12}, b_{22}, b_{32}). \qquad (10)$$

Thus the "six-level" logrolling model of Riker and Brams allows for a wealth of political maneuvers. On each level above the first and below the sixth, different results can be obtained by

forming new coalitions, either by using the same number of issues as before or by adding additional issues and thus moving to higher levels. On the same level, moreover, the same number of different issues can be combined.

It is important to realize that all these complicated possibilities are present before bringing in the additional conditions responsible for the "Paradox of Vote Trading." It seems, therefore, advisable to keep clearly apart the different assumptions, problems, and consequences of the model.

PETER BERNHOLZ
Universität Basel, Switzerland

TO THE EDITOR:

In our essay, "The Paradox of Vote Trading," we were careful to define vote trading as exchanges between members of votes on issues and showed that such trading could render every member worse off. Professor Bernholz has redefined trading in terms of complex alternatives (made up of several trades) and in terms of "complete" trades on all issues; he then conjectures (with our example) that at least one trader will be better off from such "complete" trading. For the following reasons, this conjecture does not seem to us to resolve the issue:

(1) Bernholz's "complete" trades involve all motions in the system including, of course, those that may be already resolved and those that have not yet been made or thought of. It is hard to think of *any* institutional arrangement that admits of such trades. By contrast, our definition of trades fits naturally with institutions—like American legislatures—where logrolling is practiced on an issue-to-issue basis.

(2) Our assumption—perhaps myopic—is that trades, once made, are final, if only because they are ultimately embodied in actual votes. Bernholz, however, assumes that trades can be perpetually reopened. It seems to us that any possible outcome that is offered as a conclusion to this indefinitely long process ought to be stable in the sense that the supposed conclusion is preferred by a majority to all other outcomes. But Bernholz's "complete" trades are not stable. Indeed, the very set of trades he offers as a candidate for a final outcome (i.e., b_{12}, b_{21}, b_{32}) in statement (7)) is itself wholly unstable. At least two sets of trades are preferred to it by a majority: the status quo of no trades, and the set of trades we pointed out as the paradox of vote trading (see Table 1). Thus, while Bernholz is technically correct in claiming that there is at least one voter who benefits from "complete" trades, still this benefit cannot be realized in practice because a majority prefers other situations. (If two members can enforce the "complete" trades, then they can just as easily enforce no trades.) It seems to us, therefore, that Bernholz's claim "(b) that vote trading can only bring about a worse situation for everybody, if logrolling remains incomplete" is highly misleading because of the fact that Bernholz ignored the problem of the stability of outcomes. Specifically, the fact that "complete" trades are themselves unstable, and can be undone, means that the paradox can recur with continued trading on less complex alternatives. There is no reason to assume that trading must proceed in one direction from less complex to more complex alternatives when it is individually rational for voters to reverse this process.

Table 1. Members' Utility for Sets of Trades

	Status quo point of no trades	Trades in paradox of vote trading	Bernholz's "complete" trades
Member 1	0	−2	−4
Member 2	0	−2	−4
Member 3	0	−2	+4

(3) We also believe that his assertion "(a) that the results concerning the 'paradox of vote-trading' are not a special consequence of logrolling, but can also follow from straight majority voting" is equally misleading. In Bernholz's argument, the simple majority voting takes place on "complex alternatives," which are precisely the completed trades of our example of the paradox of vote trading. Thus, simple majority voting reveals the paradox simply because the paradox is embedded by vote trading in the very issues voted upon. It is not clear, therefore, in just what way Bernholz's example is independent of the paradox of vote trading and his assertion (a) must be regarded as not proven.

(4) Finally, with respect to his assertion (c) that there is no necessary relation between the paradox of vote trading and the paradox of voting, we point out that we did not claim that the relation is necessary. Our claim is simply that in our particular ordinal example of the paradox of vote trading, there is an underlying paradox of voting.

WILLIAM H. RIKER
University of Rochester
STEVEN J. BRAMS
New York University

TO THE EDITOR:

J. C. Harsanyi's latest communication (*APSR*, 68 [June, 1974] 730–731) to you renews his disagreement with my "third breakdown of rationality." His initial disagreement, in his review of my book, was based on a mathematical error that he has now retracted. Thus Harsanyi's present criticism of the concept is the *first* that is not based

on a mathematical error. His communication rather obscures this point.

In fact, however, Harsanyi's latest communication, though it purports to criticize the "breakdown," does not actually discuss it at all. The "breakdown," as revealed by Theorem 9 of my book, consists in a conflict between two kinds of motivation that a player may have in choosing a strategy; one is the so-called "rational" (i.e., optimizing) motivation of increasing his own payoff *assuming the other player's strategy to be fixed;* the other is the so-called "inducement" motivation of choosing so as to increase one's own payoff *assuming that the other player will be "rational"* (i.e., will optimize). The specific difficulty revealed by my theorem is that a sure-thing strategy, which is undoubtedly *best* from the "rationality" viewpoint, is *worst* from the viewpoint of "inducement": the theorem states that a sure-thing strategy always induces the best equilibrium *for the other player.*

In light of this theorem one cannot assert that there is no difficulty (or a negligible one) with sure-thing strategies unless one asserts that "inducement" as a motivation is negligible. On the other hand, whether the difficulty is serious enough to constitute a "breakdown" is perhaps arguable. Harsanyi, however, in his communication does not discuss the matter at all. He fails to mention either the "inducement" motivation, or the fact that sure-thing strategies are the worst possible ones from this viewpoint. Instead, he points out a certain "rational" characteristic of sure-thing strategies.

Although he does not describe this characteristic in general, but only in reference to a particular 2-by-2 game that he uses as an illustration, I understand him to have in mind the following generalization: If B is a sure-thing strategy for Player 1, and if (B, Y) is an equilibrium more favorable to Player 2 than an equilibrium (A, X) that is more favorable to Player 1, then *any* positive probability of Y being played will cause strategy B to have a higher expected payoff for Player 1 (and therefore to be more "rational" for him) than strategy A; whereas there will be a *range* of probabilities of A being played such that Y will be more "rational" than X for Player 2.

This, of course, is true. It indicates, correctly, that the rationality of (A, X) for Player 1 is tenuous, and easily upset. Hence it strengthens the rationality argument for the sure-thing strategy B, and hence strengthens, rather than weakens, the third breakdown! For the breakdown, as noted above, consists precisely in the fact that *the worst choice from the "inducement" viewpoint* (which is the sure-thing strategy) is precisely the *most* rational choice! Thus Harsanyi, though he does not discuss the "third breakdown," produces an argument that, by underlining the "rationality" of sure-thing strategies, actually tends to underline the "breakdown" of rationality. For the rationality of the sure-thing approach only makes it the more disconcerting that what is the *best* strategy from the viewpoint of rationality is the *worst* from the viewpoint of inducement.

It is interesting to note what happens if we apply Harsanyi's probabilistic type of analysis to the "inducement" justification for strategy A (the justification that Harsanyi ignores). From the "inducement" viewpoint, what matters to Player 1 is Player 2's subjective probability that he (1) will change his strategy from A to B. Player 1, in order to "induce" the equilibrium (A, X), must try to make this subjective probability as low as possible. We find that it may rise quite high (in Harsanyi's example, up to 1/2) before it reaches the level at which Player 2 ceases to be "induced" to choose strategy X. Thus the "inducement" justification for strategy A is not tenuous at all! Indeed, it has the same strength as Harsanyi's argument would give to the "rationality" justification for Player 2 to choose strategy Y.

Incidentally, Harsanyi's argument that Player 1 in his example is in a "*much weaker* bargaining position" than Player 2 is remarkable in one respect: it contradicts the well-known "Harsanyi-Zeuthen" criterion for assessing players' bargaining positions in a conflict between two equilibria. According to this criterion (Harsanyi, J. C., "A Solution for Non-Cooperative Games," in *Advances in Game Theory*, Annals of Mathematics Studies No. 52, Princeton, 1964), the players' bargaining strengths are in fact equal. Thus Harsanyi's argument contradicts the very criterion he has made famous.

I conclude by giving an example in which (unlike Harsanyi's example) the unreasonableness of singling out the sure-thing strategy as the unique solution is rather obvious. I suggest that in the following game one would hardly expect that row player would invariably submit meekly to the (1, 5) equilibrium.

(1,5)	2,4	3,3	4,2	5,1
0,0	(2,4)	0,0	0,0	2,4
0,0	0,0	(3,3)	0,0	3,3
0,0	0,0	0,0	(4,2)	4,2
0,0	0,0	0,0	0,0	(5,1)

NIGEL HOWARD
The Conan Institute, Ottawa

TO THE EDITOR:

In my last letter I discussed a simple two-person game with two pure-strategy equilibrium points, viz. (A, X) and (B, Y). The former is more favorable to player 1, while the latter is more favorable to player 2. I have argued that nonetheless player 1 will have to accept the second equilibrium point (B, Y), even if it is less favorable to him. This follows from the sure-thing principle (since B is player 1's sure-thing strategy). More importantly, it follows from the fact that player 1 is in a much *weaker* bargaining position because he could use strategy A, associated with his favorite equilibrium point (A, X), only if he were *absolutely sure* (with a subjective probability one) that player 2 would also use the strategy X associated with (A, X). In contrast, player 2 can afford to use the strategy Y associated with his favorite equilibrium point (B, Y), even if he is *far from sure* that player 1 will likewise use the corresponding strategy B (so long as player 2 assigns at least probability 1/2 to this possibility).

Howard does not deny the cogency of this argument in terms of what he calls the "rationality" criterion. But he claims that in terms of another, equally valid, strategical criterion, which he calls the "inducement motivation," it is irrational for player 1 to accept the equilibrium point (B, Y) less favorable to him. This is so because player 1 *might* be able to get player 2 accept equilibrium point (A, X). He might be able to achieve this by convincing player 2 that he (player 1) would stick to strategy A anyhow—or, more specifically, by inducing player 2 to assign a subjective probability higher than 1/2 to this possibility. According to Howard, this means that there is a real paradox here: one rationality criterion (which Howard simply calls "rationality") suggests that player 1 should accept the less favorable outcome (B, Y), whereas another rationality criterion (which Howard calls "inducement motivation") suggests that he should not.

My own view is that there is nothing paradoxical in the situation—for the simple reason that Howard's "inducement motivation" is not a valid rationality criterion at all. This is so because player 2, if he is rational, will never take seriously any possible threat by player 1 to stick to strategy A in order to enforce the outcome (A, X)—since the obvious weakness of player 1's bargaining position will make such a threat wholly incredible. The point is that, in this game, *one* of the two players *must* accept a relatively unfavorable outcome: but then this must be precisely player 1 because he is obviously in a much weaker bargaining position.[1] This may be a very unwelcome conclusion from player 1's point of view, but there is nothing paradoxical about it.

The numerical example that Howard adduces in his last letter to the editor does nothing to change this conclusion. In his example, there are five equilibrium points in pure strategies. Of these five, the first one, yielding the payoffs (1, 5), is the least favorable to player 1. Yet, owing to the extreme weakness in his bargaining position, player 1 can hardly do anything else but accept this equilibrium point as the outcome of the game.

At first it may seem surprising that he should have to accept such an extreme and highly unfavorable outcome as (1, 5), instead of such a "reasonable compromise" outcome as (3, 3). But, on further reflection, one can easily see that this is the case. As simple computation shows, if player 1 feels even the slightest uncertainty about player 2's likely strategy choice then he cannot avoid choosing his first strategy, associated with the very unfavorable outcome (1, 5) under discussion. He could choose any other strategy only if he were *absolutely sure* (with subjective probability one) that his opponent would also choose the corresponding strategy. In the contrast, player 2 can afford to choose his first strategy—associated with the outcome (1, 5) very favorable from his point of view—even if he is *far from sure* that player 1 will also choose his first strategy. (For example, player 1 can use his first strategy even if he assigns only probability 1/2 to player 1's using his first strategy, and then divides the remaining probability 1/2 equally among player 1's other four strategies, etc.)

In other words, (1, 5) is the *only* possible outcome if the players start out (as they must) with some—however small—initial uncertainty about each other's strategies. This fact simply rules out any other one of the four alternative equilibrium points as a possible outcome for rational players.

To conclude, in our examples there may very well be a conflict between what Howard calls "rationality" considerations and what he calls "inducement" considerations. But this does not in any way represent a conflict between two different aspects of rationality itself, simply because Howard's "inducement" considerations are not a valid rationality criterion. For instance, in my own example, player 1 could induce player 2 to use strategy X only if he himself could credibly threaten to use strategy A no matter what—but,

[1] In my opinion, the probabilistic considerations described above clearly demonstrate the extreme weakness of player 1's bargaining position. However, Howard is right in pointing out that the concept of "risk-dominance" as defined in an earlier paper of mine (in *Advances in Game Theory*, Princeton: Princeton University Press, 1964) is not sensitive enough to bring out this asymmetry in the two players' bargaining positions.

as we have seen, given the extreme weakness of player 1's strategical position, in actual fact such a threat by him would lack any credibility.

This, however, means that we have no paradox whatever here, and Howard's third "breakdown" of rationality is just as spurious as the first and the second "breakdowns" are.

JOHN C. HARSANYI
University of California, Berkeley and
University of Bielefeld

Political Dictionaries: A Bibliographical Essay*

CLEMENT E. VOSE
Wesleyan University

The editors and authors of these ten books call them dictionaries. All the same, these political dictionaries must not be confused with a standard desk dictionary, let alone the massive unabridged kind. *Webster's Eighth New Collegiate Dictionary* (1973) has 150,000 entries with 27,000 usage examples which, with some additions, is based on *Webster's Third New International Dictionary* (1969) with 450,000 entries and 200,000 usage examples. The largest of the volumes reviewed here has fewer than 4,100 entries, and most have far fewer, so that the books are not comprehensive in the sense of exhausting their subject and capturing all possible terminology.† It remains true that the more specialized books here, limited to definite subjects, provide cohesive and ample treatments. This is particularly true for Abrams on cities, Luttwak on armaments, and Safire on American political catchwords and usages. They are really books that could have been cast in other forms than dictionaries, but their authors simply found it attractive to do their work in this format rather than as one-act plays or videotape quiz shows. Theirs was an artistic judgment. Having chosen the dictionary form, they then "do their own thing" with it. They cover many more words and subjects than indicated by a count of the entries because they go into so much more depth than the ordinary dictionary attempts. The resulting art form is really different from a dictionary, and as such it seems to me to succeed when it is handled both with imagination and competence. It fails when taken too literally and given insufficient infusions of fresh air and individuality.

Dictionary making is so likely to be associated with great institutional edifices like Merriam-Webster's unabridged and the eleven-volume Oxford English Dictionary that it is well to hail, if critically, this batch of modest hand dictionaries from the cottage industry sector. Their publishers have doubtless come to believe that acquisition librarians are more likely to give the nod to a book that purports to be a dictionary than a book cast in the form of numbered chapters. Although all of the books here deal with concerns of substantial interest to political scientists, they have little else in common aside from their alphabetical organization.

The editorial apparatus in these dictionaries affords some measure of editorial patience, thoughtfulness, and sophistication. The preliminaries of a preface or foreword and an introduction explain the editorial purpose and plan and, where missing, forecast trouble ahead in the text. Cross-references must be built into the text itself. This feature is absent only in Dunner, an omission explained possibly by the book's being a compilation of entries written by about 200 different political scientists; these authors were not able to refer to each other, and this deficiency was not remedied by the editors. Discussion of method and of sources usually appears in the back matter, along with an index. A judgment of the quality of each apparatus contributes silently to the rating for editing of each dictionary. (See Table 1.) It should only be added that the Cranston and Lakoff *Glossary of Political Ideas* deals with such a small number of topics as to obviate the need for complex editorial tools. By the same token explanatory features become all the more essential as the entries rise to more than 1,000 for Laqueur and for Plano and Greenberg; to nearly 3,000 for Dunner; and to 4,000 in Smith and Zurcher.

**The Language of Cities: A Glossary of Terms.* By Charles Abrams. (New York: Viking Press, 1971. Pp. 365. $10.00.)

A Glossary of Political Ideas. Edited by Maurice Cranston and Sanford A. Lakoff. (New York: Basic Books, 1969. Pp. 180. $5.95.)

Dictionary of Political Science. Edited by Joseph Dunner. (Totowa, N.J.: Littlefield, Adams & Co., 1970. Pp. 585. $3.95, paper.)

A Dictionary of Politics, 6th ed. By Florence Elliott. (Baltimore: Penguin Books, 1969. Pp. 480. $2.54. paper.)

A Dictionary of Politics. Edited by Walter Laqueur. (New York: Free Press, 1971. Pp. 593. $14.00.)

A Dictionary of Modern War. By Edward Luttwak. (New York: Harper & Row, 1971. Pp. 224. $7.95.)

The American Political Dictionary, 3rd ed. By Jack C. Plano and Milton Greenberg. (Hinsdale, Ill.: Dryden Press, 1972. Pp. 462. $5.95, paper.)

A Dictionary of Political Analysis. By Geoffrey K. Roberts. (New York: St. Martin's Press, 1971. Pp. 229. $8.95.)

The New Language of Politics: A Dictionary of Catchwords, Slogans & Political Usage. By William Safire. (New York: Collier Books, 1972. Pp. $4.95, paper.)

Dictionary of American Politics, 2nd ed. By Edward C. Smith and Arnold J. Zurcher. (New York: Barnes & Noble, 1968. Pp. 434. $2.50, paper).

† Where references to the dictionaries may be located alphabetically, no page number is given.

Table 1. Editorial Apparatus in Selected Political Dictionaries

	Total Entries	Preface or Introduction	Method Essay	Essay on Sources	Index	Cross References	Editorial Ranking
Abrams, *Language of Cities*	979	+	+	+	+	+	2
Cranston & Lakoff, *Glossary of Political Ideas*	52	+	−	+	−	+	5
Dunner, *Dictionary of Political Science*	2768	+	+	−	−	−	9
Elliott, *Dictionary of Politics*	952	−	−	−	−	+	10
Laqueur, *Dictionary of Politics*	1433	+	−	−	−	+	7
Luttwak, *Dictionary of Modern War*	539	+	+	+	−	+	4
Plano & Greenberg, *American Political Dictionary*	1143	+	+	−	+	+	3
Roberts, *Dictionary of Political Analysis*	477	+	+	−	−	+	6
Safire, *New Language of Politics*	845	+	+	+	+	+	1
Smith and Zurcher, *Dictionary of American Politics*	4080	+	−	−	−	+	8

From all of this it must not be assumed that the apparatus provided tells the full story of quality; it just gives a hint.

The author of a monograph will ordinarily take some pains defining old words and introducing new ones with careful attention to meaning. Good books are strewn with definitions and explanations of words. By this measure many books could be dictionaries and vice versa. It is quite easy to imagine Abrams, Laqueur, Luttwak, or Safire presenting their ideas in other than alphabetical order. Abrams's book is about the impact of various public policies on the economic and social life of cities around the world. Laqueur's book is about the course of ideological conflict among nations since the Russian Revolution with particular attention to the political leaders embroiled. Luttwak concentrates on national rivalries almost exclusively and assesses power capability at strategic rather than tactical levels. Safire has really written a history of the place of charismatic personalities in the communications network of American political life.

Individuals who write dictionaries often do so with some passion, and this is most evident in the works here of Abrams, Laqueur, Luttwak, and Safire. They bring an enthusiasm to their work in the tradition of Samuel Johnson and Henry L. Mencken. Abrams's book will be a joy to students because the brief definitions of "land fragmentation," "noise," "physiographic determination," and "piece of the action" are accompanied by apt historical information and pungent editorialization. The first of these, among his shorter entries, shows how Abrams brings history and current issues together and includes a typical array of cross references:

land fragmentation The division of landholdings through distribution or descent into parcels too small for economical use. In Europe primogeniture and entailment of land concentrated ownership until the French Revolution brought a counter-trend with its own faults—*i.e.*, the division of land from generation to generation until parcels were too small to be economically developed. In France consolidation of uneconomic holdings has been introduced in the hope of eliminating some of the by-products of fragmentation. One of urban renewal's main purposes is to assemble fragmented land and redevelop it into more practicable plots. (See ASSEMBLAGE; ENTAIL; FEUDAL TENURE; LEX ADICKES; REPARCELATION; URBAN RENEWAL.)

Cities are ancient and exist all over the world, and it is fitting that Abrams's cosmopolitan learning cheerfully registers in this book. He infuses it with humanizing references and with incisive explanations of the consequences of "blockbusting" and the "industrial-development bond." He gives the origin of "sector theory" and tells its chief limitation.

sector theory A theory of urban growth that holds that classes of residential uses tend to be arranged in wedge-shaped sectors radiating from the central business district along major transportation routes. Developed by Homer Hoyt in his study of residential areas in the 1930s, the theory holds that high-rent residential areas are prime shapers of city growth and that there is a decline in desirability and value in all directions from them. Different income groups of a city segregate themselves residentially in sectors and then migrate within the same general sector but away from downtown as the sector grows. The middle-income groups attach themselves as closely as possible to the upper-income areas, which usually have the highest and best land, the most prestigious residents, and the best access to other parts of the city. The theory has been criticized by Lloyd Rodwin, who said that Hoyt's conclusions were based on questionable assumptions and on an *ad hoc* theory of class structure. It assumed that locations were selected for class at-

tractiveness while it ignored other considerations such as the functional adequacy of established environments. (See CONCENTRIC ZONE THEORY; GRESHAM'S LAW OF NEIGHBORHOODS; MULTIPLE NUCLEI THEORY.)

As the gusto in all of this suggests, Abrams has prepared a highly personal tour of the urban world from the experience of a man who was at once a lawyer, a political figure, and a careful reader in the literature of city planning, law, history and the social sciences. Abrams commands the respect and gratitude of all who seek to know this vast subject.

Laqueur's *A Dictionary of Politics* arranges itself through cross-references around the great ideological struggles of the past forty years. These are not explicitly suggested but close study shows this to be a highly integrated book. *On Fascism:* Mussolini, Ethiopia, Nazi Party, *Mein Kampf*, Hitler, Concordat, Goebbels, Goering, Munich, Chamberlain, Mosley, Gestapo, Crystal Night, Spanish Civil War, Himmler, Concentration Camps, Holocaust, Eichmann, Churchill, Pearl Harbor, Roosevelt, Axis Powers, Japan, World War II, and Nuremberg War Crime Trials, among others. *On Communism:* Bolshevism, USSR, Lenin's Testament, World Communism, Sovietisation, Purges, Moscow Trials, Show Trials, Slave Labor, Pyatakov, Kaganovich, Khrushchev, Tito, Yugoslavia, Czechoslovakia, Kuomintang, Mao Tse-tung, Chinese People's Republic, Sino-Soviet Conflict, Lin Piao. *On American Policy:* Cold War, McCarthyism, HUAC, Truman Doctrine, Marshall Plan, Korea, Eisenhower, Kennedy, Fourteenth Amendment, Martin Luther King, Stokely Carmichael, John Birch Society. Similar strings of entries spell out the development of Zionism and the Arab response; Colonialism and the Third World; tensions in East Europe; and conflict over the unification of Europe. Laqueur says a good deal, too, on the theme that domestic politics affects foreign policy. This is particularly evident in the treatment of several personalities, institutions and incidents in the domestic political life of France and the United States.

Luttwak, Deputy Director of the Middle East Study Group in Jerusalem, brings an impressive intellect and a strong interest in specifics to the universe of modern war and weaponry. His essay on "war" is a concise philosophical statement about the four rival concepts, hardly precise parallel categories, of von Clausewitz, Marxist-Leninists, pacifists, and peace-researchers. This discussion leads into "total war," "central war," "deterrence," "controlled response," "propaganda," and "political warfare." There are also many articles on particular international incidents or events such as "Warsaw Pact." But the passion shows itself in the fastidiousness of detail in the more technical articles. A book like this relies on other sources, and Luttwak's excellence is rooted in his skeptical, intelligent handling of those sources. Thus he observes that *JANE'S All the World's Aircraft* and related reference sources do not assess the claims of manufacturers. He praises the timeliness of another reference source, *The Military Balance*, issued annually by the Institute for Strategic Studies in London. Luttwak's feeling for words is expressed in his foreword:

> For obvious reasons, there is a shortage of reliable information about the more advanced weapons. The Russians publish almost nothing, and while Western arms' manufacturers do publish a great deal, many of them also cultivate "brochuremanship," where extravagant claims are camouflaged under the pseudo-technical language fashionable in military circles. ("Advanced" means that it does not work yet; "semi-automatic" means that everything has to be done by hand; "anti-tank secondary kill capability" means that only a hero or a fool would approach a real-life tank armed with one of those . . .) Information about Soviet weapons comes almost entirely from intelligence reports which are leaked, selectively, by the Western military. See *Scarp* for a Soviet I C B M whose performance details were published when the Pentagon had to prod a reluctant Congress to release funds for the *Safeguard* missile defence system.

Luttwak explains that modern weapons have longer lives than is generally thought, and this assertion is proved convincingly in the entry for "B-52" which is cross-referenced to "stratofortress" used in the December 1972 bombing of Hanoi and Haiphong. Detailed specifications of size, range, cost, numbers, load, and so on are included for this and other weapons.

Luttwak suffers from "lexicographer's palsy" when he comes to attempt a definition of "aggression."

> Aggression. The UN special committee working on a definition of Aggression has been unable to produce one after about eighteen years of deliberation. No definition is given here; everybody knows what the word means.

This is hardly helpful in a dictionary on war. A superior approach would explain *why* representatives of competing nations in the United Nations propound rival criteria for aggression and what the problems are with so-called neutral definitions of this and other political words; it would then proceed to lay down alternative definitions. Gertrude Stein may know that aggression is aggression is aggression, but when the dictionary makers announce that "everybody knows what the word means," they can close shop.[1]

[1] After the present essay was completed, the U. N. Special Committee on the Question of Defining Aggression agreed upon a draft definition of the word, sub-

The high quality of Luttwak's attention to the military usage could be shown by many examples. Here is his treatment of a term that has entered popular language but is not to be found in any of the other dictionaries:

Fail-safe. A procedure whereby US strategic bombers with nuclear weapons proceed to target only if specific orders to do so are received at a certain point on their intended path. These communication arrangements are designed to prevent a nuclear strike resulting from technical failure. The alternative procedure is "recall," where the delivery system proceeds as programmed unless actively recalled. Fail-safe was introduced by the U S Strategic Air Command as a replacement for the traditional recall procedure used in non-nuclear bombing. In S A C it is known as "positive control."

Most of the entries for weapons are too long and detailed for reproduction here, but partial quotation from one shows Luttwak's style, his skill at conciseness and detail:

"Frog." Soviet surface-to-surface unguided rocket series. Frog (Free Rocket Over Ground) is the N A T O designation for a series of Soviet unguided rockets similar in concept to the U S Honest John (M G R-1). These spin-stabilized rockets are sufficiently accurate for nuclear bombardment of rear areas, and are seen as the modern counterpart of classic heavy artillery.

Frog 1 First of the series and in service since 1957. Six tail fins, solid-propellant spin-stabilized sustainer, no booster. It is 31 ft long, weighs about 6,000 lb and has an estimated range of 40 miles. It is carried on, and launched from, a converted heavy tank chassis.

The treatment of "Frog" continues through "Frog 7" which was first seen in 1965. The entry concludes with a cross-reference to "Scud, Shaddock, and Ballistic Missile for Soviet missiles." There are no direct references to the finely printed photographs of ninety-six weapons in the back of this book but No. 91 shows the "Honest John MGR-1"; No. 92 is "Frog 7"; with additional chilling photographs of "Scud," "Minuteman," "Polaris" and "Poseidon."

Safire's dictionary treats everything in sight with generosity. His accurate subtitle calls the book *A Dictionary of Catchwords, Slogans & Political Usage*, and a long introductory essay and final note on sources and research, plus a bibliography and index to names, make the book a treat to read as well as a durable reference book. The index to names converts this into an excellent book of quotations, one that remedies Bartlett's omission of notable *political* sayings. Innumerable entries give a history that reaches out with a cascade of observations as, for example, with "MY FRIENDS" which starts with FDR, shows that Horatio Seymour used the greeting in 1863, and unwinds with a discussion of the subject of salutations generally, with references to John Witherspoon, Napoleon, Chief Tecumseh, Jefferson, Taft, Kennedy, Truman and Willkie. This entry concludes with details about Roosevelt's apocryphal "Fellow immigrants" address to the Daughters of the American Revolution, April 21, 1938, where the President simply said: "Remember, remember always, that all of us, and you and I especially, are descended from immigrants and revolutionists." There are entries for "LIMOUSINE LIBERAL," "MANIFEST DESTINY," "MEDICARE," "MILITARY-INDUSTRIAL COMPLEX," "MOONLIGHTING," "ONE MAN, ONE VOTE," "RIGHT TO KNOW," and "PENTAGONESE." The book is written by a journalist for readers interested in the development of American political jargon. It is not a political science or an academic book. It is to be highly recommended for its thoroughness, the detail with which entries have been researched and the generous way in which a coinage is turned around prismatically for examination.

Its title to the contrary, Florence Elliott's book is of relatively little interest or value to students of politics. In saying Lyndon B. Johnson "did not contest the elections of November 1968," the author treats us to a correct statement that is an inadequate reading of the truth. In a curious gratuity, Elliott gives Harry S Truman a middle name, Swinomish, when he had none. This dictionary seldom touches political terms and does not tell what precisely is political about its entries on individuals, nations, and events. Its discussion of "League of Nations," "Luxemburg," and "Mendès-France" are barely more than what a fair world almanac would provide. Elliott and Laqueur use the identical title, *A Dictionary of Politics*, and both books are intended to cover the same nations, institutions, ideologies, conflicts and personages in the recent past. Both have an English or European vantage point. Here the similarity ends, simply because Laqueur is interpretive and Elliott is not. Elliott's book in effect is a rather poor World Almanac for 1968–69, and it is simply audacious commercialism that it should be portrayed as *A Dictionary of Politics*.

A Glossary of Political Ideas is a collection of

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mitting it for adoption by the General Assembly at its September, 1974 session. The draft is divided into eight articles containing seventeen paragraphs, too lengthy for quotation here. Article 1 begins: "Aggression is the use of armed force by a state against the sovereignty, territorial integrity or political independence of another state or in any other manner inconsistent with the Charter of the United Nations, as set out in this definition" (*New York Times*, April 13, 1974, p. 6). Details on the history of the Special Committee may be found in numerous official publications such as *Yearbook of the United Nations 1971*, v. 25 (United Nations, N. Y.: Office of Public Information, United Nations, 1974), pp. 597–601.

terse essays by fourteen British social scientists edited by Maurice Cranston and here edited with additions for American readers by Sanford A. Lakoff. The first of fifty-two entries are "Anarchism," "Authority," "Coexistence," "Colonialism," "The Common Good," "Communism," and "Conservatism." The treatment is in the conventional mode of classical political philosophy with "Equality" never reaching the twentieth century. Yet the book includes brief entries for "Titoism" too sparse to be useful and one for "Trade Unionism" that is too British to be of any service to a student of American institutions. There is a bibliography following each entry. It isn't at all clear what this book is intended for. The entries are less complete than those on nearly every similar topic in the *International Encyclopedia of the Social Sciences* and of the newer four-volume *Dictionary of the History of Ideas*. There is not enough space available for an author to get off the ground. The result is a book of marginal value to a student beginning to think about the great subjects of political philosophy and of even less interest to a well-read scholar. This Cranston and Lakoff dictionary is competent and reliable, in contrast to Elliott, but it is not imaginatively conceived or energetically executed.

The three dictionaries by Dunner, by Plano and Greenberg, and by Smith and Zurcher have value for the audience of beginning students whose political memories barely reach back to the assassination of President Kennedy. There are hundreds of words that apply to some event or some recurrent matter in the history of American politics. It is a conceit of age to note rudely youth's ignorance of Joe McCarthy's "point of order" or of the Eisenhower delegates' "fair play amendment." These books will surely instruct undergraduate students of American government and politics. They simply cannot be endorsed as the kind of quality political dictionary graduate and other professional students of politics should use.

The Dunner dictionary is the work of many hands, with very brief entries for its many items about world and American history and politics. It is concerned with events of the early years of the Republic as much as it is of recent matters. Except for a worthwhile introductory essay on political science by Dunner, there is very little social science in the book. It does contain longer and better entries for the same items than will be found in Smith and Zurcher. Dunner is also distinguished by its many brief biographies.

Biographical entries appear in three of the political dictionaries but not in the other seven. (See Table 2.) Those in Laqueur are ably researched and written to stress the political significance of the personages described. Those in Dunner contain greater biographical detail, are more concise and often capture in just a few words the key point about an individual. The biographical portraits in Elliott are weakly drawn and omit vital details as well. Mostly, all of these entries are inferior to the short biographies in the *Columbia Encyclopedia* (3rd ed., 1963). In other words, they may jog one's memory in an emergency, if they are handy, but the biographical entries in these dictionaries are not to be substituted for a quick piece of library research. On the positive side, biographical entries inform us that human beings coin political words and develop political ideas as well as act in political life. The inclusion of biographies reduces the function of these dictionaries for the exacting work of word definition and fails, partly by attempting too much that is diversionary.

Plano and Greenberg have selected 1143 terms, agencies, court decisions, and statutes "for their pertinency to the general introductory course in American government and to an understanding of contemporary political events" (preface, p. v). A strong topical ordering gives this book the flavor of a basic text rather than a mere companion. As such, the authors provide no explicit story line but present a bland system of government with few rifts. Societal traits and tensions are ignored, and the tour of federal agencies is comparable but far less comprehensive than that provided by the *United States Government Organization Manual*. The entries in a chapter titled "political ideas" barely carry the subject into the twentieth century, which is to say that there is essentially nothing of the behavioral persuasion in political science in this book. The comments on "ideology" and "pluralism" give no hint of the disputations in the discipline over the proper application and utility of such terms. The book becomes a catalog of familiar enough terms, informing or reminding the nonspecialist about the *Wong Kim Ark* case, the Ramspeck Act and the Hare Plan. Like many books with technical inadequacies, this dictionary does not point the reader beyond itself; it gives citations to court cases but not to statutes and rarely tells where the information presented in each entry came from or where one can look further.

The fault of all three of the dictionaries of American government is not their limited size but in the way many items are treated. After the judicial revolutions since 1937 it is misleading to discuss "precedent" and "*stare decisis*" without an entry for "overruling," accompanied by the statistic that the Supreme Court has overturned its own precedents on more than one hundred occasions. It would not be amiss, further, to observe that many of these decisions have taken a form that many call "judicial legislation" and to make all of this clear through ample cross-references.

Not only the words included in these diction-

Table 2. Political Dictionaries with Biographical Entries

Editor and Title	Total Entries	Total Biographies	Per Cent Biographies
Dunner, *Dictionary of Political Science*	2768	549	20
Elliott, *A Dictionary of Politics*	952	140	15
Laqueur, *A Dictionary of Politics*	1433	454	32

aries but also the omissions betray the absence of a sense of history, of a theory of political power and development, and of an attendant concern for exactness. That government contractors have special obligations could not be learned here. Yet, long before 1965 when "affirmative action" was coined to expand employment pools to hire without regard to race or sex, the federal government was using its contracts to dictate wages, hours, loyalty standards, and other employment policies to numerous corporations.

Although Roberts's ideal is more precise usage, his *Dictionary of Political Analysis* is offered as "a mirror of the present vocabulary of the discipline, not a proposal for a future technical word list from which all ambiguities and imprecisions have been removed" (introduction, p. xx). There is much more here than words of political analysis such as "model" and "attitude scaling": as the book treats "Zionism" and many other political movements as well. It is notably weak in entries on American political institutions as seen by making "Electoral College" in the United States equivalent to the College of Cardinals, whereas Corwin once explained definitively that this is "actually a congeries of 'colleges'."[2] Roberts is another victim of the well-known "introduction to politics syndrome" which has felled many well-intentioned scholars. False parallels are simply one symptom. Another is the effort to provide a denotative definition for a word after insufficient study of its full history and use. "Judiciary" is an example.

Roberts does make an admirable start in introducing the subject of contemporary political science in dictionary form. He includes "gatekeepers" and refers the reader ahead to "systems analysis," which is discussed in a brief essay. The right distinction is drawn between "paradigm" and "ideal type." There is a good definition of "interest articulation" and the only adequate mention of "community studies" in any of these dictionaries. Political scientists wishing to catch up to the growth of the behaviorial persuasion might use Roberts's book as a Berlitz guide to "cohort analysis," "computer utilisation," "conversion process," "Delphi method," "droop quota," "funnel of causality," "grounded theory," "ladder of values," "longitudinal studies," "macro-politics," "metapolicy," "Pareto optimum," "pillarisation," "psephology," "reductionist theories," and "teleological explanation." Many of Roberts's entries are a page or two long, and practically all have cross-references. The best entries contain citations that take the right path as far as they go. Books in point are mentioned but we need page numbers and journal articles as well, about which there will be more further along.

Having carped at some authors for neglecting the discipline of political science, I must fault Roberts for inadequate handling of established legal and political usage. "Conspiracy," "sedition," and "civil rights" might just as well be omitted as given such cursory treatment. They are not words of political analysis in any event, and their meanings in different times and places give them none of the traits possessed by such terms as "attitude scaling." The political scientist as historian and observer of the passing parade will inform himself of the British Secrets Act and of "shield laws," but the kind of dictionary Roberts provides will not carry him far. The political scientist as analyst, on the other hand, has an entirely different intellectual need which Roberts seeks to serve.

As a literary genre, the dictionary of politics is largely uninterested in the history of the words it treats. In his final book, E. E. Schattschneider asserted that political science was impossible a century ago because its vocabulary was inadequate. We have had to borrow. He shows as convincingly as anyone could that the words of politics are derivative, most being old words which have acquired new meanings. Schattschneider asked:

> Where do the new words come from? They come from everywhere. Some are figures of speech, as machine, ring, heckle. Some are derived from the place in which activities are carried on, as cabinet, house, or lobby, or from a seating arrangement, as left and right. Some come from the church: hierarchy and propaganda. Many come from military life: campaign, filibuster, insurgent, picket and partisan. A host of political words are made by adding a new ending to an old word: anarchy becomes anarchism, isolate changes to isolationism, nation to nationalism, total to totalitarianism, and so on.[3]

[2] Edward S. Corwin, *The President: Office and Powers, 1787–1957*, 4th ed. (New York: New York University Press, 1957), p. 39.

[3] E. E. Schattschneider, *Two Hundred Million Americans in Search of a Government* (New York: Holt, Rinehart and Winston, 1969), p. 14.

The common words that have acquired special meaning through political usage present one difficulty. But the derivations of words peculiar to political science require careful analysis and attention to give us a clear measure of their meaning. However well versed in contemporary political science, these lexicographers, save for Roberts, are not dependable teachers of this subject. If they were, they would have done a number of things. First, they would have had to ascertain the universe of publications in which to search for words. The word list need not be comprehensive; seasoned judgment in building a list would be quite acceptable. But one would hope for a careful scanning of, say, the leading texts in the subfields of political science; the leading articles such as those in the Bobbs-Merrill Reprint Series; a checklist of classics and key monographs; and the suggestions of a band of specialists with varied orientations and political dispositions. If there is to be a new wave of dictionaries of political science which inform us all and build upon each other, it will be essential that the sources scoured for words be named.

Second, the words must be defined in context and their history indicated so that their functional and operational meaning can be grasped by the novice. A splendid instance of this is provided by Frank J. Sorauf in his text, *Party Politics in America*, by his explanation of the word "stratarchy."[4] He gives us both a connotative and denotative treatment and, in a footnote, tells something of the history of its usage.

> The distribution of power within most American party organizations can be described in Samuel Eldersveld's apt term "stratarchy." It is "the enlargement of the ruling group of an organization, its power stratification, the involvement of large numbers of people in group decision-making, and, thus, the diffusion and proliferation of control throughout the structure."

His citation is then as follows: "Eldersveld, *Political Parties: A Behavioral Analysis* (Chicago: Rand McNally, 1964), p. 99. The term 'stratarchy' comes from Harold Lasswell and Abraham Kaplan, *Power and Society* (New Haven, Conn.: Yale University Press, 1950)." In a dictionary of politics one would attempt to go further with "stratarchy," to determine how Lasswell and Kaplan picked it up, to ask whether this is yet another borrowing from science—from "strata" in geology, perhaps, as my former colleague David Adamany once suggested to me.

Third, in reaching out, our hypothetical dictionary maker would have cosmopolitan tastes and unbounded curiosity. Not one of these ten dictionaries mentions "true believer" from Eric Hoffer's book of the same title.[5] This is indicative of a too-narrow preoccupation with the words of formal government and the language of dues-paying political scientists.

Fourth, both jargon and humble usages, which are part of the speech of political scientists who forget or never knew where the words came from, could be captured in a carefully done dictionary. Remember V. O. Key's "friends and neighbors" in his treatment of Alabama politics?[6] Other words include "polyarchy" made famous by Robert Dahl.[7]

Fifth, we need rival definitions of our most used words such as political parties. The first thing to say about "political party" surely is that the most thoughtful students of the subject prefer differing definitions. Alex Gottfried in Dunner comes closest to saying this by quoting Edmund Burke but without a reference or date. He then gives an alternative. But why turn to a political dictionary when a textbook does the job better? In his parties text, Sorauf does what the dictionary makers should do by listing a variety of definitions of the political party in a way that is so simple and clear that it deserves quotation here:[8]

> Party is a body of men united, for promoting by their joint endeavors the national interest, upon some particular principle in which they are all agreed. Edmund Burke, *Thoughts on the Cause of the Present Discontents* (1770).
>
> . . . We may define "political party" generally as the articulate organization of society's active political agents, those who are concerned with the control of

[4] Frank J. Sorauf, *Party Politics in America*, 2d ed. (Boston: Little, Brown and Company, 1972), p. 110.

[5] Speaking of the active, revivalist phase of mass movements, Hoffer states that this phase is dominated by the "true believer" defined as "the man of fanatical faith who is ready to sacrifice his life for a holy cause." Eric Hoffer, *The True Believer: Thoughts on the Nature of Mass Movements* (New York: Harper and Brothers, 1951), p. 10.

[6] The "friends and neighbors effect" is Key's phrase for a powerful localism in Alabama factionalism where "candidates for state office tend to poll overwhelming majorities in their home counties and to draw heavy support in adjacent counties." Key believes this "points to the absence of stable, well-organized, state-wide factions of like-minded citizens formed to advocate measures of common concern." V. O. Key, *Southern Politics in State and Nation* (New York: Alfred A. Knopf, 1949), p. 37.

[7] The idea of employing the word "democracy" to connote the ideal and "polyarchy" the real, the latter to serve as a measure of achieving the former appears first in Robert A. Dahl and Charles E. Lindblom, *Politics, Economics, and Welfare: Planning and Politico-Economic Systems Resolved into Basic Social Processes* (New York: Harper & Row, 1953), pp. 272–323. Dahl has reintroduced "polyarchy" instead of the word "pluralism" in the latest edition of his text, Robert A. Dahl, *Democracy in the United States: Promise and Performance*, 2d ed. (Chicago: Rand McNally and Company, 1972), p. vii.

[8] Sorauf, p. 8.

governmental power and who compete for popular support with another group or groups holding divergent views. Sigmund Neumann, *Modern Political Parties* (Chicago: University of Chicago Press, 1956), p. 396: italics omitted.

. . . Any group, however loosely organized, seeking to elect governmental office-holders under a given label. Leon D. Epstein, *Political Parties in Western Democracies* (New York: Praeger, 1967), p. 9.

An "established political party" is hereby declared to be a political party which, as to the state, at the last general election for state and county officers, polled for its candidate for governor more than two per cent of the entire vote cast for governor in the state. Missouri Statutes (V.A.M.S. sec. 120.140, para. 2).

Sorauf also includes a longer section of the Delaware statutes and thus respects the reader's intelligence with a true variety. He also demonstrates the value of an up-to-dateness by including the "label definition of party" developed by Leon Epstein.

Dunner as well as Roberts pays attention, and sometimes impressive attention, to the behavioral persuasion. I was gratified with the treatment of "Games, Theory of" by Harold Guetzkow in Dunner, as he provides us with a coherent two-paragraph history of the subject with comments on its applications, uses and limitations. He begins with this definition: "A theoretical or normative branch of mathematics and economics in which treatment is made of conflict of interests by the formalization of strategies." He mentions Shapley, Shubik, and Riker but without citations. The absence of cross-references in Dunner is a serious hindrance compounded in this instance by the lack of related entries such as "prisoner's dilemma" which is quite fully discussed by Roberts. But Roberts also fails us by supplying no references whatever for this entry. The uninformed reader, whose curiosity is aroused, had better not be stuck with these dictionaries on a desert isle. I have had better fortune in happening to read John Rawls's *A Theory of Justice*, in which he discusses this matter almost in passing: "The prisoner's dilemma (attributed to A. W. Tucker) is an illustration of a two-person noncooperative, nonzero-sum game; noncooperative because agreements are not binding (or enforceable), and nonzero-sum because it is not the case that what one person gains the other loses."[9] He goes on at length and cites books and articles by W. J. Baumol, R. D. Luce and Howard Raiffa, and D. P. Gauthier.

This borrowing from other disciplines that seems to characterize political science presents the creator of a political dictionary with different choices. If too venturesome, the dictionary will tend to become an unabridged dictionary of the whole language; if too circumscribed, it will be barren of much of the terminology political scientists put to use. In a prize-winning book in American Political Science Association competition, for example, William K. Muir, Jr. teaches us a great deal about the concept of "cognitive dissonance" which, as he says, "deals with the individual's reactions to the costs of exercising choice, after his mind is made is made up."[10] Yet there are other steps needed to legitimize a term borrowed from social psychology such as "cognitive dissonance" to make it one that has a familiar, exact, and useful place in the language of political scientists. The textbook and dictionary writers have a function to play in this task but, again, one must lament that few see the problem, fewer define their mission in this way, and a still smaller number take the pains to keep up with the literature—the language that publishing scholars employ, in fact—and then introduce these words to a wider audience.

The heaviest borrowing by the dictionaries on American government and political science is from law, and a consistent deficiency is the absence of key words and the scant interest in the functional meaning of the words that are chosen. Plano and Greenberg are clearly superior in explaining that the Supreme Court controls its business by handling "certiorari" and that interest groups present their views there as "amici curiae." In Dunner and in Smith and Zurcher, these words are included but given wooden definitions. None but Smith and Zurcher mention the "removal" of cases from state to federal courts, but even they neglect to explain its origin in the Judiciary Act of 1789 and its use by the modern civil rights movement. All omit "barratry," the ancient word for soliciting litigation, and consequently cannot discuss the state legislation against barratry designed to stop NAACP lawyers in the South.

The law entries present still other problems and highlight the disadvantage a lexicographer faces in defining words one-after-another as against a researcher dealing with a few words in the context of a specific inquiry. Consider the doctrine of "dual federalism," so beloved by conservative jurists before the constitutional revolution of 1937, and note what two dictionaries say:

Dual federalism . . . held that a grant of authority to the national government does not destroy local power

[9] John Rawls, *A Theory of Justice* (Cambridge, Mass.: The Belknap Press of Harvard University Press, 1971), p. 269.

[10] William K. Muir, Jr., *Prayer in the Public Schools: Law and Attitude Change* (Chicago: University of Chicago Press, 1967), p. 4. Muir has thus brought to the attention of political scientists and lawyers a term propounded by Leon Festinger in *A Theory of Cognitive Dissonance* (Evanston, Ill.: Row, Peterson, 1957).

reserved to the states. Plano and Greenberg, *The American Political Dictionary*, 3rd ed.

A doctrine, occasionally maintained by the Supreme Court, especially 1836–64, that the powers of the national and State governments are separate and distinct, and that the powers delegated to the national government are limited by the powers reserved to the States under the 10th Amendment. Smith and Zurcher, *Dictionary of American Politics.*

Years have passed since Norman Jacobson told me the most simple and best definition of "dual federalism" was Fellman's. I think it probably still is:

Briefly, the doctrine holds that the mere existence of states constitutes an independent limitation on the exercise of national powers.[11]

If the language of law and politics of an earlier day is not always captured in crisp definitions, there is also failure in handling modern legal usage. There are important lapses in what is included, and because it is unclear what sources are utilized both for the word list and for definitions, the failure sometimes becomes a disaster.

An example of an omitted word is "class action suits" which is nowhere to be found in these dictionaries. Under the *Federal Rules of Civil Procedures* "one or more members of a class may sue or be sued as representative parties on behalf of all," if certain prerequisites are met.[12] Such suits began more than two centuries ago in England and have been used increasingly in the United States in civil rights, voting rights, welfare and environmental cases. It is fair to say that the leading decisions of the Warren Court could not have taken their sweeping character had they not been brought in the form of class actions to apply to others similarly situated with the plaintiffs. This is a fair sample of what is omitted in these dictionaries.

Students of the American constitutional system will find two dictionaries superior in several respects. Plano and Greenberg have divided the material into eighteen topical chapters with the index serving as a full alphabetical guide to entries. The topics are ordered to follow the structure of most textbooks in American government and each entry is composed of two paragraphs, one definitional and the other addressing itself to the significance of the term. This shows relationships well and means that one may profitably, and with a minimum of confusion, actually read the dictionary straight through. Also, important agencies' cases are grouped together toward the end of each chapter, then important cases and important statutes. The new, third edition of Plano and Greenberg offers readers the advantage of up-to-date information about modifications in Supreme Court rulings, especially compared with rival dictionaries. Smith and Zurcher is the only other dictionary that gives much attention to cases and statutes, with Dunner a weak third. (See Table 3.) Smith and Zurcher provide short treatments, but reliable ones, of the cases and statutes treated. They also include the specific dates of statutes but neither they, nor Plano and Greenberg, cite statutes either to the *Statutes at Large* or the *United States Code* while both are predictably fastidious to cite cases to the *United States Reports.*

Commentary about political science and political words will help would-be lexicographers prepare better dictionaries. Examples abound in a book of readings edited by Max J. Skidmore offering some forty essays on rhetoric and politics which begin with George Orwell's "Politics and the English Language." Skidmore makes many points about the rise and fall of political words that show erosion in the English language that had better keep us from joking about the vain attempts of the Académie Francaise to shore up the language of the French. He argues that it is "no longer possible accurately to grasp any sense of meaning from the words 'defense,' 'victory,' 'free world,' 'aggression,' 'national security,' 'patriotism,' 'policy action,' 'pacification,' 'demilitarized,' and even 'orderly withdrawal'."[13] (Evidently we must choose between Luttwak's belief that everyone knows what "aggression" means and Skidmore's conclusion that nobody knows.) The readings are almost all recent, since 1960, and consequently concern the Vietnam war and the prose of Richard Nixon. But there is debate here, as seen in the attack on Martin Luther King's view of war casualties by Ernest W. Lefever and responses to this criticism. There are fine retrospective articles on what happened to the meaning of words in Nazi Germany which imply that cataclysmic politics foster rapid deterioration of language. By reflecting on political words, rather than defining them, Skidmore's book is more stimulating and may be as useful to political scientists and their students as some political dictionaries.

Political scientists do not yet have a dictionary of professional quality. Most efforts are directed toward undergraduates or the general public. To say that the state of a discipline is revealed by textbooks and reference books is probably to claim too much. Monographs and journal articles surely lead the way. It is a misfortune that the

[11] David Fellman, "Federalism," *American Political Science Review*, 41 (December, 1947), 1142.

[12] Fed. Rules Civ. Proc., 23 (1966). See 26 U.S.C.A. for annotations.

[13] Max J. Skidmore, ed., *Word Politics: Essays on Language and Politics* (Palo Alto: James E. Freel & Associates, Inc., 1972), p. 7.

Table 3. Statutes and Court Cases in Selected Political Dictionaries

Editor and Title	Total Entries	Statutes	Court Cases	Total Statutes & Cases	Per cent of Statutes & Cases
Dunner, *Dictionary of Political Science*	2768	34	23	57	2
Plano and Greenberg, *The American Political Dictionary*	1143	58	105	163	14
Smith and Zurcher, *Dictionary of American Politics*	4080	199	234	433	11

dictionaries in political science seldom incorporate the professional findings. There is evidently a place in the market for political dictionaries for undergraduates; it is hoped that political dictionaries can be improved for the whole profession. If that is done, there will be a better basis for preparing dictionaries that are suitable for beginning students, too. Meanwhile, if the form of a book you are planning does not seem to jell, you might think about casting the first draft in alphabetical order and shipping it around the publishing circuit.

Political Implications of Gender Roles: A Review of the Literature*

WILMA RULE KRAUSS
Northern Illinois University

Introduction

The purpose of this essay is to introduce the reader to the contemporary literature on gender roles and feminine behavior, including the major concepts, empirical findings and social thought which have implications for political behavior and research. Generally the emphasis is upon current published writing, although there are exceptions for singular works which it is believed will be useful references. This literature includes reprints of classic statements such as Mill's, Engels's and Putnam's and more than 200 new books, monographs and articles within the past five years. It ranges across disciplines of anthropology, economics, history, psychology and sociology as well as political science; it includes futuristic schemas and offers fresh perspective and new areas for inquiry.

Gender roles as they relate to the psychology and activity of men and women and their systemic cultural, economic, and legal ramifications provide an explanation and a basis for understanding political behavior and thought, including recurrent women's protest movements. Therefore the first part of this essay concentrates on literature which explicates gender roles, life cycles and economic and legal factors which the writer views as independent variables related to political participation. The presentation of material in these areas I believe constitutes a logical progression: once the complex of civic cultural relations is examined, current trends in women's voting, campaigning and officeholding may be understood as "consequence" and "cause."[1] This portion of the literature contributes to building an accurate body of knowledge regarding contemporary political woman, and has potential usefulness for public policy: a significant expansion of democracy is viable with the discernment and removal of the various barriers which have hindered substantial proportions of women from achieving political leadership and hence participating in authoritative decision making and value allocation.

In the next section of this essay, discussion focuses upon works providing background for women's contemporary political behavior. Part two provides an historical sketch of feminine roles in the polity and research and writing on women's political movements of the 19th and 20th centuries. This writing is important for several reasons—there has been little attention given these subjects until recent years; moreover, with few exceptions, social scientific endeavors have been androcentric in focus and viewpoint (Beard, pp. 65–66; Flexner, vii; Lopata, p. 7).[2] Thus these new publications fill a scholarly void, and offer a heretofore rarity—women's interpretations of their sociopolitical history and collective struggles for equality; they may, therefore, be of especial interest to those whose academic concerns encompass political thought and/or political movements.

The origins of gender relationships are explored in the third part of this essay. An essential literature for political scientists' attention, the new writing calls into question the conventional wisdom, that is, the ideology of female inferiority and biological determinism, social constructions which serve as a simplistic, albeit nonscientifically grounded basis for explaining women's past and present political behavior (cf. Bardwick, 1971; Carlson, Tiger). The bibliography which follows is not all-inclusive but will serve to acquaint the reader with major references on this subject. (See also Porter; Rosenberg and Bergstrom.)

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[1] Models incorporating the reciprocal causation concept are presented in Hubert M. Blalock, Jr., *Theory Construction* (Englewood Cliffs, N.J.: Prentice-Hall, 1969), 115–125; also 95–97.

[2] See, however, the following: David Easton and Jack Dennis, *Children in the Political System* (New York: McGraw-Hill, 1969), 337–343; Harold F. Gosnell, *Democracy: The Threshold of Freedom* (New York: Ronald, 1948), chap. 4; Fred Greenstein, *Children and Politics* (New Haven: Yale, 1965), chap. 6; M. Kent Jennings and Kenneth P. Langton, "Mother Versus Father: The Formation of Political Orientations Among Young Americans," *Journal of Politics*, 31 (May, 1969), 329–358; James March, "Husband-Wife Interaction over Political Issues," *Public Opinion Quarterly*, 17 (Winter, 1953–1954), 461–470; and John Fraser, "Orientations Toward Parents and Political Efficacy," *Western Political Quarterly*, 25 (December, 1972), 643–647.

The Contemporary Situation

In the welter of writing on women in contemporary society, the dimensions that I believe are most relevant to political scientists are the following: (1) gender roles and their perpetuation (2) feminine life-cycle and occupational differentiation (3) gender-biased law, and (4) current trends in voting, campaigning, and office holding.

Gender Identity and Gender Role. Gender is a more precise definitional construct than the more popular concept "sex." Gender consists of two aspects—gender identity and gender role. Identity, Bernard observes (p. 16) in her *Women and the Public Interest*, begins in the hospital delivery room where, on the basis of anatomical cues, the child is classified as male or female. It is this act which primarily seals the child's destiny. Gender may be independent of the genetic sex, external genitalia or internal anatomy of individuals, yet misassigned persons will ordinarily accept the gender identity without question and exhibit the social traits of the gender. Why there are exceptions is unknown, and research findings are equivocal (Holter, 1973, p. 17; Bernard, p. 17; Ramey). While sex is a biological fact, gender is a social-cultural-sociological-psychological fact, Bernard observes. Gender, in my view, is a highly significant "*political fact*," as well, directly related to the political socialization the child will receive, the law which he or she will need to abide by, the differential distribution of social values, and the micro and macro power relations of men and women.

Gender roles, the constellation of traits and behaviors which are "feminine" and "masculine," vary across cultures and times; for example, it is masculine in some societies for men to be highly emotional and sensitive (Bernard, p. 2). They also vary by social economic stratification in the United States and in England. Lower- and working-class male youth are socialized to be politically passive while middle-class youngsters are nurtured to be politically active,[3] whereas the opposite is the case for physical aggression (Strauss; Goode, 1971). There is also the assertion—I believe quite correct—by Holter in her comprehensive *Sex Roles and Social Structure* (pp. 42–52)—that certain traits are functional to those in power (see also Klein, 1972, p. 230). Thus the subordinate group learns to be self-sacrificing and expressive whereas the superordinates are expected to be self-interested and instrumental (also see Bernard, p. 26). The degree to which roles are strictly differentiated appears directly related to the differential rewards in a polity. Hence gender differentiation is not just an intellectual curiosity, but a practice which involves both economic, social and political stakes. Therefore there are incentives to maintain and perpetuate gender patterns (Holter, p. 48).

How Are Gender Roles Maintained? Holter (chap. 1, pp. 16–52; chap. IX) elucidates the following elements: (1) a normative belief and value component which is the basis for moral judgments and for ascribed behavior, traits, work tasks and privileges. (In each culture the role traits are considered inborn, natural, or sacred.) (2) the language (Barron), family, and school (Freeman, 1970; Ehrlich; U'ren), religion, mass media and popular entertainment channel both genders to appropriate life cycles and spheres (Bernard; p. 24; also Ehlmann; Flora; Kohlberg; Komisar; Martin; Trilling). (3) informal positive and negative sanctions, subtle encouragements, and not so subtle discouragements, including ridicule (Holter, p. 202; Palme). (4) occupational gender segregation, stratification and discrimination. To this list, I would add (5) co-optation and tokenism, and (6) coercion through the political process, particularly—in my view—by means of the law (See Holter, p. 21; Mill, pp. 154–180).

While (2), (4) and (6) above have been studied, the remaining elements have yet to be thoroughly investigated. Little is known about gender norms, the degree of passion with which they are held, and the extent to which they underlie social and political behavior. A significant element in (1), it seems to me, is false consciousness,[4] against women's objective self-interest, which includes beliefs in the appropriateness of lower pay, of doing menial work, and of eschewing leadership positions and politics. The lack of substantial numbers of role models in the higher occupational and governmental (Wormser) strata—and the differences in pay and prestige within a given stratum—seem to reinforce belief in a subordinate gender role, whereas co-optation and tokenism appear to function in part to legitimize the claim that the meritorious are rewarded.

Feminine Life Cycle, Occupational Stratification, Political Participation. White-middle-class American and Western European women appear to have similar gender life cycles. They learn supportive and affective roles, internalize politics as a masculine domain, set their aspirations to marriage and motherhood, and are ambivalent (Bardwick and Douvan) about higher education or view it as a means to a feminine job or oc-

[3] Michael Mann, "The Social Cohesion of Liberal Democracy," *American Sociological Review*, 35 (June, 1970), 423–439.

[4] Raymond E. Wolfinger, "Nondecisions and the Study of Local Politics," *APSR*, 66 (December, 1972), 1077.

cupation. Some bright girls seek to fail academically and/or others have psychological conflicts (Chesler) about achievement (Horner)—or—lack of achievement, in a society where it is highly valued as a norm, albeit viewed as a masculine behavior (Horner; Holter, pp. 227–229). Most women, however, marry young and after a brief period in outside employment, they are occupied full-time at home with care of infant children (see Gavron). Isolated, they lack a sense of political self (Lynn and Flora), civic role (Lopata, 1971, p. 47) and political efficacy. Job and career interruptions (Coser and Rokoff) vary with number of children (Bernard, p. 184), and household chores continue when women work for pay (Pyun), occupying from 25 to 40 hours weekly of their time (Myrdal and Klein; also Organization for Economic Development, pp. 185–214; Haavio-Manila; Paloma).

The amount of segregation of women in sex-typed occupations in the United States exceeds that of racial segregation—67% to 47%—women being concentrated in the nonsupervisory (Gross) and non-politically relevant feminine occupations of nursing, elementary school teaching, library and social work, home economics and clerical work (Safilios-Rothschild, p. 100; see also U.S. Department of Labor, *Job Horizons for College Women;* Hapgood; Harbeson). The percentage of women in the remaining professions ranges from less than one per cent of engineers, to 3.5 per cent of lawyers, 7 per cent of doctors, and 28 per cent of biologists (Epstein in Theodore, p. 54).

Within the hierarchy of occupations and in each stratum women rank lowest and are paid less (see Committee on Education and Labor). It is appropriate here to present some statistical documentation: When women are equally qualified and as experienced as male workers there is an average salary discrepancy of 42 per cent with dollar gaps ranging from $3,000 to $6,000 annually. The percentage of women having wage differentials of $3,500 less than their male counterparts is lowest among women operatives and service workers, greater for clerical, and highest among professional workers (Levitin, Quinn and Staines; for Britain see Klein, 1965). See Table 1.

Lest the reader be left with an assumption of a universal pattern, let me sketch some cross-cultural differences. Black women in the United States have long-term commitment to and confidence in their own executive ability, lack strong compulsion to marry and a motherhood mystique, occupy professions in similar proportions to black males (Epstein, 1973; also see Fichter; Bock), and not so incidentally comprise four of the sixteen women presently in Congress (for more on the black woman see Cade; Hare; Ladner; and Lerner, 1972). The pattern of women's options cross-nationally appears to be curvilinear, with the middle range GNP countries per capita offering most opportunity. Thus in Greece, Costa Rica, India, Pakistan, Turkey, Scandinavia and Eastern Europe there are high percentages of women in the "masculine" professions, including architecture, dentistry and medicine (Safilios-Rothschild; see also Epstein, *Women's Place*, pp. 12–13; Lapote; U.S. Department of Labor, *The Fuller Utilization of the Woman Physician;* for women in planning, see Hapgood; for banking, see Alexander; for management, Basil; for academe see American Political Science Association; Austin; Chancellor's Advisory Commission; Graham; Kreps, chap. 4; Oldman; Rossi and Calderwood; Suelzle; see also Szalai).

Why differences? My hypotheses are as follows, holding other variables constant: Rapidly developing economies provide employment opportunities for "mixed game" gender occupational patterns; post-industrial economies with small percentage GDP increases and greater unemployment resemble "zero-sum games," with men's mobility into the professions tending to vary inversely with women's. However, when there is discrimination against a subgroup, as with blacks in the U.S., there is also a "mixed game" with differing values, beliefs, and self-perceptions: women are viewed as occupational cooperators, not as competitors (cf. Epstein, 1973).

The skein of relationships between elements of gender roles and political participation is yet to be discerned. There are mediating factors of religion (Islam, for example, appears to be inversely associated with women's political participation, whereas Catholicism may not be), of family and school socialization prior to job decision making and to political activity. Table 2 suggests some research possibilities. When women have "high" decision-making experiences in the family and school (as operationalized in Almond and Verba's interview schedule)—as in the United States—then "low" decision making on the job appears to be a critical variable related to low political participation. In Mexico, however, both school and job decision making, although of different magnitudes, may be of significance to political participation. (For women in Latin America see Pescatello, Stevens.) Other aspects of political participation are discussed in greater detail below.

Coercion to Gender Role Through Law. Why —asked John Stuart Mill—if women are naturally subordinate, is law needed to keep them so? Perhaps because neither socialization, channeling, subtle social control nor co-optation of a few always works; the law is needed to force the "uppity" into line (see Freeman in *Valparaiso*

Table 1. Discrimination Against the American Working Woman: Percentage of Women in Occupational Categories Having Total Annual Income Discrepancies of −$3,500 or More When Equally Qualified and Experienced[a]

Occupational Category[b]	Percentage	Base *n*	*df*	x^2
Professional, technical and managerial	70.3	74		
Clerical and sales	64.7	139		
Operative and kindred workers	11.9[c]	59		
Service workers, excluding private household workers	30.4	46	3	64.465[d]

[a] Table adapted from Teresa Levitin, Robert P. Quinn, Graham L. Staines, "Sex Discrimination Against the American Working Woman," *American Behavioral Scientist*, 15 (November/December, 1971), Table 2, p. 248. Briefly the method on which this table is based is as follows: After randomly dividing the sample of men and obtaining, by multiple regressions optimal weighting of selected achievement variables, and after expected values were obtained on the second random half sample of men, these latter figures were compared with observed values (pp. 234–5). The mean of the observed-expected discrepancy for women was −$3,458 (S.D. = $2,200); for the second random half sample of men the mean was −$27 (S.D. = $4,523) (p. 245).

[b] Managerial workers ($N = 19$) were combined with professional and technical workers ($n = 55$). Sales workers ($n = 17$) were combined with clerical workers ($n = 122$).

[c] In the original Levitin, Quinn, and Staines article the figure is 15.2%, which is an error, according to Staines (personal communication, July 16, 1974).

[d] p .001.

Law Review). From the little we know, English, American and French law based on the Napoleonic code is equally oppressive to women (see Beard, pp. 88–155; Silver, p. 85). Kanowitz's excellent *Women and the Law: The Unfinished Revolution* documents the massive body of statutory and judicial discriminatory law based on beliefs concerning the masculine and feminine genders which would be overturned were the Equal Rights Amendment adopted (see also *Harvard Civil Rights-Civil Liberties Law Review*, and Committee on the Judiciary).

The discriminatory law in the United States is based on three notions: (1) *Natural male dominance* (Kanowitz, p. 36). Hence in the "doctrine of coverture," the legal unity of husband and wife, the latter's legal identity is eliminated while the husband is given legal charge. In some states the law requires the wife to assume her husband's name, to live where her husband decides; she forfeits rights in some states to contract, to assume liability and to control and manage her own property. The husband is to support the family in marriage and in divorce (Kanowitz, chap. 3). One

Table 2. Percentage of "High" Decision-Making in Family, School, Job and "High" Political Participation: Civic Culture[a] Data, United States and Mexico[b]

	Per cent of *High* Responses			
	United States Sample		Mexican Sample	
Item	Male	Female	Male	Female
Family[c]	27.1	27.8	26	19.3
School[d]	32.8	33.3	21.6	12.1
Job[e]	37.1	10	31.5	5.5
Political Participation[f]	16.7	8	7.9	1.6

[a] Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little Brown, 1963).

[b] I have constructed the above table from data kindly made available to me by Edward Glab, Jr., of the Institute of Latin American Studies, University of Texas, Austin, who utilized Inter-university Consortium for Political Research source decks.

[c] Average responses to variables 8, 9, and 10, influence in family decisions.

[d] Average responses to variables 12, 13, 14, discuss and debate in school, feel free to talk to teacher if treated unfairly.

[e] Average response to variable 15, usually consulted on job decisions affecting respondent, and variable 16, feel free to complain on decisions disagreed with.

[f] Average response to variables 2, 3, 7, 18 influence local decisions and/or Congress, active in political campaign, and member political organization.

study, however, indicates that in most cases, court-ordered child support is not complied with, and in a recent investigation of 12,000 divorces no alimony was requested in 93 per cent (Nagel and Weitzman). (2) *A double standard of sexual behavior.* According to Kanowitz males are allowed great sexual freedom, whereas severe restrictions are imposed on females (p. 23). The law often frees the male customer and punishes the prostitute (Kanowitz: pp. 16–18; see also Petrie; Silver, f.n. 17). It allows the man but not the woman the unwritten law of defense in case of marital infidelity, allows intercourse of underage boys with older women but not vice versa, and does not define sexual assault in marriage as rape (Kanowitz, chap. 2). Until the historic Jane Roe vs. Henry Wade decision of the Supreme Court on January 22, 1973, abortion was illegal in many states and other jurisdictions (Kanowitz, p. 25). (3) *Natural female weakness.* With this notion women were held under interpretations of the fifth and fourteenth amendments to be a separate legal class from men (Kanowitz, pp. 151–154). As recently as October 27, 1971, the New Jersey Supreme Court upheld in State v. Constello, a statute which allows longer sentences for women offenders than men offenders convicted of the same criminal conduct (see Temin, p. 357). The law in some instances has sanctioned denial of jury duty; prohibitions from practicing law or working in bartending, wrestling and mining, and from night and overtime work, and inequality in wages (Kanowitz, chap. 6). One justice remarked that women may well be the "victims of protection" (Kanowitz, p. 5). Nevertheless no state law as of 1970 provides job security for women on maternity leave, and only two provide maternity benefits (Ross; see Sweet, p. 208) whereas such is the case in a number of European common market countries (Organization for Economic Cooperation and Development, pp. 32–33; see Hoskins and Bixby).

While the Equal Pay Act has been with us since 1963 and the Civil Rights Act since 1964, and while these appear to portend change—how pervasive are the outcomes in eliminating discrimination? Certainly the decade ahead provides an outstanding research opportunity for policy analysis of these laws, including affirmative action, in a time of constricting labor markets in many professions (Gold). The politics of the Equal Rights Amendment and the counter movement to the Roe v. Wade abortion decision are also timely areas for inquiry. Such studies would deepen understanding of the political and economic forces seeking containment of women within traditional roles, and the processes involved.[5]

[5] See "Opposition Rises to Amendment on Equal Rights," *New York Times*, January 15, 1973, 1.

Political Perceptions and Political Behavior: Plus Ca Change, . . . ? What are the trends in women's political perceptions, voting, campaigning and office-holding? The contemporary American pattern, it appears to me, is as follows: Tensions brought by the interaction between the traditional gender role and the rhetoric of democracy in the home, school, and cultural milieu have been resolved by (a) increased women's voting and political volunteerism, and (b) political behavior within the confines of the gender role, which in turn results in (c) few inputs of political demands (d) few women elected to political office, and (e) few policy changes which would eliminate legal barriers and facilitate more options, and hence give feedback of a broadened gender role. Thus I suggest (following Gross), that the more things have changed, the more they have remained the same.

Past research shows that on the average, women behaved politically in a way we would expect them to, given the nurturing of passivity, expressivness, supportiveness, apoliticism, child and home orientation, time constraints, lower prestige and authority in the occupational hierarchy, lack of political resources and networks critical to political leadership (Lynn and Flora) and a sense of political efficacy necessary in politics. (See Andreas, chaps. 1–4; Heiskanen; Amundsen, p. 130.) One sees, as one would expect, low voluntary organizational membership, low rates of voting and political activism, few officeholders, and a concern for home-oriented issues. (See Costantini and Craik's succinct summary, p. 218; for an early account of women's voting patterns in Europe see Duverger.)

Within recent years, however, women's voting and volunteer political activity has been increasing and concerns and aspirations have widened. We continue to observe trends noted in *The American Voter:*[6] Southern women, those over 55, and women with eight years or less of elementary school are those whose voting rates continue to lag behind men's (Lansing; cf. Kruschke). However, women's turnout has increased at a rate three times that of men's in the years 1948 to 1968, as shown in Ferris's *Indicators of Trends in the Status of American Women* (p. 181). By 1972 the difference in men's and women's vote had narrowed to two per cent and women comprised 52 per cent of those voting.[7] The number of women who take part in one or more campaign activities is now only about 10 per cent less than men, but young

[6] Angus Campbell, Phillip Converse, Warren Miller and Donald Stokes, *The American Voter* (New York: John Wiley, 1960).

[7] "Voter Participation in November, 1972," *Population Characteristics* (Washington, D.C.: Bureau of the Census, December, 1972), Series P-20, No. 244, 3.

women and the highly educated are more active than their male peers (Lansing).

In a 1971 poll women expressed greater concern than men about ending the Vietnam war, poverty, and drug and gun control (Harris, p. 68 and p. 79). Before the nominations more women than men preferred Senators Muskie, Humphrey, and Kennedy whereas there was greater men's support for President Nixon and Governor Wallace (*Ibid.*, p. 103). Almost half of all women approved efforts to strengthen their status, with 6 out of 10 of the single, college educated and black women holding this view (Harris, p. 2).

As far as I can tell, however, there is no identifiable "women's vote" in the sense of a bloc vote for women's direct self-interest (Chafe, p. 45), be it for the semi-issues of strict enforcement of nondiscrimination at work, easily available abortion and child-care centers, or the nonissues of housewives salaries and pensions, and paid maternity leave guaranteed by law (see Koontz). Contemporary women perhaps represent the ideal type of public regarders and noninstrumentalists, both in voting and in volunteer political work where they usually do the menial tasks. It is noteworthy that even women party leaders do not ordinarily seek the prestige or influence which would advance their own self-interest and/or that of other women (Constantini and Craik, pp. 233–235). In consequence, women's political participation has had scant political impact in policy formulation and outcome.

The lack of a significant proportion of women in state legislatures and in the Congress, is, as suggested earlier, both a consequence and a cause of the continuance of traditional gender roles. For women the recruitment path to elected state and federal office appears to be rockier than for their male peers; they have a narrower network base, fewer political resources, barriers may be thrown up by party leaders and support by men and women harder to obtain. (See Krauss; Lynn and Flora; Bullock III and Heys; Kuriansky and Smith; Jennings and Thomas.) Hence it is not surprising that only about three per cent of the state legislators and the Congress since World War II have been women (Amundsen: p. 78; Werner, 1968; U.S. Department of Labor, 1969, pp. 125–126). Their numbers have been small, and they have lacked legislative leadership positions to make policy changes and to further their legal liberation. Women legislators must ordinarily depend on the altruism of their male colleagues for support. The latter have little to gain politically by offering such support or to lose by withholding it.

To what extent are there harbingers of change? As of 1974, state legislatures remain 94 per cent male, and state senates and the U.S. House of Representative, 96 per cent, although the number of women in state legislatures increased exponentially over 1972 (Lynn; Krauss). While the percentage of women in legislatures shows a fluctuating upward trend in the past ten years, the pattern in Congress appears to be cyclical (see Werner, 1966, Table 12).

The few women who do become office-holders in the United States comprise an interesting leadership group for study. Werner and Bachtold found that while both men and women state legislators have traits closely identified with their gender, e.g., feminine sensitivity, masculine ego strength, psychological trait overlap is considerable—both groups being bold, venturesome, intelligent. Women legislators tend to be more liberal in their political orientation than their male colleagues, according to Werner and Bachtold (fourteen of the sixteen women in Congress are Democrats [see also Means, p. 496]). But, although there are data on the congresswomen's demographic backgrounds, we have little research on their legislative roles (see Foote; also Center for the American Woman and Politics), nor do we have cross-state comparisons of women as state legislators. The question of how women acquire or develop a psychological predisposition to seek political office is also largely unstudied. (See, however, Means; Krauss.)

On the local level in the United States (and in England), the percentage of women in office is higher (see Sullerot, pp. 222–229). This is especially the case with school boards where women may number up to 15 or more per cent (see Gruberg, p. 215). Again, however, little is known about their activities. The rate of participation of women in local and state office in the U.S. seems to vary inversely with the degree of prestige and power of the office, the amount of salary, urbanism, and political bossism, and positively with party competition, number of offices available, proportion of women and men professionals, and the general economic affluence of the particular area (Constantini and Craik; Porter and Matasar; Krauss).

As the reader would suspect, however, the American pattern is not the world pattern. The incomplete and fluctuating U.N. data on women's proportions in parliament and the other available research seem to indicate a general curvilinear pattern corresponding roughly to the options of women in the professions; that is, the middle level GDP (gross domestic product) countries have greater proportions of women in the parliaments than do countries with the highest domestic income and degree of industrialization (Devaud; Fujita; Means; Menon; Norris; Raksasataya; Krauss). There are clearly other factors at work, however. There are no women in par-

liament in the Islamic Middle East, as expected, yet we find also none in Spain; in the Philippines, New Zealand, Israel, and the Netherlands women have comprised up to 7 per cent of the legislative bodies. The highest Western percentages are in Scandinavia—Sweden and Finland having up to 20 per cent women members of parliament. What also appears to be the case is that as a country develops into the postindustrial phase as in Japan, the proportion of women in parliament (Koyama, p. 142; see also Goode, 1963, p. 350) drops to the 2–4 per cent level of the United States (Werner, 1966; Lamson), England, and Canada (Boyd). The Eastern European countries have consistently high percentages (Chylinska), but in those bodies in which legislative policy is formulated[8]—i.e., in the paid Communist Party hierarchy—there are presently few women or none at all (Kurganoff; Horoszowsky, p. 15; Lapidus; cf. Mandel).

In addition to the occupational opportunity hypothesis which I have advanced earlier to help explain differences in women's proportions in parliament, another major explanatory element may well be structural: Electoral systems based on proportional representation have greater proportions of women in office, as in Scandinavia; such is the case even when proportional representation is combined with single member districts, as in Germany, a point noted by Means (p. 499; see also Krauss). This tendency may reflect another one: the belief that women should be on the ticket, whether as token to appeal to women voters, or as parliament members in their own right (Means, p. 517 and p. 496, f.n. 19; see also Bullock III and Heys). It appears, however, that in either case, parliamentary office-holding ordinarily constitutes an addition to the gender role for exceptionally capable women who are able to combine political careers with the childrearing and household duties traditionally ascribed to the feminine gender (see Means, p. 521).

Gender Roles and Women's Political Movements in Historical Perspective

This section is addressed to these questions: (1) What is women's historical role in the polity? (2) What are the circumstances under which the first women's political movement developed in the 19th century? (3) Why did it decline? (4) What accounts for the resurgence of feminism in the 1960s and what are its goals compared to the earlier movement?

Varying Gender Roles. After reading Bullough and Bullough's *The Subordinate Sex: A History of Attitudes Toward Women* one is left with the impression that the gender role has varied little over the ages. While the misogynist ideology, so well documented by the Bulloughs, survives through the centuries, overt manifestations have been quite different within, and across polities and from period to period (Putnam; Beard; Giele in Klein, p. liv).

Putnam's newly republished 1910 work, *The Lady*, presents an account of women of "the favoured social class" in the Greek, Roman, and later European contexts. She depicts the interrelation of customs and laws to women's roles and offers trenchant commentary on the differences between Plato's and Aristotle's views toward women. Her book, therefore, will rankle some who disagree with her interpretation, but perhaps will interest others in re-examining Plato's and Aristotle's political philosophy with regard to the capacity and place of women in the polity.

"One of the temperamental differences between Plato and Aristotle," Putnam wrote, "consists in the greater willingness of Aristotle to acquiesce in existing conditions and to exert his imagination to provide reasons for their permanence" (Putnam, p. 33). There was ample room for comparative study: Spartan women enjoyed social freedom, shared the physical training of males (see also Mill, p. 139), and held property whereas in Athens it was considered natural for women to live indoors and they were the property of their fathers and husbands. (This near chattel status appears to have followed an earlier period when Athenian women were legally politically equal to men and voted in popular assemblies [Marmor].) As Putnam notes, Aristotle classed slaves and women as naturally inferior (see also Mill, p. 137); whereas Plato in *The Republic* envisioned a polity wherein both sexes would have the same opportunities; women or men talented for the higher occupations—medicine, music, guardians—would enter them regardless of who performed a particular reproductive function (Putnam, p. 36; Beard, p. 70).

In Athens, there is some indication that women participated in the intellectual ferment of the Golden Age, studied with Epicurus, Plato, Socrates and especially Pythagoras, and that several were philosophers (see Davis, p. 192 but cf. Hackert and Pomeroy; see also Beard, pp. 320–325); nevertheless, most of them were restricted to producing legitimate heirs, directing and managing the husband's house and engaging in religious activities (Putnam, p. 1). The Roman lady was quite different, for she had legal control over property. She also was unencumbered by household or childcare tasks since this work was done by Greek slaves. The Roman lady was also educated in the Greek classics, read and wrote Latin,

[8] D. Richard Little, "Legislative Authority in the Soviet Political System," *Slavic Review*, 30 (March, 1971), 57–73.

and had a more public and diversified role (see Beard, pp. 298–309).

While the independence of women was the subject of some uneasiness in Rome (Putnam, pp. 46–47; Bullough and Bullough, chap. 4), in early Christendom the abbess was accepted by monarch and popes as baron, administrator, scholar, artist and minister to the poor and sick (many women were canonized as saints [McGinley]), and this option afforded women freedom from the Hobson's choice of home or outlawry. Women played a prominent part (Beard, pp. 215–278) in the development of the medieval guilds, without experiencing occupational barriers (see Klein, 1972, p. 9). Differences were customarily based on class, rather than gender lines (Beard, p. 254). During the Renaissance women participated in the enlightenment, attended universities, were afforded a classic tutoring, and several were rulers. In addition, the salons of Paris conducted by women were centers of learning, "politesse" and power; women spoke out against the androcentrism of Rousseau, and their aspirations for equality were supported by Diderot and Voltaire (see Bullough. chap. 11; see also Rowbotham, pp. 38–40).

The industrial revolution was to have a profound effect on the future of women, for with it work and home were separated, the extended family gradually declined (Goode, 1963, pp. 11–26), urbanism increased, women became isolated and were either dependent upon their husbands or alternatively they became the poorest paid factory proletariat (Klein, 1972, pp. 13–15; see also Bebel). It was in this context of industrialization that Blackstone developed the doctrine of coverture by which married women in England and then America suffered "civil death," having no legal existence apart from their husbands (Flexner, pp. 7–8). Women of the middle classes, their position considerably lessened, sought education and access to the social and political system as equals while the early factory women wanted protection from sweatshop conditions (see Klein, 1972; p. 15). It was this milieu of industrialization, urbanization and declining status for women that set the stage for the beginnings of protest in the United States and Europe. And it was this class difference in type of repression of women in the lower and middle classes—in my view—which could be exploited to divide and divert them from mutually beneficial social and political goals.

Feminist Political Movements

How did they begin? Women's movements throughout European history—and, it appears, in China, India, and Pakistan (see Rowbotham; Goode, 1963, p. 303, also Ward, p. 66) as well—arise phoenix-like within social revolutions and movements for independence. Women's movements seem to have five analytic components: (a) women, at first other-directed, begin to extend the ideology of equality and social justice to their gender; (b) a new and true consciousness of the role develops out of the general questioning of institutions and norms; (c) the old movement does not adequately meet the newly articulated concerns of women; and (d) the communication network of the old movement serves as the nexus of a women's political organization, and (e) political reforms may be attained which may be more or less real or symbolic in their application, which may lead eventually to the revival of a feminist movement (cf. Freeman, 1973).

In the subsections which follow I shall concentrate primarily on the developments in the United States, covering (a) beginnings of protest; (b) the early feminism; (c) the question of failure of the women's rights movement; (d) the new feminism, and (e) themes of the contemporary movement.

Beginnings of Protest. In 1777 Abigail Adams wrote to her husband, John:

> . . . [In] the new code of laws which I suppose it will be necessary for you to make, I desire you would remember the ladies and be more generous and favorable to them than your ancestors. Do not put such unlimited power into the hands of the husbands. Remember, all men would be tyrants if they could. If particular care and attention is not paid to the ladies, we are determined to foment a rebellion, and will not hold ourselves bound by any laws in which we have no voice or representation. (Schneir, p. 3)

The above is quoted in Miriam Schneir's outstanding compendium, *Feminism: The Essential Historical Writings* where we learn that John[9] replied, "As to your extraordinary code of laws, I cannot but laugh" (Schneir, p. 4). Yet Abigail Adams, for ten years separated from her statesman husband, managed the farm, acquired property for the estate, directed construction of buildings, and superintended dairying operations, according to Buyer's *Notable American Women, 1607–1950*, a most comprehensive three-volume biographical work. Charles Francis Adams who collected the letters of his famous grandparents wrote: "The heroism of the females of the Revolution has gone from memory with the generation that witnessed it, and nothing, absolutely nothing remains upon the ear of the young of the present day but the faint echo of an expiring general tradition" (Schneir, p. xi). (For egalitarian effects of the frontier in eighteenth-century America and decline of women's status in the nineteenth, see Demos.)

[9] Cf. Jefferson, who wrote, "It is civilization alone which replaces women in their enjoyment of their natural equality," from *Notes on the State of Virginia, 1781–1795*. quoted in Montagu, 36.

In 1792 in England Mary Wollstonecraft published *A Vindication of the Rights of Women*, which is considered the founding work of the modern women's rights movement (Flexner, p. 15). In 1798 after the French National Assembly adopted the Rights of Man, a number of women petitioned in vain to have a Declaration des Droits des Femmes accepted (Klein 1965, p. 19). As some American women taxpayers in New Jersey in 1776 had their suffrage rescinded later in 1807, (U.N. Commission on the Status of Women, p. 134) so with the promulgation of the Napoleonic code (de Beauvoir, pp. 101–102), French women were stripped of divorce and other rights gained in the French revolution. Yet the writing and the speaking for *égalité* continued abroad (de Beauvoir, pp. 116–117) and in the United States. Two southerners who freed their slaves and went North and there agitated against human bondage were Sarah Moore Grimké and sister Angelina; they spoke before large audiences not only in the abolitionist interest but for women. Wisconsin historian William L. O'Neill (1969) credits Sarah Grimké's *Letters on the Equality of the Sexes and the Condition of Women* (1838) with engendering the sympathy of Protestant clergy to the woman's rights movement (O'Neill, 1969, p. 12; see also Lerner, 1967. For women of the South see Scott, 1970; Lerner, 1972).

The Early Feminism: For Equal Rights. After many women had braved hostile mobs at meetings on behalf of the abolition of slavery, had served as "agents," i.e. official speakers, in that cause, and had circulated anti-slavery petitions which were presented to Congress, Lucretia Mott and Elizabeth Cady Stanton, along with others of their sex, were excluded from being official delegates to the World Anti-Slavery convention in London in 1840. This "moment of truth" stung many women into new thought and action, according to Stanton, and eight years later, while they both continued as abolitionists, they with three friends called the first political convocation of women. Eleanor Flexner's *Century of Struggle: The Woman's Rights Movement in the United States* discusses the first meeting at Seneca Falls in 1848 at which time a paraphrase of the Declaration of Independence—"All men *and women* are created equal; that they are endowed by their Creator with certain inalienable rights . . ."—was adopted and several resolutions passed. Their Declaration covered the scope of grievances which they vowed to undo—the taxation without representation, the inequitable property and divorce laws, the monopoly of the professional occupations, the denial of higher education. (The text may be found in Schneir, pp. 77–82.) A narrowly passed resolution, considered by many to be too radical for the time (1848), called for suffrage and was adopted after being defended on the floor by Frederick Douglass, the black abolitionist leader, among others.

There followed seventy-two years of agitation in legislatures and in Congress with advances and failures; i.e., male blacks were given equal rights and the vote by 1870, while both were denied to women. Among suffrage opponents were the brewery, oil and other business interests, according to Flexner, who feared not only female suffrage but also reform laws which might follow for the now large number of women who toiled in the garment factories (see Scott, 1971, chap. 2); other "antis" were southern politicians who wished to deny the black woman's vote, and northern political bosses wary of a "clean up" in politics (Flexner, pp. 294–305; also David Morgan, chap. 11). Each gain was fought for arduously. There were 277 campaigns for inclusion of suffrage planks in state platforms; 480 state legislative attempts for suffrage referenda in the states and 56 referenda campaigns; on the national level there were 30 to obtain woman's suffrage planks in party platforms (Flexner, p. 173). The suffragists also worked with nineteen separate Congresses until, in 1920, the 19th amendment to the Constitution was adopted and ratified by the states (Flexner, p. 173). More than a million women participated in the final phases (O'Neill, 1969, p. VII; David Morgan, p. 99; see also *History of Woman Suffrage;* Catt and Schuler; Kraditor 1970 and 1971; Parker).

Was the Women's Rights Movement a Failure? The answer to this question depends upon the criteria employed in evaluation. No, if one points to the winning of the suffrage. But in the long run —at least until the 1960s—did this not prove to be a Pyrrhic victory socially and politically? For, as nearly all writers point out, with the gradual disappearance of the movement (Riegel, chap. 11), there was a slow decline in women's status for the next thirty years, measured by fewer women proportionately in the colleges (O'Neill, 1969, p. 93; Newcomer; Epstein, in *Woman's Place*, 1971, pp. 56–60) and in the professions (Knudsen), and an increase in the inequality in wages (U.S. Department of Labor, *Fact Sheet on the Earnings Gap*. p. 1; Knudsen; Suelzle, 1970). Also there were no meaningful increases of women in positions of social and political authority nor of social legislation directly beneficial to the gender (David Morgan, p. 195; O'Neill, 1969, p. 267; Chafe, pp. 37–47).

Why? First, there were divisions within the ranks of the feminist organizations in the 1920s, abetted by the industrial and business interests which feared both the other-oriented social pro-

gressivism of one segment and the self-interested egalitarian thrust of the other (see Lemons, chaps. 6–8). Feminist leaders neither mobilized women's bloc voting nor made unified demands for political representation in the parties. During the depression of the 1930s, married women, without any legal protection, were fired from their jobs as a matter of governmental and business policy (Lemons, pp. 230–231). After World War II, many women who had entered the work force were forced out by the closing of government-sponsored child-care centers (Chafe, pp. 186–187), and, later, I believe, by the post-Korean War recession. Many left voluntarily in the ideological milieu of a new familism buttressed by a popularized Freudianism which portrayed the nonhome-bound woman as psychologically afflicted (see Bird, p. 31; Firestone, pp. 25–30; O'Neill, 1969, chaps. 9 and 10).

The decline in women's status from the 1930s to the 1960s occurred in Europe, as well as in the United States. (For England see O'Neill, 1971; for France's different pattern see Silver.) The most retrogressive cases were in Germany and the Soviet Union. In Germany where women obtained the suffrage in 1919, Hitler imposed the Napoleonic ideal of "kuche, kirche, kinder" and a women's quota of 10 per cent was set for university entrance; women were removed from judgeships and from the Reichstag, forced into menial and hard physical labor, and prohibited from utilizing birth control devices (Millett, pp. 159–168). In the Soviet Union revolutionary decrees in 1917 and 1918 gave full and equal political and legal rights to women, but the lack of a sustained women's political movement led to the eventual loss of gains for the masses of Soviet women (Salaff and Merkle). In the drive for industrialization in the 1930s, the exigencies of World War II and reconstruction, the official egalitarian ideology was altered, as was the law. Abortions were restricted, then prohibited in 1944, divorces were discouraged by large fines, and the patriarchical family structure was restored (Millett, pp. 168–176). In the mines and heavy industry, a majority of the workforce was female (Schuster), and although there were some advances in education and the professions, the high prestige positions within them were occupied by men (Bernard, p. 137; Salaff and Merkle, p. 177; Horoszowsky, p. 15; Rowbotham, pp. 164–169).

While many European countries granted full political rights to women by the 1920s (a notable exception being France), women's political emancipation in most African and Asian countries occurred after World War II in association with independence and socialist movements (see U.N. Commission on the Status of Women; Ungor; Wolf cf. Snow; see also Rowbotham, chaps. 7 and 8; Ward, pp. 60–76). In Japan, however, political equality was written into the 1946 constitution (Koyama, p. 18), and in Southeast Asia rights were won rapidly and easily (Ward, p. 72).

The Rebirth of Feminism in the United States. Although Simone de Beauvoir's *The Second Sex* was first published in the United States in 1953, it was not until the publication of the best seller, *The Feminine Mystique* ten years later that the U.S. movement got underway with Friedan's critique of the neo-familism of the '50s, her designation of the suburban woman's malaise as "forfeited self" and her analysis of the commercial interests served by encouraging women's role as super-consumer (see also Israel and Eliason).

Meanwhile, women's participation in the traditional feminine occupations was rapidly increasing, and many were underutilized in terms of their educational qualifications (U.S. Department of Labor, *Underutilization of Women Workers*, p. 17; Oppenheimer, p. 187). Also coincident with Friedan's book was the issuance of the report of the presidential commission on the status of women, which John F. Kennedy approved earlier on the urging of its first chairman, Eleanor Roosevelt (Bird, pp. 36–37). The *Report of the President's Commission on the Status of Women* led to the establishment of 50 state commissions (see, for example, California State Commission), several city and territorial commissions, and later a number of federal task forces (see Citizens Advisory Council on the Status of Women reports) which documented inequities, the need for child-care centers for the large proportion of mothers in the workforce, and discriminatory laws. (See also President's Task Force on Women's Rights and Responsibilities, and Citizens Advisory Council on the Status of Women, January, 1972.)

Expectations raised, and spurred on by the Civil Rights Act of 1964 (in which a prohibition of sex discrimination had been added as a tactic for defeating its passage [see Kanowitz, pp. 104–5]), Friedan and other older women formed the National Organization for Women in 1966 to pressure federal agencies to enforce the law, and to work for equality in all spheres (Freeman, 1973). Meanwhile, young radical women who had participated in the civil rights and youth movements of the 1960s—like the feminists of a hundred years earlier—were "stung" into separatism (*Ibid.*). Assigned to menial tasks and told to stay in their place (Bird, p. 211; Chafe, p. 233), they split off from the youth movement and organized consciousness-raising groups (Tanner, pp. 231–253; Freeman in Sochen, 1971, pp. 152–153) to analyze where they were and how they got there (Jones and Brown; see also Bird, pp. 211–255). It

is estimated that by 1970 as many as 100,000 women in 400 cities were engaged in some aspect of the new United States women's movement (Bernard, p. 217; for impact at Yale, see Lever and Schwartz).

Holt and Levine's *Rebirth of Feminism* (see also Carden; Mitchell, part 1; Firestone, pp. 30–40; Freeman, 1973, 1974; Romer and Secor) includes chapters on the movement's origins, major ideas and critiques, forms of resistance, and the areas of action, including the church and the media (chap. 6). While Holt and Levine employ the methodology of social history, their book offers some touchstones for empirically-oriented political scientists who may be interested in exploring the relationship of the effect of "underground" and "overground" media on public opinion and authoritative decision making.

Themes of the Contemporary Movement: Toward Androgynous Roles. The themes of the present feminist movement are both reformist and revolutionary. Future studies of political thought may find contemporary feminism related not only to the general historical movement for equality but also to the humanistic (Boals) and anti-bureaucratic ideology of the counterculture[10] and to the radical political philosophy of Marcuse.[11]

The reformist view incorporates ideas of early feminism—for equality, life, liberty and happiness, in a sense Seneca Falls all over again. It also calls —most significantly I believe—for sharing of gender roles rather than narrowly differentiated ones, for a marriage partnership with mutual child-rearing and financing of the family, with a career for the wife or husband who wants it (see NOW program in Epstein and Goode, pp. 193–198; Robin Morgan, pp. 512–513; Hobbs; also Rossi in Lifton. For a recent comprehensive study of couples who share roles see Fogarty, Rapoport and Rapoport; also see Holmstrom).

On the basis of Jan Dizard's study of 400 families, Bernard holds that role sharing and non-sex-typing of work would be beneficial for the public interest (best person for the job) and for the happiness of most persons, including children (see Table 3). In Sweden the elimination of gender roles in all aspects of life including the school curriculum, higher education and the law, is government policy (Swedish Government; Palme; Leijon; Frederickson). A government objective is part-time and other work arrangements which will allow both genders to be equal parents. One concern is that children be given more attention by fathers, and another is that women be freed to participate in politics (Dahlstrom). The goal is emancipation of both men and women (see Palme). In my view, a longitudinal study of the Swedish efforts now under way, including their impact on political life, would be a fruitful research undertaking with significant policy implications.

The revolutionary feminists envisage a variety of life styles for women (see, e.g., Bart; Balogun; Densmore; Dixon; Greer), men and children, as well as new forms of the family (e.g., Babox and Belkin; Cudlipp; Densmore; Mandle; Robin Morgan; Sochen, 1971; Stambler; Tanner; Ware; for a 19th-century revolutionary see Gilman; also see Giele, 1972; Sochen, 1972). Perhaps the most radical and future-oriented contemporary work is Shulamith Firestone's *The Dialectic of Sex: The Case for Feminist Revolution (1970)* which calls for a "cybernetic socialism" in a century or so. In her utopia, which is based in part on Aries's work[12] and bears some resemblance to Huxley's *Brave New World*,[13] technology eliminates labor and makes possible creative activity for all—men, women and children, as well as the artificial creation of human beings. Youngsters would be reared in communal households (not families) and everyone, including children, would receive the same monetary allowance and would be fully integrated into the larger society in which there would also be a pluralism of sexuality. Like Skinner's *Walden II*[14] and to a degree Toffler's *Future Shock*,[15] Firestone's vision fails to elucidate the political structures and political rules which will assure popular control of technology for freedom, egalitarianism and self-actualization.

Origins of Gender Relationships

How did it all begin? When and how did some women first become subordinated? What accounts for cross-cultural differences between the genders? What is the relationship of gender roles to development of government? And what is the political significance of biological differences?

While these questions are only partially answered by the literature, what we have suggests the following: (1) the ecology of a region determines the means of subsistence and thus affects the division of labor between men and women; (2) the control of the means of subsistence is related to superordination-subordination or egalitarian family patterns; (3) the creation of surplus value leads to the rise of the state and to consoli-

[10] Theodore Roszak, *The Making of the Counter Culture* (Garden City, N.Y.: Anchor, 1969).

[11] See, for example, Herbert Marcuse, *An Essay on Liberation* (Boston: Beacon, 1969).

[12] Philippe Aries, *Centuries of Childhood: A Social History of Family Life* (New York: Knopf, 1962).

[13] Aldous Huxley, *Brave New World* (New York: Harper, 1950).

[14] B. F. Skinner, *Walden II* (New York: Macmillan, 1948).

[15] Alvin Toffler, *Future Shock* (New York: Random, 1970).

Table 3. Differntiated vs. Shared Roles and Pursuit of Happiness
(Based on Dizard's Study of 400 Families)[a]

Degree of Sexual Specialization of Roles	Pursuit of Happiness[b]						
	Type of Woman			Extent of Male Achievement and/or Motivation			Children
	Career	Mixed	Housewife	High	Average	Low	
One Role (maximum role differentiation)	−	−	−	−	−	−	−
Two Roles (wife shares provider role)	+	+	−	+	+	+	+
Shared roles (men and women share many roles)	+	+	+	−	+	+	+
No sex-typing of work	+	+	+	+	+	−	+

[a] Chart adapted from Jessie Bernard, *Women and the Public Interest* (Chicago: Aldine-Atherton, 1971) p. 276 on permission of the publisher. The chart is based on research reported in Chapter 5, "Work, Marriage, and the Pursuit of Happiness."

[b] + means pursuit of happiness is enhanced; − means pursuit of happiness is retarded.

dation of gender power relationships, and (4) selected biological attributes are emphasized or minimized in the supporting ideology,[16] which serves to explain the extant political arrangements in the particular polity.

Regional Ecology, Family Structure, and Power Relationships. In a study of 748 nonindustrialized societies compiled from the *Ethnographic Atlas*, Sanday found that women contribute the most to subsistence where there is shifting agriculture and horticultural cultivation. Conversely, intensively-cultivated, plough agriculture, fishing and big game hunting (cf. Gough) involve greater proportions of men in subsistence providing. Women's contribution was greatest in Africa and the insular Pacific (also see Mead, 1949, 1963) and smallest in the area around the Mediterranean, followed by North and South America, and East Eurasia. Sanday's analysis appears related to Boserup's findings in *Women's Role in Economic Development*. She observes (p. 60) that in sub-Sahara Africa women were the major agricultural cultivators and landholders prior to European colonization. Women also comprise the vast majority of traders and appear to have played a prominent role in pre-colonial African politics (Lebeauf; Leavitt).

What is the linkage between means of subsistence and family structures? Horticultural economies tend to matriliny (Schlegel, p. 17; see Goode, 1963, pp. 194–195), while plough agriculture is associated with patriliny, and the wet rice cultivation in Southeast Asia (Green, p. 60) is related to a colineal or radial structure. Thus Burma, Cambodia, Java, Laos, Malaya, Philippines (see Green) and Thailand have had a longstanding egalitarian tradition, including, in most areas property and inheritance rights for women (see Ward, pp. 81–87).

While modern researchers discount Morgan and Engels's theory, in *Origin of the Family, Private Property and the State*, of the communal family predating other forms (Gough, pp. 766–767), Engels (p. 43) was certainly right when he called the plough the means of creating surplus value. Before the first milennium, B.C., according to Engels, private property originated in the Mediterranean area and the family structure was subsequently changed from matriarchy to patriarchy. Women were believed to be the first slaves, acquired as sexual property and then utilized as menial servants (Collins, pp. 8–9; Mill, p. 130). They were first legally subjugated under the early autocratic state (Jesser, p. 248). Conversely, matriliny (see Schlegel's study of over 60 matrilineal societies) tends to be associated with minimal states (Schlegel, p. 17).

While it is unclear whether matriarchy, rule by women over men, has actually existed (see Schlegel, p. 139; cf. Davis), patriarchy appears to be a real as well as a nominal classification. Both matriliny and patriliny do not necessarily lead to one gender's superordination, for in the former there may be deference to brother or husbands in property and other matters (Schlegel, p. 64), and in the latter, as Jesser reports, real power may lie in women's *use* of property even while there is a mythology of male superiority (Jesser, pp. 249–

[16] This term is used synonymously in this essay with social theory. See Robert D. Putnam, "Studying Elite Political Culture," *APSR*, 65 (September, 1971), Table 1, p. 655. For discussion of the evaluative aspects of ideology, herein referred to as norms, see Willard A. Mullins, "On the Concept of Ideology in Political Science," *APSR*, 66 (June, 1972), 508–509.

250). Ward, in *Women in the New Asia* (pp. 77–78) also points out that although the ideal family unit was patriarchical and extended in China and India, there was considerable freedom and status among women of the lower classes since they were co-contributors to subsistence and their family structure was actually simple, not compound. The well-to-do patriarchial family, often polygynous, denied women the right to divorce and the guardianship of children, the major objective being the perpetuation of the male line (Ward, pp. 84–85).

These fine points are insufficiently analyzed in Millett's *Sexual Politics*, but I believe she is correct in noting that analysis based on economic development is by itself inadequate to explain the power relationships between the genders. It seems that when roles are polarized into expressive and instrumental ideal types, as was the case early in the West, and are reinforced by androcentric beliefs, men's instrumental activities lead to pyramiding of resources, the growth of hierarchical arrangements, the monopolization of force in the state, and power relationships over other men and women (Holter, p. 47; also Gough, p. 768). Millett (chap. 2) points to the matrix of contemporary men's power, in the patriarchal family, in commerce, industry, education, the judiciary, government, police and military forces. She holds that until patriarchy is eradicated, women's inequality will prevail, as it does in the Soviet Union despite socialization of property (Millett, p. 157 and pp. 168–176). Talcott Parsons (1967) expresses a similar view.

Social Construction of Biological Differences. The term "sexism" in the women's literature is utilized analogously to racism to apply to the system of political institutions and processes which maintains the gender order, as well as the ideology which is used as justification for social norms (see e.g., Gornick and Moran, pp. xii–xiii). The ideology of sexism is based on the thesis that women are biologically inferior to men, or that they have particular differences or deficiencies which naturally limit them to a subordinate role (Montagu, p. 205 and *passim;* see Bullough and Bullough for historical account). The fact that women bear children and breast feed them is a biological fact probably related in part to why, in prehistoric times, they most likely did not participate in big-game hunting or other activities far from the hearth (Jesser, p. 248; Gough, pp. 767–768), but were horticulturists, gatherers, or cocultivators of particular crops. Such constant female characteristics, when divorced from the social context, cannot account for variations or changes in gender roles; the explanations must lie rather in various ecological and political developments. Thus, for example, industrialization and urbanization in sub-Saharan African polities are effecting a change from matriliny to patriliny (see Goode, 1963, pp. 194–195).

It seems likely that ideological beliefs concerning the natures of men and women are socially constructed[17] (see Freeman, 1970) as labor and political patterns become institutionalized. The same biological attributes are variously interpreted in differing polities and times as capacity or disadvantage[18] or emphasized or minimized (Montagu, p. 19). The constellation of traits and behaviors incorporated in a particular gender role are then linked to the particular conceptualization of the biological natures of men and women (see Collins, pp. 12–16), and then tend to be self-fulfilling prophesies. A set of explanations, however, may also be altered or extended to account for changes in gender behaviors. Thus, also to use a contemporary example, in the Soviet Union it is now believed that women's nature especially befits them for being medical doctors (Safilios-Rothschild, p. 104).

The particular existential, economic, social and political considerations of a particular culture thus account for the meaning and emphasis given to physical differences (cf. de Beauvoir, p. 31). In those where a male ideology of physical superiority prevails—such as one where muscular strength is particularly valued—the fact that male mortality rates are greater at every chronological age is generally ignored. Montagu in *The Natural Superiority of Women* notes that it has also been overlooked that males are more subject to disorders and disease than women, e.g., the chromosomal constitution of women allows suppression of the hemophilia gene, color blindness and some sixty other disorders peculiar to males, double the number occurring among females (Montagu, pp. 76–78).

In recent times Freudian psychology, ethology and hormonal research have been utilized to substantiate the belief that women and men's natures account for their behavior in modern society and politics. Space limits an extensive discussion of the logic and scientific findings which refute or question this view. Psychologist Karen Horney (pp. 24–70) notes the male orientation of Freudian psychology as a point of departure and comparison for understanding the psychology and role of

[17] See Peter L. Berger and Thomas Luckmann, *The Social Construction of Reality* (Garden City, New York: Anchor, 1966).

[18] Galtung notes that the axioms of a theory shall be noncontradictory, i.e., "If it is possible to derive both H [a set of hypotheses] and $\bar{H}$ [an alternate set] from *A* [an axiom set], then *A* is said to be *contradictory*," Johan Galtung, *Theory and Methods of Social Research* (New York: Columbia University Press, 1969), 463.

women (also de Beauvoir, pp. 33–47; Millett, pp. 177–210; see also Sherfey), and Weisstein has questioned the reliability and validity of the Freudian methodology. Ethological writing, in turn, has focussed on selected cases, ignoring or minimizing the extensive variations in family units and behavior related to particular ecologies of the non-human primate universe (Weisstein; Gough; cf. Tiger; also see Elaine Morgan, chap. 10). In contrast to the nonhuman primates', the human cerebral cortex confers the almost infinite variable characteristics of individual human responses and individual adaptability to environment, according to biologist Ramey. She also holds that there is psychologic sexual neutrality in humans at birth (Ramey, pp. 238–239). Moreover, the social situation affects hormonal levels, as is the case with levels of other secretions, such as adrenaline, rather than vice versa, and there are variations with age and individuals (pp. 243). In consequence Ramey (p. 244; cf. Bardwick) concludes that no psychic response is unique to women or men, despite attempts to construct a case to the contrary (p. 237).

A critical question for political scientists it seems to me is not whether male or female biologies are superior or inferior in certain respects, nor how endocrines secrete, but rather: What variances, if any, in the political behavior of men and women may be attributed to biological factors? These linkages are to our knowledge presently unknown. A second area of relevance of the social theories and research presented in this section and throughout this essay perhaps lies in the processes by which they may modify or replace the old gender ideology and norms—and the political paradigms based on them—and in the concomitant impact on policy which would emancipate both men and women and facilitate wider participation in the polity. Third, one hopes that such explanations and research may serve to stimulate others to seek out empirical data which will further the description, understanding and explanation of the many facets of women's and men's political history, thought, and behavior which concern the discipline.

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BOOK REVIEWS

Spinoza's Philosophy of Law. By Gail Belaief. (New York: Humanities Press, Inc., 1971. Pp. 151. $7.50.)

Benedict (Baruch) Spinoza devoted his brief lifetime (1632–1677) to a major reconstruction of philosophy in metaphysics, ethics, and politics. His importance was acknowledged during the seventeenth century, indeed while he was still alive, and subsequent philosophers have explored his work extensively. Relatively little attention, however, has been given to the passages scattered throughout his writings that pertain to the philosophy of law, even though within Spinoza's philosophy, law fulfills an important role in the development of human perfection understood, ultimately, as the intellectual love of God. Gail Belaief's monograph is therefore a welcome effort to expose Spinoza's philosophy of law in a systematic and critical manner and to demonstrate its implications in the solution of some of the theoretical problems with which current philosophers of law are concerned.

In the exposition of Spinoza's position, Professor Belaief has adopted a method of internal and logical analysis. The monograph is totally devoid of any indication of the historical background, circumstances, or influence of Spinoza's philosophy. Rather, given the author's intention, one is introduced to a logically formulated interpretation of the form and implications of Spinoza's doctrine itself as a viable alternative in current discussions of the understanding of law.

The monograph opens by distinguishing two senses of law in Spinoza's writings. In its wider sense, law means regularity of action; in its narrower and stricter sense, law means a plan of life established by man for himself or others with a certain end. With that distinction as a background, Belaief's primary focus in the bulk of the monograph is on the question of the relation between civil law and higher law, including in the latter category both natural law and moral law.

The first three chapters present exceedingly compact expositions of Spinoza's concepts of civil law, natural law, and what Belaief terms "moral law" to designate the principles in Spinoza's ethical theory relevant to the issue of civil law. Belaief attempts to hold the discussion of each of these concepts within the compass of legal theory, narrowly construed. Consequently, other dimensions of Spinoza's metaphysical, ethical, and political philosophy are incorporated only insofar as they appertain to the basic question of the monograph. Thus, for instance, Spinoza's doctrine of man as governed largely by passions but destined for rationality and his justification of the principle of freedom of thought and speech are properly stressed in varying contexts. But the intricacy of his understanding of the relation between God and nature, the implications of his geometrical method, and his rendering of the character and perversions of various forms of government (which might be understood as a matter of fundamental law) are not given their full due. The result of the strictures within which the discussion is held is a highly disciplined, but also highly formalized and at times barren analysis.

In the fourth chapter, Belaief presents a critical correction of Spinoza's argument that there is an intrinsic connection between civil law and morality and that subjects have a moral obligation to obey civil law. Belaief contends that on the basis of Spinoza's own principles, a legal system may at the same time be valid but violate the moral law and that therefore legal obligation and moral obligation are logically distinct. The contention is attractive and plausible, even if not fully convincing.

The final chapter is perhaps the most creative. In schematic fashion, it outlines the location of Spinoza's legal thought on a set of six perennial problems in the philosophy of law, such problems as the grounds for the evaluation of civil law, the source of law, principles of judicial interpretation, limits to the sphere of legal jurisdiction, the purposes of law, and legal obligation.

The monograph concludes with two appendices. The first, on "The Classical Doctrine of Natural Law," is not terribly useful, although it is part of Belaief's often reiterated point that Spinoza's notion of natural law radically diverges from the classical and medieval traditions. The second appendix contains the Latin texts of some of the more important passages from Spinoza translated in the body of the monograph itself.

Belaief's effort to demonstrate the continued significance of Spinoza's concept of law is admirable and worthy of attention even if it is too limited to do full justice to Spinoza's thought in the area of law and morality and far too expensive for its length.

DOUGLAS STURM

Bucknell University

Toward a Critical Sociology. By Norman Birnbaum. (New York: Oxford University Press, 1971. Pp. 450. $3.50, paper.)

Norman Birnbaum is a publicist, combining academic and journalistic roles, who addresses himself to basic political and social issues and to the politics of his academic peers. In the nineteen-

fifties and early 'sixties, when he lived in Europe, he was among the few Americans up-to-date on the course of Critical Theory in Germany and Marxist trends in England and France. The seventeen essays, articles and reports reprinted here are meant to stand "as records of the intellectual situation" between 1954 and 1970 and also as evidence for the author's deserved claim to be recognized as one of the earliest critics of Parsonian functionalism and as one of the oldest men of the New Left. Birnbaum concedes the unevenness of the selections. Some are unduly dated because they are in the nature of a review of then current events, which might and do look different today. Younger readers, to whom the author wants particularly to appeal, could have profited from a re-evaluation explaining the significance of certain events and developments in the light of his own hindsight.

The chapters deal with (1) the state of the nation: England from the late 'forties to the early 'sixties; the counterculture, knowledge elite, and "post-industrialism" of the U.S.; (2) the state of the literature: the treatment of ideology from 1940 to 1960; (3) the state of the profession: the resistance to sociology in the old English centers of learning before the 'sixties; the meetings of the International Sociological Association in 1956 and 1970; (4) the state of mind of David Riesman —liberal pluralism—and of Robert Nisbet— conservative sociology; (5) the state of Marxism in East and West. For reasons of space and mental economy I shall select three dimensions of the collection: the relationship between religion and capitalism, the crisis of Marxism, and the author's notion of critical sociology.

In view of the continued topical relevancy of "religion and capitalism," the volume's only piece of historical research, "The Zwinglian Reformation in Zurich" (1959), warrants special attention. As the author sees it in retrospect, the essay "argues for the capitalist origins of early Protestantism" (p. vii), but a close reading reveals that he promises more than he is ready to deliver. Birnbaum is too prudent a scholar to advance a primarily economic interpretation and in fact does not disagree with Max Weber's multicausal view. He tries to identify the economic and political interests which came to be allied to Zwingli's religious cause, and concludes that the Reformation in Zurich "entailed an alliance of a new mercantile and productive elite with a large group of lesser artisans, against the patricians (mercenaries and rentiers) and certain artisans, very possibly concentrated in the more traditional sectors of the economy" (p. 153). This sounds plausible, but the evidence adduced, especially lists of a few dozen prominent supporters and opponents of Zwinglianism, is less "reasonably clear" to me than to Birnbaum, who himself tones down his conclusions with qualifying phrases. The social composition and occupational makeup of the guilds is not clearly presented, and hence the kind of capitalist characteristics they may have had is not sufficiently elucidated. The relationship between technology, capitalist organization and relative freedom from guild traditionalism remains moot. If some guilds and trades were indeed less tradition-bound because of their fairly recent establishment or reorganization, does support for Zwingli's reformation on the part of some of their leaders and members constitute prima facie evidence for an attitudinal predisposition or elective affinity? As in so many revolutionary situations, it seems to have been only a minority, in this case of guild masters and council members, who were fully engaged by the religious issue: "The others are sick of the matter," as one council member put it (p. 153). The Zurich Reformation, then, may be said to have resulted from an amalgam of religious virtuosity, status preoccupations, economic interests and *raison d'état*. The very republicanism of a city run by its factious upper strata made it likely that the Reformation, short of a successful social revolution, would be channeled into the establishment of a state church controlled by the city council, and neither this development nor the theology of Zwingli, who was primarily a religious virtuoso, can be reduced to "capitalist origins."

In fact, the author himself perceives, and by his reflections contributes to, "the crisis in Marxist sociology"—the title of a 1968 essay, which advocates the transformation and transcendence of categories such as base and superstructure (cf. p. 111); and later he warns the young critics of high culture and the university that "cultural forms are never the direct result of social process" (p. 411). His concern with a critical transcendence of Marxism made Birnbaum act for years as a kind of broker between writers in East and West. Several of his travel and conference reports describe the emergence and incipient decline of an East European intelligentsia similar to its western counterpart in its dissatisfaction with material progress alone, whether capitalist or socialist ("goulash communism"). In the meantime, this decline has become precipitous not only in Poland and Czechoslovakia but also in Hungary and even Yugoslavia—witness the expulsion of three sociologists and philosophers from the Hungarian Socialist Workers' Party on May 14, 1973 and their dismissal from professional positions, together with four nonparty philosophers, and witness also the gradual re-establishment of party authority over the University of Belgrade (as reported in *The New York Review of Books*, Feb. 7, 1974).

This course of events leads me, finally, to some observations on the fortunes of critical sociology. Within the state-socialist realm the adoption of views similar to those of western liberal social scientists or of their leftwing critics inevitably appears as a "critical sociology," a challenge to the powers-that-be, which sooner or later makes a repressive reaction likely. As the Hungarian Central Committee saw it, the ideological deviation committed by the seven writers was the "pluralization in Marxism-Leninism," made doubly heinous in that they mixed a "rightist-revisionist platform" with "concepts from neo-leftist western ideas alien to socialism" (cf. Radio Free Europe, *Hungarian Press Survey* No. 2309). For the near future at least such critical sociology will be highly circumscribed in these countries. In the West one ambiguous source of support for critical sociology has also lost ground. Birnbaum's collection does not reflect yet another "end of ideology," the waning of the recent academic rebellion. His last essays were written at the height of the student rebellion, but while taking a sympathetic attitude toward its intentions or felt causes, he was quite skeptical of some of its major manifestations, such as its "almost barbarian disregard for what is positive in the legacy of the past" (p. 410), its inability to recognize that the university must be a protected space in which a critical culture can develop, and its failure to understand that "the counter-culture movement does not constitute a substitute for a socialist politics in America" (p. 387). What, then, makes the author's critical sociology different from ours? He assures us that he has a "critical intention with respect to society" (p. vi), but this attitude does not seem to be decisive for the quality and tenability of his analytical points. Maybe he wishes harder than most of us that American social science and society were different and better, but almost all social scientists seem to have plenty of good intentions as well as many dissatisfactions. And good intentions alone are not a precondition for scholarship, which is inherently and irreducibly critical if it truthfully follows its own canons.

GUENTHER ROTH
University of Washington

Politics in Science. By Marlen Blissett. (Boston, Mass.: Little, Brown and Company, 1972. Pp. 229. $4.25, paper.)

Marlen Blissett's Book, *Politics in Science*, is full of ideas and questions. Its "major purpose," as Blissett states in his very first sentence, "is to uncover the political techniques the scientific community utilizes in focusing and coordinating its research." But the book goes much farther, as it has to. For the criteria and system by which scientists make choices about their research are increasingly affected by the configurations of power in the broader political system within which they work. And to go even farther, in the United States (and undoubtedly more generally in all industrialized countries), the political system itself is in a state of transformation. Having already shifted, according to Professor Blissett, from a liberal to a bureaucratic model, it now is moving toward a "post-industrial" model. Through each transformation, the autonomy of science—and thus the capacity of scientists to make their own decisions about their work—becomes increasingly weakened *unless* (and this has all kinds of implications) they become a political force of their own. But here, like paradox or (for some) tragedy, political activism can tend to weaken professional autonomy even more. There is no return to paradise.

Blissett builds his analysis on the broad literature that has emerged on science and society, particularly in the last twenty years. He makes frequent reference to Kuhn and uses Sheldon Wolin's concept of "political space" as a way of looking at the distances and relations between science and the state. From Wolin, he moves forward with the writings of Michael Polanyi, Alvin Weinberg, Don K. Price, and Stephen Toulmin, using each to illustrate a different model of those relations along a spectrum from Polanyi's view of science as an autonomous profession to Toulmin's view of science as enmeshed in the power relations of society. For Blissett, it is Toulmin's view which comes closest to unfolding the situation of science in the "post-industrial state," though Blissett's analysis of the "post-industrial state" at the very end of his book is brief and incomplete since, by his own admission, "it may be hazardous to speculate about an uncompleted development." Indeed, the very term, "post-industrial state," which we all have borrowed and use, shows up the poverty, or temerity, of our analytical capacities; it tells us where we have been, but not where we are going.

Blissett also depends on the results of a questionnaire submitted to a broad range of scientists on their perceptions of their disciplines and professional communities, and on depth interviews with a smaller sample. The responses are used to reinforce the analysis throughout the book and are partially summarized in a series of useful charts, included in the appendices with the questionnaire and a note on research design and methodology. Blissett, moreover, invites his readers to write to him should they wish further data from the responses to the questionnaire. What he provides, however, already indicates the dilemma that scientists realize they are in: a deep attachment to professional autonomy, but a foreboding sense of its vulnerability.

Don K. Price's early book, *Government and Science*, gave many political scientists their first set of intellectual tools to deal with the political implications of science and technology that weighed on our minds from the time the awesome consequences of the wartime Manhattan project became public knowledge. Since then, a widening band of teachers and researchers has grappled with the increased complexities of a science- and technology-based polity. Blissett helps carry on the inquiry. He begins most especially to identify the linkages between his major questions about "politics in science" and the growing questions about "science in politics." Science and politics are now intimately entangled; let there be no question. But there is for all, and not only for scientists, an irreparable loss if this entanglement completely destroys the autonomy of science. Blissett makes no judgment; but his analysis underscores the problem—and the danger.

GENE M. LYONS
Dartmouth College

Science, Values and Politics in Max Weber's Methodology. By H. H. Bruun. (Copenhagen: Munksgaard, 1972. Pp. ix, 300. 62.10 Danish Kr. [$9.00].)

Max Weber's strategic demand for "value-freedom" and "objectivity" in sociocultural and historical sciences has been the focus of unceasing controversy, bitter polemic, and political, philosophical, and methodological misunderstandings ever since its publication in 1904. The depth, intensity, and persistence of these misunderstandings is evident from even a cursory review of recent critiques of the presumed implications of Weber's position, critiques in which we encounter such typical and recurrent arguments as the following:

(1) Weber's demand for a "value-free" social science is a politically pernicious positivism. "Value-free" explanations have the effect of legitimating the events they purport to "explain" by excluding critical judgments, and by hindering perceptions of the self-contradictions immanent in all historical sociocultural processes.

(2) "Value-freedom" is a logical absurdity, for no one can engage in social or historical inquiry without some commitment to value. Better an open and explicit acknowledgment of values than illusion, pretense, or political humbug disguised as "neutrality."

(3) A "value-free" social science is a prescription for a sterile social science because it would block cognitive access to the constitutive moral dimensions of sociocultural objects and realities. All historical sociocultural objects are constituted by value orientations, moral commitments, and life directions psychoculturally immanent within the concrete histories of political communities, socioeconomic institutions, religious movements, and cultural productions.

(4) "Value-freedom" is the ideology of a technocratic, managerial, and instrumentalized scientism. It is the expression of a functionalized "one-dimensional" mode of "rationalism" that negates all possibility of a humanistic critique of taken-for-granted social and political goals.

(5) "Value-freedom" is a transparent attempt to legitimate the opportunistic accommodation of social and political theorists to the dominant class and de facto power structures of society.

(6) "Value-freedom" encourages a nihilistic "decisionism" that divorces moral and political judgments from historical experiences, natural realities, and cultural, social, and religious traditions for the sake of atemporal, capricious acts of political will.

(7) As an alternative, there are many contemporary social and political theorists who champion modes of "reason," cognitive presuppositions, and traditions of inquiry which (it is alleged) promise to declare universally and unambiguously to the consciences of all "rational" men of "good will" the true basis of valid critical judgment and moral consensus. These alternatives (e.g., "natural law," "philosophical anthropology," "dialectics," "emancipatory or critical reason," "biocentric ecology," "linguistic universality," "cybernetic culturology," "analog rationality," "Platonic realism," "rational contractual egalitarianism," "true community" or "fraternity," etc.), are presumed to be incompatible with "value-freedom."

One of the merits of Dr. Bruun's new study of the relations of "values" to "science" in Weber's methodological essays, is the clear implication that the consequences imputed to Weber in the above arguments, are not logically entailed by Weber's concept of "value-freedom." Dr. Bruun's book, despite several limitations—an emphasis upon exegetical exposition rather than systematic critique, and the deliberate neglect of Weber's sociological works—is the most thorough and scrupulously close textual analysis of Weber's notion of "value-freedom" and its cognate ideas now available. The five chapters of Dr. Bruun's book are titled: (1) "Values as a problem of scientific inquiry: value freedom"; (2) "Values as a precondition of scientific inquiry: value relation"; (3) "Values as an object of scientific inquiry: value analysis"; (4) "Values as an instrument of scientific inquiry: the ideal type"; and (5) "The complementary relation of values and scientific inquiry: politics and science." Dr. Bruun's close textual analyses, and citations from Weber's unpublished letters and manuscripts establish that Weber was neither an apologist nor an advocate of narrow positivism, nihilistic de-

cisionism, or technocratic scientism. Weber's mode of value analysis was intended to turn the light of logical and empirical causal analysis upon taken-for-granted social conventions, political and religious dogma, and cultural presuppositions.

As Dr. Bruun notes, Weber's assertion of a logical gap—a dualism between the heterogeneous realms of "is" and "ought"—was an attempt to distinguish between empirically demonstrable "fact" and concrete value "preference." But "value-freedom" ("*Wertfreiheit*")—the denial that *practical* value judgments (i.e., expressions of concrete preferences) were obligatory for social scientists in "empirical" (not "policy") sciences—was a misleading term; for "a 'wertfrei' science is *not* free from all value elements" (Bruun, p. 16). It should more accurately have been called "freedom from value judgment" ("*Werturteilsfreiheit*"), for Weber did not intend to exclude "values" either as the object of study or as a precondition of empirical social science (Bruun, p. 16). Weber's tactical demand for "value freedom" was iconoclastic, not opportunistic. It was not an argument for moral indifference, but—in the historical context of an intensely nationalistic German educational and cultural establishment (where Weber himself was a nationalist)—it was devised to encourage the emergence of neutralized sociocultural spaces; in these "thought zones" theoretical interests might flourish, at a culturally legitimated distance from the immediacies of subjective passion, political or religious pressures, and institutionalized dogma.

From Dr. Bruun's careful reconstruction of Weber's complex argument it is evident that many critics who regard Weber's move towards logical clarification as unacceptable on either theoretical or moral political grounds, have usually overlooked the heuristic location of "value-freedom" within Weber's comprehensive subtle research strategy, by means of which, "values"—at several levels of theoretical sublimation—were incorporated *heuristically* (but not "practically") into the search for "objective" empirical truths. One source of the distinction between the "practical" and the "heuristic" (a distinction contested by some) is traced by Dr. Bruun to a previously unpublished letter to Rickert (June 14, 1904) in which Weber wrote: "*Ihre Zustimmung zu dem Gedanken des 'Idealtypus' erfreut mich sehr. In der That halte ich eine ähnliche Categorie für notwendig, um 'werthendes' und 'wertbeziehendes' Urteil scheiden zu können*" (cited in Bruun, p. 207). The evolution in Weber, from the language of "immediate" or practical evaluation to the "contemplative" language of "value relevance" ("Wertbeziehung") and "ideal type" (see p. 158 of Weber's *Methodology of the Social Sciences*, New York: Free Press, 1949), represented a conceptual shift from levels of immediate dogmatic affirmation to modes of theoretical heuristic intentionality. Nonetheless Weber insisted that a researcher's cultural sensitivity to values was indispensable. It was likely to prove fruitful precisely when one was able to sublimate the most deeply felt value concerns (but not repudiate them!) for the sake of a theoretically mediated causal analysis of culturally "significant"—that is, value-saturated—historical objects (e.g., "the Protestant ethic," "Faust," "Germany," etc.).

Dr. Bruun has chosen to ignore the relationship of Weber's value-analytic and ideal-type methodology to his mature sociology. Instead Bruun emphasizes the "complementary" relations of science to politics in Weber, and argues that the demand for "value-freedom" was primarily "indebted to a growing awareness of the importance of values other than science" (Bruun, p. 76). Weber, however, was always aware of the "polytheism of values." When Weber moved strategically towards "value-freedom" in 1904 it was to break through the stultifying methodological dualisms, restricted epistemological horizons, and value parochialisms of German historicism. Weber remarks in his essay on Eduard Meyer that a proper value analysis would have to become causal-historical in order to fully comprehend ideal value-objects.

From the perspective of Weber's mature sociological works, "value-freedom" was a cognitive precondition for the development of a polycentric comparative historical sociology. It was a tactical move in his effort to transcend cultural parochialism; to combine historical with sociological studies in a new way. Weber's value analysis ultimately resulted in the invention of differential sociological typologies (e.g., *Economy and Society*) appropriate for the depth-historical comparison of plural societies, conflicting cultures, and radically diverse civilizations.

JEROME GITTLEMAN
Monticello, New York

Ferdinand Toennies on Sociology: Pure, Applied and Empirical. Selected Writings. Edited with an Introduction by Werner J. Cahnmann and Rudolf Heberle. (Chicago: University of Chicago Press, 1971. Pp. 351. $13.50.)

This volume presents a selection of largely new translations from Ferdinand Toennies's sociological work. Oriented especially to a clarification of the conceptual background of the key notions found in Toennies's renowned *Gemeinschaft und Gesellschaft*, it offers, for English reading audiences, a first full account from Toennies's own hand of his systematic perspective.

A large portion of the book is devoted to translations which present Toennies's "pure sociology"

and its related applications. In addition to four immensely compact statements of his basic concepts in Part II ("The Elaboration of Concepts"), Part III contains a full discussion of his pure sociological concepts of social entities, values, norms, law, social institutions, and the "effectiveness of factors" in social structure. Extensive selections from Toennies's *Einfuehrung in die Soziologie* (1931) are especially in evidence here. "Empirical" and "applied" sociology—the other two divisions of Toennies's "special sociology"—receive less attention. Indeed, the mere dozen pages on "empirical" sociology appear almost gratuitous in the overall context of the volume. The selections from Toennies's "applied" sociology—in the editors' words, that part of his sociology which "applies the static concepts of pure sociology to the dynamic processes of history" (p. xi)—deal with "The Power and Value of Public Opinion," "Historicism, Rationalism, and the Industrial System," "The Individual and the World in the Modern Age," and "The Active Forces of Social Development in the Modern Age."

The first part of the volume, which the editors have entitled "Formation of Concepts," offers fascinating selections from Toennies's discussions both of his own intellectual background and of the origin and development of sociological conceptualization. Here, a revealing autobiographical statement ("My Relation to Sociology"), containing much helpful information about the influence on Toennies of such writers as Otto Gierke, Henry Sumner Maine, Karl Marx, Jacob Burckhardt, and others, is coupled with the prefaces to the two editions of *Gemeinschaft und Gesellschaft* as well as excerpts from his various works on Hobbes. These last—regrettably brief—pages on Hobbes are perhaps the most penetrating of the volume and reveal Toennies in one of his strongest roles—as an interpreter of the history and systematics of sociological concepts in early modern political and social thought. Indeed, his eminence in this area has too often been neglected.

Toennies's early philosophical studies came to fullest fruition in his investigations into Hobbes's work, especially "his writings about the philosophy of law and government" (p. 3), and resulted in the publication of two of that author's major works on political theory under Toennies's editorship and the completion of a lengthy study of Hobbes's life and work. These early studies remained with Toennies and left their imprint on the questions which occupied his subsequent sociological efforts as well as on the modes of conceptualization evidenced in those later writings. Although Cahnmann and Heberle have done well to remind us that Toennies was a foremost Hobbes scholar and a co-founder and first President of the *Societas Hobbesiana*, readers might have hoped for even fuller selections from Toennies's work on Hobbes and the background of modern social theory in the central political, juridical, and philosophical debates of the early modern period.

The editors might also have stressed more directly that Toennies was a largely forgotten pioneer in the analysis of "philosophical terminology," a field which was to become in various forms central to twentieth-century philosophy. Unhappily, they chose not to include a selection from Toennies's book-length Welby Prize Essay of 1898 on that topic.

Indeed, from both these perspectives, the volume appears lacking in appropriate balance. Toennies's alleged "influence" upon later sociologists and his "anticipation" of their concepts, methods, and findings tend to be emphasized at the expense of selections featuring areas where he did some of his most effective and still illuminating work and was in many respects more truly a pioneer.

The differences—as well as the similarities—between Toennies and other sociocultural analysts of his time, notably Max Weber, also require appropriate emphasis; indeed, they become especially evident in the final essays of the book ("Applied Sociology"), which present Toennies's views on the central political, economic, and sociocultural transitions involved in the origins of "modernity." Despite the editors' fitting emphasis (p. 37) on Toennies's methodological foreshadowing of Weber, the latter's use of "ideal type" concepts occurred more in connection with depth-historical, comparative investigations into axial social and cultural processes in the transformations of complex societies in East and West and was carefully circumscribed by the particularity and multiplicity of histories under consideration. This sense of historical variety and complexity is far less evident in Toennies's more philosophical, historical (and rather schematic) rendering of these basic transitions and movements. In this connection, it is worth noting Toennies's frequent recourse to "historical materialism" for explanatory modes in accounting for concrete historical events and processes.

On such questions, however, readers must be content to anticipate the editors' companion volume which promises to contain more systematic comparisons—absent in these pages—between Toennies's writings and those of Marx, Weber, Simmel, Durkheim and others who broke new ground in the analysis of sociocultural process. Until then, the present—most welcome and excellent—translations offer the fullest statement in English of Toennies's conceptual system and are thus indispensible for readers wishing a fuller

picture of the foundations of that author's axial notions of *Gemeinschaft*, *Gesellschaft*, and the fundamental forms of human will.

DONALD A. NIELSEN
Virginia Polytechnic Institute and State University

Hobbes and Rousseau: A Collection of Critical Essays. Edited by Maurice Cranston and Richard S. Peters. (New York: Anchor Books. 1972. Pp. ix, 505. $2.50.)

Nine essays on Hobbes, nine on Rousseau, and one linking essay, make up this volume in a "Modern Studies in Philosophy" series. Seven of the essays were written especially for it, the rest are from journals or books published mostly within the last few years. One expects some unevenness in any such collection, and one finds it here—not as between the published and the commissioned essays, nor as between those by established scholars and those by younger ones (which is not quite the same classification), but as between those on Hobbes and those on Rousseau. This unevenness is not a difference in quality, which is on the whole excellent in both, but in the writers' approach and, consequently, in the constituency they will interest. The papers on Hobbes, though not all by professional philosophers, are, with a few notable exceptions, more technically philosophic analyses, devoted to narrower points, and less rewarding for nonphilosophers, than are the papers on Rousseau (none of which are by philosophers). So the Rousseau papers will probably be more welcome to the political theorist.

Of the already published papers, those on Rousseau are by Leo Strauss (1947), Judith Shklar (1964), Bertrand de Jouvenel (1964), Roger Masters (1965, now first published in English), John Plamenatz (1965), and John McManners (1967); some of these are already classics, and it is good to have all of them in this handy form. The new essays on Rousseau are welcome additions. William Pickles gives us new insights by examining Rousseau's attitude toward time. He considers first the question how far Rousseau was a radical and how far a traditionalist, and comes down in favor of his traditionalism. Yet he shows that Rousseau also accepted the inevitability of change, most of it for the worse. Man is good, men are bad. The pessimism about men, and any institutions they are capable of setting up and running, is consistent with the traditionalism. Rousseau's traditionalism is simply making the best of a bad job; men, having failed at the emergence of civil society to steer away from the moral decline which was not then inevitable but optional, must now be content to put up with what they have got, or at least amend their institutions only slowly. The argument is persuasive, though one might ask if it makes enough allowance for the implied position of the *Social Contract* that the General Will might rule if three stipulations were met (a moderate equality of property, the prohibition or offsetting of private associations, and the moral transformation of the people by the charismatic Legislator), and for the outright assertion there that "the moment the government usurps the sovereignty, the social compact is broken, and all private citizens recover by right their natural liberty, and are forced, but not bound to obey." Depending on the weight one puts on "are forced" and on "not bound," this is pessimism or incitement to revolution. Pickles argues finally that the time dimension in the *Social Contract* is the source of some of the perennial difficulties of that work, the fault being in Rousseau's having cast into a temporal sequence what is essentially an abstract moral argument.

Ronald Grimsley brings together and finds a consistency in Rousseau's varied reflections on happiness, which at first sight seem far from consistent. As seeker of happiness Rousseau is often an escapist, retreating to Nature, to reverie, to minutely shared intimacy—yet he is a believer in a benign harmonious universal order. Grimsley finds an intellectual consistency here, without exploring its possible social roots. Could it be that both these positions are reflections of Rousseau's *petit-bourgeois* consciousness? A similar question might be raised about John Charvet's study of individual identity and social consciousness in Rousseau's philosophy. The contrast between *amour de soi* and *amour-propre* is plain: the former is dominant in the first stage of the state of nature when by hypothesis a man is incapable of comparing himself with others, the latter (which consists in such comparison) comes only at the later stage of already somewhat social savage existence. But why did Rousseau extrapolate backward from a savage society (for which there was some evidence) to an utterly unlikely condition when individuals were so unaware of each other as to be incapable of making comparisons? The question is not asked, but the answer might be that it was required as the logical starting point for a *petit-bourgeois* vision of the good man and the good society.

The already published papers on Hobbes include Brian Barry's incisive criticism of Warrender and his critics (1968), and Quentin Skinner's classic paper (1966, but now in an "abbreviated and much altered version") decisively refuting the common view that Hobbes's works were uninfluential in his own time, and showing the absurdity of the Taylor-Warrender-Hood view that Hobbes's political obligation was based on a traditional doctrine of natural law; they also include

pieces by Richard Peters and Henri Tajfel (1957), J. W. N. Watkins (1965), W. H. Greenleaf (1969) and K. R. Minogue (1969).

Of the three new papers on Hobbes, two will be of interest primarily to philosophers. Ronald Hepburn offers a new view of the role of God in Hobbes's philosophy, which gives new weight to the view that Hobbes's theology cannot serve as a grounding for his moral and political theory. S. I. Benn, minutely analysing Hobbes's position on power (not *potestas* but *potentia*, power not as a right by which men control others, but as possibility, natural faculty, or *de facto* control), demonstrates some inconsistency between the accounts given in *De Corpore* and in *Leviathan* and some other works.

The other new essay in the Hobbes section will be of more interest to political theorists, and may be allowed somewhat longer comment here. William Letwin has devoted twenty pages to arguing that three of the present reviewer's theses about Hobbes are mistaken. The inadequacy of Letwin's case can be indicated in less space. The three theses are: (1) that England in 1650 was essentially a bourgeois market society; (2) that Hobbes's model of society approximates most nearly to such a society; (3) that his prescription of a self-perpetuating sovereign, which made his full doctrine unacceptable to the bourgeoisie, was the result of his failure to see the centripetal force of a cohesive bourgeois class.

On (1), Letwin apparently has not understood and so has misstated my argument from models. I did not argue, as he asserts (p. 145) that England *must have been* a bourgeois (or capitalist) market society *because* it was not either of my other two models: I argued that it was essentially a bourgeois market society because the historical evidence, especially of the extent of the wage relation, brought it into conformity with the main stipulations of the received ideal-type of capitalism (chiefly, that labor is a commodity). Letwin then proceeds to attack my model of that society by examining the attributes I set out, (incidentally reducing my eight to five and leaving out the most important one). He ridicules two of them (factual inequality and inequality of desire) by pointing out that these are characteristic of any known society. Of course they are. I did not "decline to include" (p. 146) either kind of inequality in my other models (customary or status society, and simple market society) nor would any reader who understood the use of the models be "left to infer" that inequality is an essential property only of the bourgeois model (p. 146). Those inequalities were stipulated specifically only in the bourgeois model because only there, where they are for the first time combined with labor being a commodity, and with land and resources being fully marketable, do they become operative factors in the development of the new society.

Two other attributes—that all men are rational maximizers, and that contracts are enforced—are ridiculed (pp. 149–51) on the ground that the desire to maximize, while "probably increasing during the 17th century," had been growing for centuries before then, and that contracts are as old as history. Similarly with the wage relation: Letwin is prepared to grant that about half the men were full-time wage earners, but says "the magnitude of the labour force shows that the phenomenon was not, as Macpherson implies [*sic*], new" and cites evidence of wage legislation in the 14th century. Of course there were some maximizers, some contracts, and some wage earners before the 17th century, but unless one is to say that there is no difference between feudal and capitalist society one must surely go by the measure in which they became prevalent.

As to the second thesis (that Hobbes's model of society was substantially the capitalist market model), Letwin's argument is noticeably thin. He tries to explain away Hobbes's recognition that labor is a commodity, and his scorn for commutative justice, by arguing that his statements to that effect should not be taken out of a very narrow context. He does not attempt to deal with the other evidence, such as Hobbes's dismissal of distributive justice, his views on taxation, and his analysis of changing society in *Behemoth*.

Finally, on the third thesis, Letwin has simply failed to grasp my point that the bourgeoisie would have no objection to an all-powerful sovereign body if they *were* that body, that is, if they as a class controlled Parliament (which would, as I said, make them, not Parliament, the sovereign). This failure is perhaps attributable to his belief that "there was no such thing" as "the bourgeoisie as a class" (p. 159), a belief natural enough to one who sees no substantial difference between the extent of capitalist relations in Hobbes's time and in the preceding centuries.

One may say, as a general rule, that when Letwin fails to understand the argument he imputes a different argument which he is easily able to shoot down. Readers familiar with the three theses will know how imputational and inaccurate Letwin's criticisms are; for others, it seems proper to put some inkling of the position on the record.

C. B. MACPHERSON
University of Toronto

In Defence of Politics, second edition. By Bernard Crick. (Chicago: The University of Chicago Press, 1972. Pp. 204. $7.00, cloth; $2.25, paper.)

The first edition of this book appeared in 1962. In 1964, a Pelican edition appeared, with minor revisions and the addition of an appendix en-

titled "A Footnote to Rally the Academic Professors of Politics." The present edition is a reissue of that revised edition (with a somewhat expanded Preface).

In the Preface to the first edition Bernard Crick wrote that his essay was " 'occasioned'—in the words of Hobbes—'by the disorders of the present time'." If any of those disorders have ceased, one suspects they have been more than replaced by new ones; hence a defense of politics is certainly as needed now as it was then. The tendency is widespread to begin by rejecting politics as usual and to end up rejecting politics as a whole.

Crick defends politics by giving it a precise meaning narrower than is customary: it is "the activity by which differing interests within a given unit of rule are conciliated by giving them a share in power in proportion to their importance to the welfare and the survival of the whole community" (p. 22). For politics in this sense to exist, public order must exist already. Crick distinguishes political rule from the establishing of public order, the exercise of sovereignty, the creation of a new state by a prince. Such extreme actions are needed to provide the conditions for politics, but politics itself then has a fundamentally different character, above all from the observance of restraints and limitations on action. Political rule is distinguished from autocracy and tyranny, as well as from modern totalitarianism; it is, in short, republican government or mixed regime.

In the bulk of this book Crick defends politics against two sorts of people. First there are totalitarian ideologues, democrats in the Aristotelian sense, nationalists, and technocrats, who are all opposed in some fundamental way to the political manner of acting. Second, there are those who usually favor politics but whose characteristic attitude or impatience sometimes makes them hostile to it: nonpolitical conservatives, apolitical liberals, and antipolitical socialists.

One wonders how best to describe the foundation on which Crick's prudent and moderate acceptance of politics in its complexity and with its necessary limits rests, while others either habitually or at least occasionally are carried to excess. His prudence and moderation remind one of Aristotle. Crick begins and ends his book (not counting the more academic "Footnote . . . " added to the revised edition) with Aristotle—with Aristotle's assertion, against the Socratic paradox that the perfection of the *polis* is its unity, that the *polis* is an aggregate of many (and differing) members. And he frequently refers approvingly to other important Aristotelian assertions, for example about politics as the master science (p. 106 and elsewhere) and about democracy (pp. 66–7). Yet Crick differs from Aristotle in some fundamental respects. First, he does not want to argue that good political practice must aim at the common good or be guided by a consensus on fundamentals. Such notions are rejected as pretentious claims, unnecessary extrapolitical "spiritual adhesive" or "metaphysical cement"; the real good is the activity of politics itself, the preservation of the political way of ruling (pp. 24 and 181). Second, although Aristotle like Crick asserts that some goods transcend the realm of politics, Crick does not investigate precisely what they are; one supposes they must arise somehow from the personal identity, absolute personality, and human autonomy of the individual (pp. 51–56 and 181). Third, whereas Aristotle asserts that the political association is the sovereign association, which aims at the comprehensive good of man, Crick stresses the limits on the aims of politics and usually emphasizes the low or basic concern of politics with survival; when he speaks of the further purposes of politics, his usual intention seems to be to point out the variety of such purposes and the crucial importance of proceeding in a political manner (pp. 24–5), that is, one in which retreat and compromise remain always open (p. 151).

Accordingly, Crick is closer to being a pluralist liberal than an Aristotelian. He agrees with liberals that politics should be limited in scope; "True politics cannot treat *everything* in political terms" (p. 42). He finds it necessary, however, to leave politics enough flexibility to deal effectively with unforeseeable circumstances; hence he rejects the characteristic liberal attempt "to try to remove definite spheres of human activity from any possibility of political intervention or influence" (p. 133). And his politics is a version of pluralism (p. 176n.), but unlike some pluralists he gives prominence to the fact that "Politics embodies an ethic and a conscious purpose which cannot be reduced to sociology" (p. 177). Politics requires a recognition of restraints (p. 21—the attempted distinction between moral and prudential recognition fails), a will in the governing elite to act politically (p. 180). The (moral) choice of politics needs to be backed up truculently. "For it is, after all, too kind (indeed perverse) to respect the morality and wisdom of any who, when politics is possible, refuse to act politically" (p. 31).

Crick's praise of moderation is admirable in many ways: it is presented with considerable clarity, admirable brevity, frequent felicity of expression, and wit. One cannot help fearing, however, that it fails to meet the most radical critiques of politics. Crick asserts that there are human ends higher than politics; he argues that politics cannot attain them, but can provide the necessary conditions for striving for them (pp. 156 and 159). But why might these ends not justify violating the limits on action that characterize Crick's politics?

One could meet this objection by showing that these higher ends cannot be attained by such actions (as Plato does in the *Republic*). Or one could make a kind of Kantian argument that the (moral) principle on which limited politics rests is the foundation of any genuinely human good. Crick seems to take such a position when he asserts that a "consensus about individual human autonomy . . . is the presupposition of *any* possible ethical system" (p. 181). However, aside from the fact that this view resembles the "metaphysical cement" that seemed undesirable to him earlier in the book, he does not try to argue for this position or to investigate whether it would not undermine the political prudence that he is rightly concerned to defend. (For this reason, among others, Aristotle does not make "individual human autonomy" the presupposition of his ethical-political teaching.) Or, finally, one could argue that limited and liberal politics achieves the common good of the society better than any alternatives past or present; to do this effectively one would need to compare the whole way of life to which this politics leads with the alternatives, as Montesquieu does. Crick seems to agree with this view, but not to make the argument, on account of a reluctance to specify the common good and the whole way of life of a society. He rightly fears that such notions can be used in partisan, extremist, and even totalitarian ways. But the adequate defense of politics against its deepest critics may require thorough investigation along such possibly dangerous lines. And as Crick says in another, but related, connection, it is not a very safe world anyhow (p. 28).

JAMES H. NICHOLS, JR.
Claremont Men's College

Société et Politique: La vie des Groupes. Tome premier: Fondements de la Société Libérale. Tome second: Dynamique de la Société Libérale. By Léon Dion. (Québec: Les Presses de l'Université de Laval, 1971, 1972. Pp. 444 and 616. No price indicated.)

Pluralismus: Konzeptionen und Kontroversen. Edited by Franz Nuscheler and Winfried Steffani, (München: R. Piper & Co., 1972, Pp. 345. No price indicated.)

Pluralismus zwischen Liberalismus und Sozialismus. By Rainer Eisfeld. (Stuttgart: Verlag W. Kohlhammer, 1972. Pp. 195. No price indicated.)

These are important contributions to the discussion about the political structure of post-capitalist society. Their particular value to us lies in their critical re-evaluation of American controversies and findings which the authors and editors are able to view from a different historical experience and against a variety of intellectual traditions, half forgotten or little known in the United States.

While Professor Dion's central theme is the analysis of group influence in the political process, his massive volumes encompass far more than the works by Truman, Key and others. Like Jean Meynaud, Almond, and Eckstein, he searches for a framework which would help order data and make interest group studies more truly comparative. While he too despairs of a "group theory of politics" he nonetheless finds that Easton's systems analysis, especially in its later refined versions, supplemented by some of the concepts of communications theory, is excellently suited for the study of groups. To the French reader Dion's elegant and faithful summary (supplemented by a series of original and clarifying graphs) of the systems theory will be highly profitable. What appears more doubtful to me is whether the author has really shown that the systemic approach looses its abstractness and gains in explanatory power when used for an analysis of the formation, the activities, and the general role of interest groups in the political process. Where Dion engages in the comparison of the impact on groups of political cultures, of traditional or of innovating attitudes and ideologies, he systematizes usefully existing knowledge and occasionally adds to it. (One might regret that the author, Québec's leading political scientist, draws relatively little on Canadian material.) But where he seeks to filter the dynamic relationship between groups and society through a rigorous systems analysis, he does not avoid the pitfalls he criticises in others: his modeling is often abstract and his concepts tautological.

But however one wishes to evaluate its relative success or failure in the general field of theory building, this is not the main purpose of the book or the principal concern of its author. What he furnishes is an up-to-date synopsis and broad-gauged discussion of all major problems faced by pluralist democracies and an extensive review of the literature pertaining to analytical as well as to normative pluralism. The footnotes and a systematic bibliography (of 90 pages!) are almost too rich and hence confusing. To be sure his critical review of the existing literature is not only complete; it is also eminently fair. Yet, just because the book will serve both as a basis for future discussions and a useful working tool, it would profit from a thorough editing that should eliminate at the very least repetitions not only of quotations, but also of tables and other materials that appear in both volumes.

The problems of representation and of participation are central to the consideration of what Professor Dion calls the "dynamics" of modern

society. Like Max Weber, Léon Dion emphasizes the ordering and integrative function of the defense of material interests and even of ideological values. Unlike Weber, however, he is convinced that not only the institutionalized representation of functional interests will mitigate the consequences of direct access of pressure groups to the centers of decision making but also that consultative boards, tri-partite committees, etc. have a necessary representative function. (Weber foresaw, and quite realistically, that such arrangements would do little more than increase the relative weight of the bureaucracy.) Dion's comparative discussion to this point is interesting enough, but not exactly convincing. To him consultative boards occupy (or at least should occupy) in the total political system, the same position as political parties, interest groups and mass media. He fails to see to which extent the boards are either ineffective or, when effective, an additional institutionalization of interest groups. Probably the author's down-grading of the role of political parties parallels his prospective views on consultative boards. There is accordingly, in a work devoted to a study of the dynamics of democracy, relatively little discussion of the problems of political opposition.

Throughout the work, the author has difficulties with the central question of the pluralist state: how much integration is needed for the survival of the community? Has not experience shown that in many settings the best integrated groups might also be the most oblivious of the needs, as opposed to the assumed interests of their members? Probably as a result of such uncertainties, Professor Dion never deals decisively with the arguments put forward by Mancur Olsen or Theodore Lowi, although he is fully aware of them.

The corrective which Professor Dion seeks for existing maladjustments is a more participatory form of democracy. His extensive consideration of this concept avoids many of the romanticized versions that have characterized previous discussions. Yet the impassioned description of the present crisis leads to a final part which reads more like a manifesto for substantial reforms than the conclusions of a scholarly work.

The collection edited by Nuscheler and Steffani combines the usefulness of a reader of writings on pluralism (over a time span of 70 years) with an interesting presentation of controversies on the subject. The volume furnishes an excellent supplement to Professor Dion's book whose discussion is limited to English and French sources. With a few exceptions, among them the classic mutations of Harold Laski's thinking from 1915 to 1938, the volume reproduces, together with an excellent introduction by one of the editors, mostly writings by German authors. A comparison between the German discussion and the concepts and controversies presented by Dion provides a striking demonstration that in Germany the problems took on a deadly seriousness which they have lacked, at least until recently, in North America. Initial positions were not far apart: the seminal discussion by von Gierke (1902) had a major and acknowledged impact on Arthur Bentley, who was still able to adjust Gierke's insights to American realities. But during the torments of the Weimar Republic, Carl Schmitt took Laski's earlier exaggerations about the desirable liquidation of the state as proof that pluralism was impeding the "decisionism" without which the state could not survive. Rudolf Smend, at the same time, called for an "integration" which, as he now explains, was at the antipode to Schmitt's notion, based as it was on a fundamental personalism—in fact, a proposal not far from Professor Dion's present stance.

In postwar Western Germany, the discussion has not abated. The most interesting writings are those by Ernst Fraenkel who has insisted that the negation of pluralism by totalitarian thought should now be negated by a neopluralism which keeps an equal distance from the utopians who expected sovereignty to dissolve, and from an enforced harmony à la Rousseau. Thoroughly familiar with the American discussion and probably closest to Dahl in his thinking, Fraenkel is nonetheless on many points original enough. The conservatives' attacks on him are represented here by writers who cling to Schmitt's tradition, presumably in order to save Bonn from the fate of Weimar. More weighty are the "disquieting question-marks" which exist for the late Karl Loewenstein when he discusses the relationship between "the pluralism of groups and the dynamics of power." A broad range of leftist criticism includes Marxist and non-Marxist authors; their arguments parallel in part those voiced by the critics of American pluralism, but there are interesting variations on the familiar theme.

Among the critical voices, Rainer Eisfeld's is the most intriguing because of the breadth of historical approach and the concreteness of analysis revealed in his book. To him pluralism has indeed added a political dimension to the concepts accredited by the liberal state and has thereby broadened the citizens' role in politics. But because of the limited representativeness of the groups, and because the inequality of resources will constantly hamper possibilities for effective representation, he believes that only a socialist reorganization of society can truly meet the challenge which pluralism wanted to present. (This view of course, returns to some of Laski's propositions.)

The contributions to the Nuscheler-Steffani

collection by an Italian and a Czech Communist (the latter, O. Sik, now an exile) raise the question, as it were, from the other end of the telescope: should one not concede, they ask, that also in socialist society, pluralist practices *and* principles are indispensable? These questions take on additional significance because political scientists in the Soviet Union and in Eastern Europe seem now to be venturing for the first time upon studies of interest group activities in their systems. Does one dare hope that, before 1984 befalls us, students and practitioners of politics will yet be converted to Pascal's wisdom (of which Ernst Fraenkel reminds us): "*La multitude qui ne se réduit pas à l'unité, c'est confusion; l'unité qui n'est pas multitude est tyrannie.*"

HENRY W. EHRMANN
la Sorbonne

Contradiction and Dilemma: Orestes Brownson and the American Idea. By Leonard Gilhooley. (New York: Fordham University Press, 1972. Pp. 231. $10.00.)

This book is renewed testimony to the veracity of Arthur Schlesinger's comment of 1966 that Orestes Brownson remains "vigorously alive as a figure in American intellectual history." Brownson appears to be generally regarded, on the basis of his essays of 1840, *The Laboring Classes*, as a "utopian liberal" (Mason) or a "pre-Marxian Marx" (Schlesinger) who turned conservative because of his disillusion with the presidential election of 1840 and, more particularly, his subsequent conversion to Catholicism. Gilhooley's careful and well-written analysis of Brownson's writings from 1838–1860 shows that because conservative tendencies in Brownson predate the famous essays his thought possesses a continuity which suggests that the essays were "an extremist assault upon an extreme position" and did not necessarily represent the main current of his early thought.

Gilhooley stresses "turns in emphasis" in Brownson's thought within the context of Brownson's approach to the "American Idea" defined as embracing the "American dream" of democratic equality, the "American experiment" of government of the free by the free, and the "American Mission" of the spread of democracy. Brownson originally assumed the validity of the idea of progress, was optimistic concerning man, and conceived of democracy and Christianity working together toward human betterment. He was, however, no majoritarian democrat or enemy of private property. As his thought developed he qualified his view of progress by his emphasis on divine providence, stressed more strongly the sovereignty of God and objective justice over the "proximate" sovereignty of the people, pointed up the ubiquity of original sin, regarded religion as a precondition of virtue and virtue as a precondition of democracy, focused on the historical providential constitutions of peoples, exalted tradition and the Anglo-Saxon common law heritage, and denied the universal exportability of American democracy, stressing instead the American mission of the spread of Christian society.

Certainly the ablest American Catholic intellectual of his day, Brownson believed that Locke's philosophy undercut individual freedom by denying to men inherent powers to attain truth. At the same time, he scored Transcendentalism for not regarding the "manifold and diverse" (p. 70) world of the senses. Because Protestantism stressed individual judgment and disregarded human solidarity Brownson considered it incompatible with civilization and good government. Liberty and order, he thought, could only be reconciled against the background of the voluntary acceptance of what he regarded as the divinely-appointed institution, the Catholic Church. Without this sheet-anchor, democracy would issue in license and majoritarian despotism. Thus Gilhooley tells us that the "American Idea" in its interrelated and interacting components was revised by Brownson in light of his religious preference as well as his regard for what he conceived to be irreducible aspects of human nature and historical influence and continuity. To bring together the emphasis on the necessity for Catholicism and the regard for historical fact, Brownson attempted to show the medieval higher law lineage of American republican ideas.

This book might be criticized for lacking critical analysis and evaluation. Its professed purpose, however, is to describe the development of Brownson's thought as a preliminary to further study. In that aim Gilhooley has succeeded and has thus greatly aided students not only of Brownson's thought but of nineteenth-century American thought as well.

A. J. BEITZINGER
University of Notre Dame

John Dewey's Philosophy of Value. By James Gouinlock. (New York: Humanities Press, Inc., 1972. Pp. 377. $10.00.)

John Dewey's thought has generally been characterized by others in three ways. The first, and perhaps the most widespread among those who have not read Dewey's works extensively, is that Dewey rejected theory in favor of a total commitment to the resolution of relatively short-range, practical problems. The second is that Dewey's thought is theoretical, since he recognized that resolution of practical problems usually requires abstraction and generalization but that it represents social theory or social anthropology, rather

than a philosophical system incorporating metaphysical assumptions. Both of these characterizations assume that Dewey rejected questions of metaphysics as useless and meaningless.

Professor Gouinlock, however, offers a third view, characterizing Dewey's thought as a coherent system about the nature of reality. In addition, he attempts to illustrate that Dewey's conception of value, including his social philosophy, is based on this systematic view of nature. He largely succeeds in both endeavors. Although he is often repetitious, Gouinlock writes clearly, is well acquainted with Dewey's most important works, and has obviously assimilated Dewey's thought.

Approximately a third of the book is concerned with an analysis of Dewey's philosophical system, which is in effect a defense of naturalistic metaphysics. It is also an attack upon the systems of "philosophical dualism" which seem to have characterized the whole of Western philosophy. Philosophical dualism, according to Dewey and Gouinlock, is the assumption of separate realms of being, which results in a number of insoluble and nonsensical intellectual problems. The most important of these problems, at least from a social scientist's perspective, is the mind-body dichotomy, created by the assumption of both psychical and physical realms of being. This problem is of special concern to the social sciences because the origin of the "fact-value dichotomy" lies in the assumed distinction between "material" phenomena and "mental," or "subjective," phenomena.

Dewey's denial of subjectivity is the most striking feature of his system. "Consciousness," "intelligence," and other psychic phenomena are "functions" of an inclusive "situation." "Situations" are, apparently, "experiences" which involve conscious human participation, and experiences are natural events. It is not exactly clear whether "nature" and "experience" are synonymous terms; Dewey's metaphysical monism would seem to dictate this, and Gouinlock appears to proceed on such an assumption. At any rate, Dewey denies autonomous ontological status to consciousness and gives it instead to "experience." Gouinlock then proceeds to isolate the generic traits which Dewey finds common to all forms of experience.

Gouinlock illustrates his abstract discussion of experience with a more specific discussion of value and commitment. Once subjectivity has been abolished, the fact-value dichotomy and its accompanying perplexities cease to be real problems. Values are intrinsic to experience itself, as are the "facts" and the ability to discern them. In Dewey's formulation, "value" is explained as an aesthetic phenomenon experienced during the consummation of a situation—either in actuality or in imagination. The individual does not arbitrarily choose his values; they are simply entailed in his situation.

The crucial question is whether subjectivity can be denied without self-contradiction. Certainly the burden of proof falls upon Dewey and Gouinlock, since the philosopher of common sense does not conform to our commonsensical perceptions on this issue. More important, his position seems to have a logical difficulty. How can one claim to have discovered the generic traits of experience while denying to himself the autonomous ability to do so? Obviously, experience has revealed itself. But why only to, or in, Dewey and his relatively small band of followers? Dewey has not established any criteria by which he can claim primacy for his philosophical assumptions. The "test of experience," so often relied upon by Gouinlock, would seem to lead one to a self-contradictory relativism. This is especially suggested by Dewey's emphasis upon the social nature of intelligence. How does Dewey escape the well-known logical difficulties that have plagued the "sociology of knowledge"? Dewey might very well have argued that worrying about such questions is a waste of time, but this argument does not render them nonsensical. Since Gouinlock argues that Dewey's thought is a coherent, defensible system, his book would have been greatly improved if more space had been devoted to refuting, if possible, such obvious criticism. Unfortunately, Gouinlock tends to defend Dewey's system by oversimplifying and summarily dismissing the major alternatives—a procedure used extensively by Dewey himself.

These deficiencies should not impair the potential usefulness of this book for political scientists. Dewey's conceptions of "community" and "democracy," actually synonymous terms for him, are extremely suggestive, and his ideas concerning the nature of evaluation and human "growth" contain a wealth of insight when viewed as refutable propositions of social theory rather than as logically derived parts of an inclusive philosophical system. Gouinlock explicates these aspects of Dewey's thought with considerable skill. More important, however, given the current vogue of "transactionalism" (originally explicated by Dewey and Bentley in *Knowing and the Known*) among some political scientists, Gouinlock effectively explicates the philosophical assumptions entailed in such a view of the purpose and method of social analysis.

EDWARD B. PORTIS
University of Georgia

L'Explication Politique: Une Introduction à l'Analyse Comparative. By Alfred Grosser. (Paris: Armand Colin, 1972. Pp. 183. F 25.–)

In this essay, Professor Grosser, better known

among American political scientists for his works on foreign policy and German politics, has undertaken a difficult job. He reviews the state of our discipline, only to raise, without always answering, some of the most searching questions. What is a political phenomenon, and how and why it should be studied. Is our discipline—or rather can it even become—cumulative? What are the limits of objectivity? Can we ever arrive in general terms to the Olympian heights of explanation, i.e., of being able to find even two interconnected and mutually interdependent variables in the sense that whenever and wherever we have one there is also the other? What is the importance of value judgments not only in the procedures we follow to do research but, even more important, in the very choice of the topics that we study?

Well-read in American political science, Grosser joins the ranks of many who have become progressively disenchanted with quantification, not only for the sake of quantification, but, with its inherent limitations; with survey research that unless put in context provides only fragmentary and almost snapshot reflections of attitudes and orientations (see how quickly the findings of the *Civic Culture* read like ancient history); with the plethora of concepts and terms that mean different things to different people and above all with the most damning emphasis, in the name of behaviorism, on all the factors that may *potentially* affect politics and policies while avoiding the very stuff of politics—the agencies that formulate claims and press for decisions and the state organs through which decisions are made.

It is unfortunate that Grosser's book has not been translated into English. It is a very thoughtful inquiry into the state of our discipline and many will regret that having diagnosed a number of the ills that beset us he did not venture to give us his own prescriptions of how to cure them. What is more, there is really very little on the procedures and techniques of comparative politics, as such, except in his concluding chapter—15 pages in all. In the best French philosophic and literary tradition he has produced an extremely provocative critical essay.

ROY MACRIDIS

Brandeis University

Revolutionary Justice: The Social and Political Theory of P.-J. Proudhon. By Robert L. Hoffman. (Urbana: University of Illinois Press, 1972. Pp. xvii, 429. $15.00.)

General Idea of the Revolution in the 19th Century. By P.-J. Proudhon. Translated by John Beverly Robinson. (New York: Haskell House Publishers Ltd., 1969. Pp. 302. $10.95.)

Pierre-Joseph Proudhon is generally known to us as an outspoken social critic and indefatigable man of letters who called for a nonviolent political revolution leading to anarchy. Beyond that, we know little about him or his thought. Professor Hoffman's book is helpful in that it combines a description of Proudhon's life—his activities as a publisher, businessman, journalist, and practicing politician; as an acquaintance of Marx and Bakunin; and as a political prisoner and exile—with a statement about the general perspective and the depth of Proudhon's thought. The translation of Proudhon's *General Idea of the Revolution in the 19th Century* provides us with the most complete statement of his views in English and with a good example of his immoderate style.

Proudhon's book brims with outrage over social and economic injustice and with scorn for traditional attempts to remove or reduce that injustice. The outrage and scorn are prompted by his passion for greater individual liberty and conviction that it is stifled by all existing political institutions. In his eyes, the only solution is a revolution which will lead to government being replaced by industrial organization and laws being replaced by contracts between individuals.

The call for revolution is supported by two kinds of argument. One, interjected here and there throughout the book, asserts the historical inevitability of revolution. Proudhon has discovered that history is progress toward liberty and that authority is no longer needed. It is now possible to go beyond Luther's substitution of the authority of reason for the authority of the church and beyond the revolutionaries of 1789 who rejected feudalism only to embrace a different kind of political order. Proudhon will show us how to abolish authority and secure freedom by means of a true revolution (*General Idea*, pp. 101, 111, 218–219, and 292).

In principle, the revolution is merely another change and can therefore be peaceful. However, a bloody counter-revolution led by the established interests or prideful government is always possible. Proudhon tries to avoid such violence by proving the objective need for revolution. The political and economic analyses or studies around which the book is formally organized provide this second kind of argument. Unfortunately, their potential conciliatory effect is weakened by Proudhon's sarcasm.

For Proudhon, revolution is necessary because the goals of the 1789 revolution have not yet been achieved. Feudalism was to have been abolished and work in society organized so that all citizens might benefit from the fruits of their labor. As is universally acknowledged, the very principles which were supposed to liberate the workers—the division of labor, competition, and credit—have placed them under greater enslavement and suffocated the revolution. Proudhon blames not those principles, but a society which has remained

feudalistic and a government which has maintained its old ways. The rich are still favored over the poor and taxes still structured to help the capitalist at the expense of the producer.

No one has yet discovered how to order society correctly. Socialist movements have held up the idea of workers' associations based on the principle of reciprocity. Proudhon rejects that idea, because the individual must bow to the will of the association at some point and thereby allow his liberty to be restricted. Attempts to reduce or abolish authority by limited government or direct legislation by the sovereign people are no sounder. Proudhon claims that authority cannot adequately be reduced, much less abolished, as long as there is government. For example, the most radical proposal for direct government would make the entire populace both sovereign and magistrate. However, the problem then arises of finding a subject over whom the people can be sovereign. The only rational solution is simply to do away with government.

In its place Proudhon proposes the reciprocal justice of contracts between freely consenting individuals. Because association will be based solely on contracts, the subordination and enforced obedience of citizens will be eliminated. The individual will be obliged only to the extent of his freely expressed will and can disassociate himself from any accord to which he does not consent. As a result, social life will be transformed. For example, criminal justice and its spirit of vengeance will give way to mediation between concerned parties and a spirit of reparation. The vision is attractive, but problematic in at least two respects.

In the first place, it presupposes that all of the evil and misery existing in the world are due to society rather than to human nature or to natural scarcity. Proudhon asserts the indefinite perfectibility of mankind and the identity of their interests, while denying the premises held by the defenders of government—the perversity of human nature, essential inequality of conditions, and perpetual antagonism among men. The disagreement is important, but Proudhon's argument never moves beyond the level of assertion.

Here, Hoffman's book could be especially helpful. He has read all of the more than 20,000 pages written by Proudhon and might have shown us how Proudhon answered problems raised in one writing by an argument in another. Instead Hoffman uses his knowledge of the different writings to show only that a given argument can be found elsewhere. He is more concerned with expounding what Proudhon said about such theoretical issues than with questioning why he said it or how it might be correct, probably because he shares Proudhon's historical relativism (*Revolutionary Justice*, pp. ix–xi, 113, 176, and 278).

Hoffman is a better guide for the second kind of problem with Proudhon's vision: its feasibility. A commendable familiarity with economic doctrines enables him to point out the practical flaws in Proudhon's proposals. Recognition of those flaws does not dampen Hoffman's affection for Proudhon, however. He is willing to dismiss Proudhon's political teaching as impractical because he thinks Proudhon's merit lies in his humanist concern, in his ability to point out the shortcomings in political and economic life, and especially in his love of justice.

Hoffman's interest in Proudhon's teaching about justice seems to prompt the curious organization of his book. In the first eight chapters, Hoffman presents a fairly detailed analysis of Proudhon's major works published prior to 1858 and suggests how different events in Proudhon's life influenced the arguments of those writings. There is only a summary statement about the books written from 1858 until Proudhon's death in 1865, and chapter 8 concludes with two poignant anecdotes about his death. Hoffman then changes his method completely and devotes the next four chapters to a very close textual analysis of the post-1858 writings: *De la Justice dans la Révolution et dans l'Eglise* (chap. 9), *La Guerre et la Paix* (chap. 10), *Du Principe Fédératif* (chap. 11), and *De la Capacité Politique des Classes Ouvrières* (chap. 12). Intent upon expounding Proudhon's teaching about justice in these works, Hoffman makes no reference to biographical material in these chapters. The book concludes with a series of speculations about how Proudhon's influence might have been felt and a tribute to his activity as a humanitarian gadfly.

Hoffman has an impressive grasp of Proudhon's thought and of the times in which he lived. From him, we learn to appreciate Proudhon as a man passionately concerned about righting wrong and fighting injustice. We see something of Proudhon's ability to follow an idea out to its end, and we learn about the opinions Proudhon had to combat. Hoffman's use of caricatures and excerpted material at the beginning of each chapter is delightful, as are his accurate and idiomatic translations from Proudhon; it is a pity he had nothing to do with the translation of the *General Idea*. Despite criticisms mentioned above and flaws arising primarily from Hoffman's confusing manner of ordering arguments and habit of raising false issues, he is to be commended for having written a fine and instructive book on Proudhon.

CHARLES E. BUTTERWORTH
University of Maryland

Willmoore Kendall Contra Mundum. Edited by Nellie D. Kendall. With introduction by Jeffrey Hart (New Rochelle, N.Y.: Arlington House, 1971. Pp. 640. $11.95.)

In Willmoore Kendall's life the accidents of

personality and of history so combined as to deny an interesting political theorist the audience he ought naturally to have expected. As Jeffrey Hart notes, Kendall was immensely delighted to discover, on his return to Oxford, that his peers knew of his work. Many of us who have been educated in the social sciences in North America can make no such claim. But now, with this publication of a large, and fairly priced, collection of Kendall's speeches, articles, and the incomplete chapters of a book on which he was working at the time of his death in 1967, Kendall's intense confrontation with those ethical, political, and intellectual issues which are basic to the crises of American society are available to all.

Let us begin with the question, Is there a conservative tradition in North America? All too frequently this question is answered in the negative. For example, George Grant, in *Lament for a Nation* (Toronto, 1965), contrasts the Canadian experience with the American by pointing to the existence of a conservative tradition in Canada and to its non-existence in the U.S.A. Hartz and McRae (*The Founding of New Societies*, New York, 1964) make similar points in asserting that the U.S.A. is a perfect bourgeois fragment. From this interpretation it is possible to conclude, as has Bernard Crick in his article entitled "The Strange Quest for an American Conservatism," (*The Review of Politics*, Vol. 17 [July 1955] 359–376), that contemporary American conservatives must face the " . . . grave difficulties and limitations of deliberately reviving or creating an American conservatism" (p. 361). Kendall forcefully and directly confronts this interpretation by demonstrating that a conservatism is not only deeply embedded in the founding of the American consensus that " . . . sustained the Philadelphia Convention, the *Federalist* and . . . the First Congress," but that it is a continuing conservatism: it is the grain of American political institutions which may have been painted over by those he calls the "liberal revolutionaries" (p. 53).

At hand is an issue which has stimulated an interesting debate among American historians, beginning in 1940. The debate, simply posed, is whether or not colonial politics and society were democratic; that is, whether colonial society, democratic in structure in that virtually all adult males could participate in politics, engendered a political conservatism characterized by *deference* and its concomitants, the separation and the delegation of power. Kendall's contribution to this debate begins with his article, "On the Preservation of Democracy for America" (1939); he writes: There is pretty general agreement, by this time, that the fifty-five gentlemen who assembled at Philadelphia in 1787 to revise The Articles were not much interested in what we in our time call democracy, i.e., " . . . a form of political organization which lodges absolute power in the numerical majority of the adult population" (p. 103). What interested the authors of the Declaration of Independence, as Kendall writes in his last work (1967), was the founding of a nation on certain principles ". . . which we, the American people, grasp at least intuitively and sufficiently understand. *Or* we are no longer clear as to what the principles are. Either the one, or the other; and if the latter, *our peril, which is the loss of our identity*, is great . . ." (p. 74).

It is of significance that in exploring throughout his life the nature of the original American consensus that ". . . sustained the Philadelphia Convention, The *Federalist* and . . . The First Congress" (p. 80), Kendall seldom permits the inquiry to remain at this historical level; rather, he revisits Socrates, re-reads Milton, Locke, Rousseau, or approaches the notion of "pure tolerance" or the "majority principle" issue head-on, in an attempt to link the particulars of the American experience to what have been for Western man the important political problems. Hart's introductory essay, being preoccupied with Kendall's interests in domestic politics, fails to convey the breadth of his scholarship and concerns. For example, Hart quotes Kendall's statement "What I do take side on is the preamble of the constitution which gives equal status to justice and domestic tranquility" (p. 15), without pursuing at all the interesting implications and possible interpretations stimulated by this insightful recognition about the Constitution. The quoted statement recalls Jacques Maritain's observation that the extreme left is willing to deny *being* for the promises of the future, while the extreme right is willing to accept injustice in order to obtain domestic tranquility. Taking Maritain's distinction, and the sense of Hart's passage which includes the above-quoted statement, one might easily identify Kendall as a simple exponent of the extreme right. But this would be a most unfortunate narrowing of Kendall's position which, to elaborate in the context of Maritain's distinction, is more properly put thus: the left is willing to deny *being* for invalid promises of the future, while the right (that is, Kendall's conservatism) holds that essential for the realization of *being* is domestic tranquility, in that it implies a consensus concerning the ordering of symbols which engender social institutions and which can only be brought into question at the expense of identity.

Kendall's intense love and concern for American political and social institutions are reflected throughout his writings. He deserves to be read for his scholarship and his far-ranging discussions of major political theorists and crucial political issues. This is not to imply that Kendall is not flawed in his handling of some of these thinkers and issues: he certainly is. Thus, I do not—as

Jeffrey Hart does—consider Kendall to be "beyond any possibility of challenge, the most important political theorist to have emerged in the twenty-odd years since the end of World War II" (p. 9). Kendall is an interesting American political theorist whose writings have helped to correct the misunderstanding of the roots and nature of American conservatism, (e.g., Kirk's), by placing it within the framework of the founding of the American nation. His last writings, in which he discussed among other things John Courtney Murray's book *We Hold These Truths*, indicates that Kendall's views had developed in the direction of Eric Voegelin's stance articulated in *The New Science of Politics* (Chicago, 1952, p. 189); first, that science, properly understood, is a search for truth, and second, that "it is the American English democracies which most solidly in their institutions represent the truth of the soul." Kendall's strength is that from his first articles in 1939, he recognized the threat posed by the liberal principles of pure tolerance, majority rule, centralization of authority, equality, etc., and by those "ideologies" known as positivism, historicism, relativism, etc., to these democratic institutions and, thereby, to the truths they represent, as well as to the American identity which is so nourished by them.

JOSEPH C. MOULEDOUX
Sir George Williams University

Modernization: Latecomers and Survivors. By Marion J. Levy, Jr., (New York: Basic Books, 1972. Pp. 160. $5.95.)

A few years ago in a book review in a scholarly journal Marion J. Levy unleashed himself, and in a magnificent display of wrath and snarling fury delivered himself of the thesis that the advancement of science calls for an end to gentle civilities and an uncompromising commitment to ruthless, unrelenting, yea even nasty criticism so that only the truth will survive. He became so carried away by his message and the glory of his cause that he completely forgot to make any substantive criticism of the book under review. No doubt the nonplussed author was puzzled as to what he possibly could have done to have provoked such a bluster of affect in the name of affectively neutral science; unless, of course, he knew that Levy was then a fundamentalist preacher of pure methodology.

Now Levy has thrown away that reputation, written a book with barely glancing references to methodology, and allowed his private propensities for wit and novelty to shine in public. Although not abandoning academic battles, Levy announces that he is reaching for the bigger public of intelligent laymen and is addressing himself to more than a scholarly audience. He does break out of the sociologist's tradition of soberly elucidating the obvious in grotesque jargon, and he achieves an impressive degree of wit and wisdom. The new Levy stands somewhere between John Kenneth Galbraith and Parkinson's Law; he does not strive for the macrovision of the tallest economist and his prose is still a bit heavy, although not ponderous, his concerns unduly academic and, sad to say, his jokes at times are too convoluted to put him in the class of *The Peter Principle*.

Levy's title suggests he intends to deal with the problems of the developing countries, and he does treat them briefly, but the burden of his interest is with the industrial societies and what makes the few that have achieved modernity so different from the multitude which have not. He claims to be uncommitted about the worth of development, he is not an enthusiast for all of modernity, and he is wise enough to see the follies of where presumed progress can end. As for where it all leads, Levy exudes a reassuringly Jobian spirit of gloom and agrees with Lord Keynes that "in the long-run we are all dead," but on more immediate matters he adopts an unconventionally optimistic outlook, finding more virtue and sense in middle-class ways than in those of backward peoples.

Levy's presentation is a nice balance between important historic themes and expressions of personal prejudice. His topics range from the problems of education for an unknown future, and the challenge of maintaining rapid social change given the difficulty of achieving even slow change, to the unique need in modern societies to deal with strangers and to operate in exotic organizational contexts, and so on. Other subjects he treats include the trend towards increasing centralization, the impact of recreation on politics, and finally the sexual revolution and the demographic changes likely to result when parents can choose the sex of their babies.

The chapters are brief, the words carefully chosen. The examples are vivid, as when Levy speaks of what is in store when longevity tilts human populations in favor of senility and "we shall see a day in which if senile Chinese are wheeled four abreast past a given point, their procession will never end" (p. 46). The thoughts are at times shockingly novel as when he notes that shortly when thirty million Americans own second houses, each on an average of five acres of land, the area damaged, according to conservationists' standards, will be greater than that destroyed by the combined efforts of the lumber and public utility companies.

Levy's capacity for enthusiasm about and astonishment over the commonplace is breathtaking, as when he makes us think twice about the meaning of modern, impersonal relations and thus professes to see a miracle in our ability to walk into a building we have never been in before

(only a sign says it's a restaurant), approach a complete stranger (only his uniform serves as introduction), and ask him for a hamburger and a milkshake and then, believe it or not, getting from the unknown the completely familiar items we want. This awe of the ordinary might put Levy back with the run-of-the-mill sociologists were it not for the cleverness of his phrases and quotes. For example:

> Homo sapiens is also homo hierarchicus. Actually down through history the achievement of egalitarianism and freedom has been primarily an elitist preoccupation of individuals who had had every reason to believe they would more than hold their own under such dispensations. Our loftiest expressions about freedom have had undertones of "It's every man for himself and God for us all," said the elephant as he danced among the chickens (p. 71).

At times he goes a bit far in his search for cleverness as when he contemptuously dismisses the concept of "Post Industrial State" as a phrase that is "only acronymically apt."

Standards differ, and different readers will arrive at different ratios of clever to too clever by a half. A useful test of a reader's tastes is Levy's Fourth Law, proclaimed while observing that virtuosity in hypocrisy is probably as rare as virtuosity in anything else, and reads:

> Always pray that your opposition be wicked. In wickedness there is a strong strain toward rationality. Therefore, it is always a possibility, in theory, of handling the wicked by outthinking them. Corollary One: good intentions randomize behavior. Subcorollary One: good intentions are far more difficult to cope with than malicious behavior. Corollary Two: if good intentions are combined with stupidity, it is impossible to outthink them. Corollary Three: any discovery is more likely to be exploited by the wicked than applied by the virtuous (p. 36).

This is an interesting book in which Marion J. Levy is at his best, which is when he is not talking about the obligations of science. Discussions of modernization have long been spiced with more pretensions than wit; Levy has raised the level of the latter without lowering the former.

LUCIAN W. PYE
Massachusetts Institute of Technology

Marx's Paris Writings: An Analysis. By John Maguire. With an introduction by David McLellan. (New York: Barnes & Noble, 1973. Pp. xxiv, 170. $9.00.)

Some decades ago, anyone asked to suggest a book by Marx that might serve as an introduction to his philosophy would be very likely to have named a work of Engels, *Anti-Dühring*. Today the choice would much more probably be the *Economic and Philosophical Manuscripts* Marx drafted in 1844 in Paris, to which perhaps a few smaller pieces he wrote during his honeymoon a few months earlier would be added. Since these small pieces were published while Marx was in Paris in 1844, John Maguire has lumped them together with the 1844 manuscripts as Marx's Paris writings; and they are the subject of his book.

The trouble with these essays is that they are written in a style and vocabulary so cryptic and oracular that they cannot be understood readily even by someone familiar with German and must be considered virtually untranslatable into other languages. Nonetheless, there are translations into English, and Mr. Maguire makes use of them, occasionally offering corrections. His book is, in effect, a translation of these translations into a language understandable to the contemporary reader, and by doing this he has rendered an important service. He has made his contribution more interesting by referring, whenever opportune, to different interpretations of these oracular essays, so that his book has become an introduction to contemporary marxological debates.

In his commentary on these essays by Marx, Mr. Maquire begins with an attempt to define their place within the development of Marx's philosophy, a difficult task which he accomplishes ably. Throughout his book, the author demonstrates his awareness of unresolved questions concerning the relationship between the young Marx and the mature Marx, and he presents a useful survey of the major schools of thought concerning these questions.

Maguire's commentary is presented in a convenient order, topic after topic, which nonetheless manages to convey the manner in which Marx's ideas developed. He takes us from Marx's critique of the democratic state to his earliest models of the capitalist economy, his tentative notions of class conflict, and his total rejection of Hegelian philosophy. Finally, he discusses the basic elements in Marx's own philosophy emerging from this rejection of Hegel and ends with a discussion of the Marxian philosophy of man and the blurred vision of a society without alienation.

Throughout these commentaries, he is aware of the many unresolved tensions within Marx's philosophy, especially that between the Hegelian and the Comtean tendencies. He brings this tension into the open most fully in his discussion of the conflict between deterministic notions and the philosophy of *praxis*. While he leans toward a view which assumes the fundamental unity of all Marxian ideas, seeing the *Grundrisse* (which should be translated as "blueprint") as the bridge between the philosophic essays and *Das Kapital*, he does express his awareness that the opposite conclusions could be drawn, i.e., that the gap between the philosophic writings and the economic work is not easily bridged.

The author is a bit more confident in asserting the essential unity of all the works written prior to 1848. But there too it would be possible to argue other interpretations. One could, I think, distinguish the period prior to 1844 from that between 1844 and 1850. The former period predates his meeting with Engels, the latter is that of their close collaboration. The early Paris period, when Marx drafted the *Economic and Philosophic Manuscripts*, serves as the bridge between them. At that time the two friends had not yet met, but Marx had read Engel's essay on political economy. Their major joint work, *The German Ideology*, would then have to be read, not as a continuation of the early Marx, but as a break with him; and indeed it can be so read. The very quotation on p. 108, which according to Maguire demonstrates the continuity, to me demonstrates the break.

The book ends with a teasingly brief but suggestive confrontation of Marxist and Christian stances in the face of the most troublesome questions about human existence. The adjectives I have used here could be applied to the entire book: It is short and at times much too summary. But it is based on impressive knowledge, and every page is thoughtful and stimulating. We must be grateful to the author for his contribution to the debate on Marx's philosophy.

One minor suggestion: the word "*Todesgefahr*" should be translated as "mortal danger;" "danger of death" (p. 4) seems awkward.

ALFRED G. MEYER
The University of Michigan

The Politics of Truth. By Holton P. Odegard. (University: The University of Alabama Press, 1971. Pp. xi, 439. $13.50.)

Attacking the institutions of pluralist, representative democracy has become almost a fad. This book by Holton P. Odegard, a planning consultant, is another such attack—erudite, eloquent, and basic.

According to Odegard, not only have problems become too complex for representative, legislative democracy, but the forms and political patterns associated therewith are based on a now obsolete view of human nature—atomic individualism and separateness and mind-body dualism. Today, says Odegard, we view man as transactional, one component of an ongoing situation. Reality is fashioned from transactions between perceiver and perceived. The Self (always capitalized) originates and grows by transactions with the other and therefore includes the other. Unity is the truth; individual separateness and all its implications and apparati, like bills of rights, constitutions, legislatures, parties, representatives, compromises are the falsehoods.

Odegard repudiates the old dichotomies: idea and referent, fact and value, individual and group, politics and administration, citizen and governor, representative and constituent, authority and subjection, cause and effect, freedom and control.

As people improve their social perception (of the purposes of others), ". . . the Self is made *whole*some with the roundness of experience" (p. 233). Thereby they achieve "Self-control." When they learn to create and integrate public purposes they achieve social control (after the notion that we control nature through physical sciences). These results will be achieved by the development of an experimental science of purpose—moral or political science, the uppermost science.

The unity of science and truth will expose the unity of moral man. Truth cannot contain conflict and so commitment to it will unify society and government. Man will be unified in science. At the same time, the self-correctibility of the scientific way of knowing means that both Self and society are perfectible.

The only organ of government remotely capable of embodying this program, says Odegard, is public administration—"the natural setting for the first investiture of creative bargaining as political science" (p. 213). Administration grows increasingly expert and scientific. It does not deal in compromise but in problem solving. It eschews partisan politics (a hangover of the phony separateness of Lockeian man). It searches out and develops relevant publics (which could turn into real participation when "human beings-doing" acquire the scientific skills of interpersonal observation, communication, and objectifying of purposes). Finally, public administration is dedicated to the public interest.

Within administration "creative bargaining" will be used to create new, higher-level purposes, rather than disappointing half-and-half compromises. It will accomplish this through "scientific mediation." Government will become a series of social experiments, testing hypotheses, self-correcting in the manner of science, and bringing a higher level of truth in its wake. Participation of "citizen scientists" is to "develop purposes—as—hypotheses," not to secure agreement (which does not validate purpose; only truth does that). Government can then justify policy publicly by reasoned argument instead of depending upon discredited persuasion and compromise.

As this process progresses, administration becomes more democratic. When administration achieves perfection in purpose-knowing, creating, and integrating, perfect democracy will have been achieved because man will be governed by truth. Only pathological cases can resist the law of truth. Eventually participation can and will be-

come vicarious. Nothing could represent me as well as truth, itself. Apathy will signal success rather than alienation. We will be democratically subject to "an authority of organized knowing" (p. 194). "The product is organization around inquiry, the politics of truth" (p. 280).

It seems to me that Odegard reifies his wishes into existing techniques. Take "integration": "Integration is a way of developing purposes, creating them out of the raw materials of the situation" (p. 194). Or again, "creative bargaining . . . is . . . a way for merging or dispelling group differences" (p. 215). Another such technique is "scientific mediation." "Creative bargaining becomes scientific mediation when the ideas involved are so formulated that putting them to use will put them to test. Creative bargaining is thereby converted into a scientific discipline" (p. 233). He launders "control" and "power" with "socialized control" and "functionalized power" (p. 281), terms as good as the originals were bad.

He gives an unusually exalted role in history to ideas. The rejected representative democracy he seems to attribute to some mistaken ideas of Descartes and Locke. And the new governmental dispensation begins "with the realities that are beginning to be understood about human nature as apparent first from experiments in the psychology of perception" (p. 357).

From early training in natural science (biochemistry and chemical engineering) he acquired a faith in science that leads him to overemphasize vastly the success or even possibilities of behavioral science (he has graduate degrees in political science). He also thereby acquired the usual optimism about the ability of science to solve social problems. Strangely, at a time when many students of administration regard bureaucracy itself as a social problem, he projects it as the ultimate hope of mankind. Politics as we know it does not appear. That was part of the old, discredited, representative, democratic system.

His style is apocalyptic and suggestive of great profundities that somehow just escape one. For example, on the role of creativity: "Drawing a deep breath *ex nihilo*, creativity steels itself for the boot-strap self-levitation needed to break out of the circle of reflex action and reaction to win new ways of Self-correction leading to the reciprocal system of deliberate experimentation" (p. 270). Because the old language has misstructured reality a new one is needed. He makes new words like "human being-doing," or redefines old ones. Instead of a "hired-hand responsibility . . . there will be one responsibility common to citizen and administrator alike and focusing on scientific method" (p. 304). Or again, "freedom is the product of control . . . the knowing-how, the controls of democratic moral science" (p. 328). The new free speech is "the basic skill of Self-control . . . that permits (frees) him to stop and reflect (listen), to attempt to allow conflicting facts to speak to him" (p. 326).

Odegard set out to create a utopia, defined as "ideal possibility" (p. 357), and it is probably not fair to complain about the absence of empirical data. Nevertheless, it raises a question: Why should I buy his utopia? Nearly all current history contradicts it. If I accept it I must do so by an act of faith—a leap into the unknown.

I am not so disposed.

VICTOR A. THOMPSON
The University of Florida

Karl Marx On Revolution. Arranged and edited, with an introduction and new translations by Saul K. Padover. (New York: McGraw-Hill Company, 1971. Pp. liv, 682. $17.50.)

Karl Marx On Revolution opens a series of thirteen volumes under the collective title *The Karl Marx Library*. Each volume is to be devoted to a general theme and will contain, in full or in extracts, Marx's writings on such topics as religion, philosophy, history, America and Civil War, Britain, freedom of the press, and so forth. The realization of this undertaking comes at an inappropriate moment. Lawrence and Wishart have apparently decided to publish the complete edition of *Marx-Engels Werke* (Berlin: Dietz Verlag, 1958 ff.) in English, and the first volumes are to appear in the near future. This development is bound to reduce the usefulness of Dr. Padover's series on which he must have expended much energy and time-consuming effort.

The volume *On Revolution* is divided into five parts concerned with Marx's revolutionary theory, his examination of communism as a socioeconomic phenomenon and as a political program, his comments and analyses of the revolutionary upheavals in France, Germany and Spain in the 19th century. For Marxian scholars the last part of the volume is of greatest value, since Marx's articles on Spain, published in *New York Daily Tribune* in the 1850s, are not easily accessible. The remaining parts contain very little that is not available in English and important for the understanding of Marx's ideas on revolution. There are, however, some surprising omissions, especially in the first chapter of the book. For instance, only a part of the *Contribution to the Critique of Hegel's Philosophy of Right: Introduction* is included and, despite its universal implications, it is assigned to the chapter on Germany. Similarly, merely a short extract from *The Poverty of Philosophy* is used in the chapter on the theory of revolution; the letter to Annenkov of December 1846 is left out entirely. *Capital* and the Preface to *A Contribution to the Critique of Political Economy*,

which are important sources of Marx's views on revolution in the later period of his life, are ignored altogether. One has the impression that Dr. Padover did not formulate for himself clear-cut criteria for what is relevant to the understanding of Marx's theory of revolution, and there is nothing in his own introduction that would guide his readers on this matter.

The sequences of extracts do not always follow their historical order and, since Marx's views on the nature, *modus operandi*, and temporal span of the revolution (in his sense of the term) changed and continued to change considerably in the period following the defeat of the 1848 upheavals, the disregard of chronology is bound to be confusing, at least in some cases, for inattentive readers or for newcomers to Marx's works. Thus, in the chapter on Germany a few articles published in the *Neue Rheinische Zeitung* in 1848–49 are followed by extracts from the *Critique of the Gotha Program* (1875), and these in turn by some letters to Arnold Ruge of 1843. Marx's ideas on revolution found in his polemical article against Ruge of 1844 (p. 35 ff.), presuppose knowledge of the *Contribution to the Critique of Hegel's Philosophy of Right* (1843), of which only the second half is included much later in the volume. "International Class Conflict" (Dr. Padover's own title given to a speech on Poland and Chartism made in 1847) precedes a fragment from *The German Ideology* (1846), although the former is incomprehensible without the latter. It is true, of course, that we cannot deal with everything at the same time, but this inescapable limitation does not change the fact that readers well versed in Marx's concepts of revolution can do without the book and those who are eager to become familiar with them are given little help.

The preface, "Karl Marx as Revolutionist," does not serve to introduce the comprehensive collection of extracts with their variety of content. The reader needs an explication of Marx's concept of revolution if he is to study the original writings with understanding; no such explication can be found in the introduction. In particular, there is no indication that the young and the old Marx had different views on many matters. For the young Marx, "revolution" meant a single, decisive and violent overthrow, a massive assault or an armed uprising, such as the French Revolution. Its driving force was the material want, poverty and moral wretchedness of the workers, and its goal was the abolition of all forms of exploitation of man by man, as it were, by one stroke. In the early 1850s, Marx realized that this idea was utopian. "Revolution" was no longer a brief trial of strength but an extended, if not permanent, social, economic and political process of change, determined by subjective and objective conditions, which may require the use of force at some stage but also may be accomplished by relatively peaceful means, by a sustained collective campaign. This campaign had to be unending, since the struggle against inhumanity and for man's self-realization has no definite end in that there is no clear-cut division between the kingdom of freedom and the kingdom of necessity.

In addition, the introduction has some other embarrassing shortcomings. Contrary to Dr. Padover's contention, Marx did not use "socialism" and "communism" interchangeably. He disliked the former and clearly preferred the latter. Marx referred to the middle classes as petty-bourgeois but he never confused them with the bourgeoisie. The author's definition of "democracy" is misleading, to say the least, and his discussion of whether Marx was a democrat is simplified in the extreme. *Germany: Revolution and Counter-Revolution* was published in book form in 1896 but under the name of Marx, to whom it was attributed by his daughter Eleanor. Its true authorship was established from Marx-Engels correspondence only later.

Dr. Padover retranslated some of Marx's writings but the reviewer has doubts whether his effort was necessary and the outcome superior to that of his predecessors. Some of his first English translations became available simultaneously in K. Marx's *Early Texts* (Oxford: Blackwell, 1971), and *The Cologne Communist Trial* (London: Lawrence and Wishart, 1971), while others appeared a little later in a more comprehensive and satisfactory form (K. Marx, *Political Writings*, 2 vols. London: Allen Lane, 1973). Dr. Padover's bibliography of Marx's books in English (p. 642) has numerous gaps. In general, his "Bibliography" (pp. 641 ff.) is highly selective, if not parochial, since he restricts himself to the books produced in the United States. It is hard to discover the audience for whom the volume is intended and suitable. Its price is prohibitive as well as excessive in relation to its content.

Z. A. JORDAN
Carleton University, Canada

Hegel. By Raymond Plant. (Bloomington: Indiana University Press, 1973. Pp. 214. $7.95.)

Hegel was the last philosopher to present political theory as an integral part of a general philosophical system. Professor Plant is therefore entirely right in his claim that the *Philosophy of Right* cannot be understood apart from its metaphysical context. As he reminds us, Hegel insisted on the point. In the *Encyclopedia* we are shown how it was meant to fit into the greater whole. Indeed, the first parts of the *Philosophy of Right* are incomprehensible, and are therefore usually ignored, when that work is examined in

isolation. Professor Plant's discussion, on the other hand, especially of these sections, is a model of succinct exposition. Every serious student of Hegel will appreciate the difficulty of the task he has set himself and will admire the lucidity and comprehensiveness of this book. He has explained the most complex of Hegel's ideas and arguments in clear English prose without simplification. That is a very considerable achievement, and this is a very good book.

There are, however, some difficulties raised by the way this book has been constructed. It is organized without any explicit principle of selection. It was evidently Professor Plant's intention to trace Hegel's development from youth to maturity. To be sure, one can agree with him that it is very useful to look at Hegel's juvenalia very carefully. His enduring aims are there: to overcome Christian estrangement from this world, Kantian dualism and Fichte's solipsism, and above all, to find a philosophical equivalent of the communal harmony of the polis. Important as these early essays are, however, one cannot dwell on them to the exclusion of the works of which they form merely the germ. Professor Plant gives us a meticulous account of all Hegel's work up to and through the Jena lectures and then jumps quickly to the *Philosophy of Right*. The result is that we get a fine picture of the origins of Hegel's mature views but not of their development. This would not matter if it did not lead to certain oddities of interpretation. It is, after all, in the *Phenomenology* that Hegel develops fully his whole philosophy of mind and the way science, culture, and religion structure the development of human knowledge. Again it is in the lectures on *Aesthetics* that we can find the best account of his philosophy of history as a teleological development that is not a theory of human progress. It is here also that we can learn what Hegel thought of his own age. It is not even odd that all this should appear in lectures devoted to art, since art and philosophy, unlike the state, are universal achievements of the human mind for Hegel.

One of the consequences of ignoring these two works is to put a strange emphasis on the influences that shaped Hegel's mature mind. We hear a great deal about his debt to Sir James Steuart, whose contribution to his early interest in theories of history has lately been discovered. But Hegel never even mentions him in the *Philosophy of Right*, although he does acknowledge Smith, Ricardo, Say, and above all, Montesquieu. More seriously, Hegel's overwhelming dependence on Aristotle is never mentioned. Indeed Aristotle's name does not even appear in Professor Plant's book. Yet Hegel often reminds us of his debt to *De Anima* especially. It was Aristotle's notion of immanent teleology that was to serve as Hegel's master idea in overcoming Kantian dualism. It was from Aristotle that he got most of his psychology and the whole conception of human development as a self-motivating and rational process leading to rational knowledge. And it was from Aristotle that he learned of a polity in which the "citizen" and the "bourgeois" were one indivisible person. Again we hear nothing here of Hegel's complex argument against Plato. Finally, although Professor Plant gives us a good account of Hegel's early interest in the Greek idea of fate, he does not show how much Hegel drew on Sophoclean tragedy in his later years. Fate was an idea that he pitted against Old Testament and Kantian legalism to develop a view of responsibility and of punishment that aimed at reconciliation rather than at justice. It was also a conception that guided his view of retrospection as the only true knowledge and of history as a tragic drama.

It is also in these works, neglected in this book, that we can discover how dim a view Hegel took of his own "age of prose." For political purposes that view can be foreseen in the passages Hegel devoted to the Roman legal system in the *Phenomenology*. That too was a state of laws, of legal "persons," not of active citizens, and it is entirely like the modern sovereign state. With that in mind, one should perhaps read the *Philosophy of Right* as a defense of political philosophy, of "the rose in the cross," rather than of the institutions of the post-Napoleonic state. Lastly, the *Phenomenology* alone can give us a clear picture of Hegel's view of Christianity and its relation to philosophy and to the State. Professor Plant is excessively worried by the use of Christian imagery, given Hegel's obvious hostility to revealed religion. In the *Phenomenology* we are told quite clearly that philosophy has succeeded in overcoming revealed religion and now fulfils all the spiritual functions that Christianity served in a "barbaric" and merely "imaginative" way. Philosophy is now able to encompass the idea of mankind and of human freedom without reference to a "beyond," because it can now demonstrate that the human spirit is responsible for its own development without any divine agency. Reason and language (logos) set the ends and stages of human development. They deliver us from subjectivity and aimlessness once we recognize both as collective achievements. There is therefore nothing "paradoxical" in Hegel's remark that the state is now our heaven on earth. The state has at last driven its old rival the "Church" and its "beyond" from the cultural arena. We will now learn to make whatever heaven we know on earth. Philosophy has liberated us finally from the "unhappy consciousness," and to that extent it is indeed "the key to community" as Professor Plant says. It is however only a key. The real work

of social transformation must be left to the "world spirit," to history, which philosophy alone can decipher, to be sure, but only retrospectively.

It is one of Professor Plant's many virtues that he does not feel obliged to exonerate Hegel from complicity in the crimes of our own century. He is mercifully above the childish game of identifying "good" and "bad" guys. He might, however, without resort to this sort of nonsense have asked some political questions about Hegel. Why does he identify freedom with knowledge of necessity and not, as most of us do, with striving for the new? How does he reconcile personal moral freedom and historical destiny? And finally, while community and rational authority are no doubt serious public values, what is one to make of them in a world that is ineluctably pluralistic?

In spite of these and other questions that one might put to Professor Plant, his *Hegel* is stimulating and illuminating and deserves a wide readership.

JUDITH N. SHKLAR

Harvard University

Handbook of Organizational Measurement. By James L. Price. (Lexington, Mass.: D.C. Heath and Company, 1972. Pp. 210. $7.95.)

This book is an attempt to present a compendium of measures used in research on complex organizations. It will be useful primarily but not exclusively to investigators planning cross-organizational survey research. Price sets out two purposes—to help improve measurement and to help achieve standardization of measurement. To me, but not to the author (pp. 1, 194), the first purpose is the most important. I agree with Professor Price that measurement in organizational research is crude and poor; in such circumstances it hardly makes sense to try to standardize! But, as Price implies (p. 195), a handy compendium will at least make it easy for investigators to see and, it is hoped, to benefit from, what has been done before.

The author chooses to present measures of the following concepts: absenteeism, administrative staff, alienation, autonomy, centralization, communication, complexity, consensus, coordination, dispersion, distributive justice, effectiveness, formalization, innovation, mechanization, motivation, bases of power, routinization, satisfaction, size, span of control, and succession. Many other concepts might have been included or substituted, and the basis for selecting these particular ones seems to be essentially subjective. The author mentions the criterion of "agreement among researchers" (pp. 7–9), but he does not precisely operationalize it. Price explains why he did not include "consideration" and "initiating structure" (pp. 193–194)—because they do not have conceptual definitions that fit any single concept on his list. One can well imagine the response of the contingency theorists: "They don't have to fit your list; we can supply our own conceptual definitions." Surely, omission on such grounds is unfortunate, and it makes one wonder how many other concepts are left out with similarly shaky justification. To the book's credit, a great many concepts not specifically treated under their own labels are included in the index and are mentioned in connection with one or more synonymous or allied concepts that *are* specifically treated. Such mentions are helpful, but it would have been much more helpful to have bibliographic references to their measurement, in addition. That way, if measures emphasizing a particular slant are not included in the Handbook, at least one would have information on where, if anywhere, to find them. I am thinking of concepts such as conflict, commitment, professionalization, prestige, acculturation, and participativeness. The researcher oriented toward cross-organizational surveys will fare better using this handbook than will those interested in experimental work, simulation, and case studies. Concepts such as organizational slack, aspiration levels, and clarity and operationality of goals, for example, do not seem to be covered.

Another problem the author faced was which measures or scales relevant to each concept to include. Although seven criteria of selection are provided (pp. 5–6), the use, nonuse, and weight given each criterion, and the *ad hoc* introduction of other criteria, seem to have been fairly subjectively and loosely decided. I was aware of many scales that might well have been mentioned but were not. In this case, however, the bibliography provided is excellent. If one's concept of interest is treated, then the bibliography should enable one to find important measures of it that are not reproduced in this volume. Again, measures not heretofore prominent in survey work are underrepresented, e.g., the many measures of centralization or participativeness in experiments, or of effectiveness in program evaluations. Some of the measures included (e.g., of centralization, consensus, innovation) are out of the ordinary and intriguing. One hopes that the Handbook will stimulate further creativity along such paths as well as reinforcing those that are justifiably well trodden.

For each concept treated, Price provides a conceptual definition and general discussion. For each specific measure, he describes the source research and its data collection, details the scoring procedure, discusses validity and reliability, and comments on quality and transferability. On the conceptual definitions, the author simply provides his own, with little defense or justification, and reports whether the measures presented follow it. Further probing, e.g., comparison of alternatives,

would have been preferable, but some attention to conceptual definitions is better than none. On validity, Price primarily reports the research results, the implication being that if the hypotheses were (un)supported, the measures are (in)valid. There is obviously an error here. Examining results for predictive validity is a reasonable start, but only that.

In a concluding chapter, the author recommends the inclusion of instrumentation in all research reports. Further, if eliminated by editors or publishers, the measures should be filed with a general distribution source (e.g., the American Documentation Institute). I agree, for requiring such procedures would not only contribute to better measurement, but would make researchers more thoughtful about the reported meaning and importance of their results.

The book has its ups and downs, one of the latter being unfortunate lapses into elementary-school workbook style, but it definitely is worth consulting by anyone embarking on the instrumentation phase of organizational research, especially surveys.

LAWRENCE B. MOHR
University of Michigan

Political Apocalypse: A Study of Dostoevsky's Grand Inquisitor. By Ellis Sandoz. (Baton Rouge: Louisiana State University Press, 1971. Pp. 263. $8.95.)

This study of Dostoevsky's Legend of the Grand Inquisitor is an extremely solid, closely thought out, vigorously argued and well-written book. Its aim is to prove that Dostoevsky, in his Legend, has "given mythopoetic expression to an experience of transcendence in the Christian mystical tradition. He has set down an apocalypse." Prof. Sandoz has studied, with a good deal of thoroughness, the social-political background of Russian culture in Dostoevsky's time, the Russian tradition of Orthodox theology on which Dostoevsky draws, and the sectarian mysticism for which he had a good deal of sympathy. All these came together in the Legend to create a powerful synthesis that renews the tradition of Biblical Apocalypse.

Professor Sandoz is a student of Eric Voegelin, and draws on Voegelin's thought for the framework of his interpretation of the political significance of the Legend. Voegelin sees modern totalitarian movements (communism and fascism), as well as a good deal of modern culture, as contemporary forms of the old Christian heresy of Gnosticism. The Legend is Dostoevsky's answer to this heresy as embodied in the radical social-political thought of his time. This, as Dostoevsky understood it, called for the nihilistic destruction of the world and its reconstruction under the aegis of naked human reason. In opposition, Dostoevsky affirmed the transcendence of human freedom to any possible order of immanence, and its subservience only to the divine truth of God and Christ apprehended intuitively by faith.

The reading of the Legend given by Prof. Sandoz is a very strenuous intellectual exercise, and one cannot help wondering whether this comparatively brief text, on the whole quite comprehensible, can really contain all the historical, theological, and political meanings that Prof. Sandoz manages to draw out of it. He certainly makes out a very good (if not entirely conclusive) case for answering this question in the affirmative. Best of all, the weight of his commentary does not interfere with his firm grasp of the existential and experiential core of Dostoevsky's text, which is the source of its power for the common reader.

There is no room here to discuss all the issues he raises, or to quarrel with the fine points of his interpretation. His attempt to turn the Grand Inquisitor into John the Baptist does not seem to me persuasive; and he goes astray in equating the Inquisitor too literally with traditional conceptions of the Anti-Christ. To speak of him as judging mankind "from a nauseous self-awareness of personal decrepitude" (p. 158) just does not correspond to the truth of the text. It is part of the greatness of Dostoevsky that he *does* allow the Inquisitor a good deal of moral sublimity in his own right. Also, while Prof. Sandoz honestly raises the problem of Dostoevsky's own gnosticism—the extent to which he himself nourished millenarian hopes about Russia—he disposes of the matter too lightly. What does it really mean to say that while Dostoevsky did "covet political power and even imperial success for Russia," he did not believe in "the use of power and coercion" to achieve the millennium (p. 235)? This is an issue that needs more elaborate discussion than Prof. Sandoz was willing to give it.

Nonetheless, Prof. Sandoz's book has the great merit of taking Dostoevsky seriously in his own terms, and as a great artist who was also an intellect of notable power. His book is a valuable contribution to the unbiased study of an artist whose work, as Prof. Sandoz amply proves, raises the deepest problems about the meaning and the fate of Western culture in the modern era.

JOSEPH FRANK
Princeton University

Computer Simulations of Voting Behavior. By William R. Shaffer. (New York: Oxford University Press, 1972. Pp. 164. $7.50, cloth; $2.95, paper.)

This is not an easy book to review, for seldom has an attack on such an important problem been so mangled and confused as in this volume.

After a fairly straightforward, albeit pedestrian,

discussion of recent voting studies, the body of this book is devoted to a comparison of the voting models of Anthony Downs and the Survey Research Center (SRC). The two models are operationalized and compared using the SRC's 1964 presidential election survey. This is a great idea whose time has certainly come. More and more scholars are beginning systematically to question the SRC model (and the SRC's analyses) for allegedly "downplaying" the role of issues and—implicitly or explicitly—the role of rationality and/or policy evaluation in voting. Since the influence of Downs is apparent in the work of so many of the SRC critics/modifiers, it is about time that the Downsian model was operationalized and subjected to some rigorous testing. While this book has been based on a very good idea, it is not a book that can be built upon. Instead it is one that needs to be done over properly.

In brief, the main branch of Downs's theory requires the calculation of four parameters for each voter: the voter's party differential—the difference the election outcome will make to the voter's utility; the probability that the individual's vote will matter to the outcome and hence to the voter's utility; the utility value to the voter of voting for the long-range value of maintaining elections; and the cost to the voter of the voting act. A voter prefers the candidate who promises to give him the largest expected utility. He casts a vote if the sum of his party differential, multiplied by the probability his vote will matter to the outcome, plus the long-range participation value of his vote are greater than his utility cost of voting.

Professor Shaffer's operationalizations of these parameters, and the way he combines them in his model, are questionable at best. A look at the main branch of his Downsian simulation will suffice to show the flaws in his methods.

For an index of the magnitude of each voter's party differential, Shaffer uses a transformed factor score based on three questions about how closely the election is being followed, how much the voter cares about the outcome, and whether the voter thinks the outcome will make a financial difference to him. The direction of the voter's party differential is determined from the latter question and is simply the party whose victory will benefit the voter the most financially. This procedure implicitly makes a strong and unnecessary assumption, that for all voters the utility associated with personal financial gain is greater than the utility of all other issues that they may be concerned with. Further, Shaffer does not even consider that different voters have different concerns and that no single question (or factor) adequately reflects an index of party differential for all voters, even though the survey contains data which would allow him to do so.

Shaffer's operationalization of the probability that a voter's vote will matter is another transformed factor score, based on the perceived closeness of the election. This may be an appropriate index, but Shaffer appears to enter this index additively rather than multiplicatively in his simulation. It is not certain, however, that he enters this probability parameter improperly because he does not provide enough information about how he processes the data; it would be impossible with only information that the book provides to take the book and the SRC data and replicate his results!

One of the few places where Shaffer is clear enough about his methods to make confident criticism easy is in operationalizing the cost of voting through an index of media usage. Media usage might offer a good lead on how the party differential was calculated, but it tells us nothing about the cost of voting itself. Not surprisingly, Shaffer finds his model more accurate without this parameter, for as he uses it, he is assuming that the more media a voter has consumed, the more likely it is that the voter will abstain.

Even more basic is the absence of discussion of how any of the individual scales were weighted or transformed. When an index of the value of a vote is compared with an index of the cost of the voting act, how is it decided whether the utility of voting is greater than the cost of voting? Does Shaffer assume—as it would seem—that both indices are not only perfectly correlated with utility, but also that both indices are measured on the same scale? Does Shaffer even know he is making assumptions about the weighting of interpersonal as well as intrapersonal utility scales? It would appear he does not. Shaffer's operationalizations are weak and their usage unfollowable, so there is little or no reason to draw anything from his discussion of Downs.

Shaffer's dealings with the SRC model are no more meaningful than his "tests" of Downs. While he has shown himself able to summarize much literature on simulations, he does not seem to understand what he has read. All his tests of the SRC six-component model are trivial. Instead of the SRC method of using least-squares techniques to assign weights to their six parameters, he either assigns weights of 1 to all parameters and calls this result the SRC model, or he assigns 5 parameters weights of 1 and lets the sixth weight range from 0 to 1. In essence, if the sum of a voter's weighted scores is exactly zero he abstains, if positive he votes for one party, if negative for the other. Where did the idea come from that 0 and 1 were natural limits for parameter weights of this type? Apparently Shaffer got this idea from misreading voting simulations where parameters were run from 0 to 1 because the parameters were probabilities and thus had "natural" limits of zero and one. In the SRC case such an assumption is meaningless. Might not

candidate evaluation matter five times as much as domestic policy and three times as much as group benefits? Such a possibility would require more careful thinking about what parameter weights are, and simultaneous parameter testing. Thus, because flow charts and basic data are seldom clearly presented, and because operationalization and testing are sometimes misleading and generally weak, this book provides neither meaningful nor intelligible evaluations of the Downsian or SRC models.

Scholars, like voters, have limited time and money to spend in acquiring information, and, like voters, often tend to go by labels. It is surprising therefore to find a book of such uneven merit under the label of Oxford University Press. I feel obliged to urge readers to look behind Oxford's prestigious label before they purchase this book.

SAMUEL L. POPKIN
University of Texas, Austin

Multivariate Model Building: The Validation of a Search Strategy. By John A. Sohnquist. (Ann Arbor: Institute for Social Research, University of Michigan, 1970. Pp. 244. $5.00.)

Sohnquist has attempted to shine light into the twilight zone between the platonic heights of empirically grounded theory and the more typical depths of idiosyncratic data-grubbing. Asserting that "theory ought to emerge from . . . data . . . " (p. iii), he and his colleagues (much of the work reported on was done collaboratively) have sought to develop a procedure which can be used to "reveal the structure of relationships implicit in a set of data," (p. iii). Dependent variables in this procedure must be either dichotomies or interval-level variables; independent variables must be coded as nominal (or possibly ordinal).

AID (Automatic Interaction Detection), a computer algorithm based on one-way analysis of variance, is to be used to partition the population sequentially into nonoverlapping subgroups such that each split "provides the largest possible reduction in the unexplained sum of squares" (p. 20), with certain constraints which prevent the process from simply generating all possible subgroups. The end product is a tree structure. In the absence of severe skewness in one or more of the independent variables, this tree will be symmetric when the impact of the independent variables can be represented additively; asymmetries in the structure suggest interaction effects.

The subgroups thus generated by the AID analysis are to be used to create new composite variables for each major "interaction" effect. The transformed set of variables is then entered into an MCA (Multiple Classification Analysis) program. MCA is equivalent to a one-zero dummy-variable multiple regression in which coefficients of each class reflect deviations from the grand mean. (Unfortunately there is a typographical error in the defining equation of the MCA Algorithm in equation (1.7), p. 18: n_{12} should be n_{21}.)

The bulk of *MMB* gives a rationale for these recommendations. One chapter comprehensively surveying the literature on "contextual" (i.e., interaction) effects for nominal-level variables (although omitting important work by James Coleman) reveals that sociologists and political scientists have been slow to grasp the proposition that "variables don't always add." The next chapters compare results of AID and other methods for previously analyzed data and evaluate the ability of AID and MCA to "recover," despite superimposed stochastic noise, the actual forms of relationships of artificially generated processes. In an admirable final chapter Sohnquist is honest about possible lack of replicability of the AID partitions, recognizing that the more complex the statistical representation, the more likely it is to be picking up idiosyncratic features of a particular data base (Fudge's Second Law). His discussion of a variety of graphical, tabular and other special techniques for data inspection is excellent, through abbreviated, and even the statistically sophisticated reader can benefit from it.

AID is basically a technique for *describing*, i.e., parsimoniously classifying, a population in terms of its value on a criterion variable. As such it *may* be a useful preliminary to theory building. However, AID tends to use variables which "divide the sample into subgroups of approximately the same size" (p. 131); thus theoretically significant effects may be overlooked for factors rare in the sample, a problem which Sohnquist dismisses far too cavalierly. Also, since the end product of an AID classification is a nominal level variable, AID seems appropriate to theory building only where level of measurement is weak.

MMB sets an extremely high standard for care and candor in data analysis and concern for the appropriate use of statistical techniques, and it is in no way a statistical cookbook. But like the children of Israel worshipping the Golden Calf, some political scientists may mistake the computer for God (or a sausage grinder), and cry out to it indiscriminately for AID so that their articles may multiply and they may flourish. If, however, they read Sohnquist with the care which he deserves, they should at least know better.

BERNARD GROFMAN
SUNY, Stony Brook

Conflict and Harmony in Human Affairs: A Study of Cross-Pressures and Political Behavior. Peter W. Sperlich. (Chicago: Rand McNally & Co., 1971. Pp. 256. $8.95.)

Students of voting behavior and the sociology (and politics) of knowledge will find Peter Sperlich's book interesting and stimulating. On the empirical level, it presents a careful and detailed

development and examination of the hypothesis that politically cross-pressured persons tend to withdraw from political participation. The results of this analysis lead to some modifications of the hypothesis through the identification of conditions under which the withdrawal effect of cross-pressures is heightened or weakened. On the epistemological level, Professor Sperlich discusses why the social sciences tend to view social and psychological conflict in a negative way. He objects to the preconception that conflict is a state that necessarily needs resolution. He believes that the cross-pressure hypothesis emerged from this negative orientation toward conflict and argues for a more balanced view in which the positive contributions of conflict are recognized.

The hypothesis that conflicting attitude elements lead to delay and withdrawal from decision making is rather widely accepted in discussions of voting behavior. Its beginnings in this field may be traced to the landmark volumes, *The People's Choice* and *Voting*, both of which reported that inconsistent demographic and social characteristics (i.e., one or more of which is statistically associated with Democratic voting and the others with Republican voting) led to decreased interest in and attribution of importance to elections, delay in deciding how to vote, and, finally, lowered voting turnout. Psychological conflict was the presumed intervening causal mechanism. Similar results were reported in the later classics, *The Voter Decides* and *The American Voter*, both of which focused more directly on conflict among attitudinal elements themselves. From these auspicious beginnings, the cross-pressure hypothesis became part of the wisdom and folklore of the voting behavior field. Occasional failures to find support for it did not receive much attention nor have much impact on the field.

The data sets Sperlich uses to test the cross-pressure hypothesis are the 1952, 1956, and 1960 election studies of the University of Michigan's Center for Political Studies. The major independent variables measuring attitudinal consistency, centrality and magnitude of cross-pressures are based on attitudes toward the candidates and parties as expressed in openended questions. Reporting both favorable and unfavorable attitudes toward one party or its candidates is considered to be an indication of cross-pressures. The dependent variables are all behavioral: time of voting decision, participation in the campaign and voting itself.

Only time of decision yields results consistent with the cross-pressure hypothesis: cross-pressured individuals decide for whom they will vote later in the campaign than do those whose attitudes are consistent. But contrary to the hypothesis, Sperlich finds that cross-pressured individuals tend to participate *more* than consistent individuals, both in the campaign and on Election Day. Among the cross-pressured, the severity of cross-pressures *is* negatively related to participation, but participation is still lower for consistent persons than even for those most severely cross-pressured.

Sperlich's basic revisions of the "standard model" result from an inquiry into the intellectual roots and assumptions about human nature made by cross-pressure theory. Its roots are identified in the psychological theories generally known as "consistency theories" associated with social psychologists such as Heider, Newcomb, Festinger, and others. From these consistency theories, Sperlich extracts three basic propositions which are the (unexamined) assumptions of the cross-pressure hypothesis. The thrust of these are that (1) inconsistency leads to psychic tension which (2) is uncomfortable or painful; hence, (3) since people always seek comfort and pleasure, they act to decrease tension as, for example, by preventing conflicts from arising or by resolving them. In criticizing these assumptions Sperlich asks, Do tensions always create discomfort? Do people universally seek to avoid pain and pursue pleasure?

To explore these questions, Sperlich reviews psychophysiological theories of pain and pleasure and concludes that tension increases may sometimes be pleasurable and that organisms thrive at optimum rather than minimum levels of tension. From a further review of the literature on hedonist motivation, he proposes that a more complex model of motivation is needed, one in which man's varying "levels of existence" are considered. Needs and motivations differ for organic, mental, and social concerns. The "standard" cross-pressure model is then revised in two ways: (1) The notion of appropriate and inappropriate levels of tension with respect to the tension requirements of an individual is introduced. If the tension requirement is high, an increase in tension will be experienced as pleasurable and will not be avoided. (2) The theory is further revised to include the proposition that "the more dominant in any given situation the aims and motives of ego and conscience, the less will behavior be guided by organismic (id) demands for pleasure and gratification, such as expressed in attempts to resolve all conflicts" (p. 84).

Sperlich's tests of the revised theory lead to the discovery of complex relationships among magnitude of cross-pressures, centrality, psychological integration, social conscience, and political behavior. Among the most important findings buttressing his critique of the pleasure principle is that within levels of magnitude of cross-pressures, citizen duty, caring about the election outcome,

and education all increase participation. These indicators of conscience (duty and caring) and ego strength (education) are interpreted as working contrary to the pleasure principle since they increase participation in the presence of cross-pressures. Thus, the findings support the revised version of the theory which stresses conditions under which people will seek complexity rather than the simplicity of denial or ignorance. "Only when ego and conscience motives were weak did pleasure-oriented withdrawal behaviors manifest themselves" (p. 134). The fact that the highest level of participation is observed among those with low (as opposed to no) cross-pressures (a low cross-pressure state is equated with one of low tension) is also cited to support the revised theory, since Sperlich views participation in this group as a "tension-increasing" response to low magnitude of cross-pressures. He raises but does not resolve the problem of why consistent persons, presumably under even less tension, should not participate even more.

Sperlich argues that his revision of the cross-pressure model basically changes it from one oriented toward simplicity and harmony to one oriented toward complexity and conflict. He believes that the empirical predictive success of the revised model challenges the harmony-oriented "ruling master theory" of the social sciences and calls for a new paradigm which incorporates conflict as well as harmony in its interpretation of human behavior.

In reviewing this particular study, it seems worthwhile to note that among the social scientists being challenged by Sperlich's analysis are those who originally collected the archival data he uses. Sperlich's work may therefore be added to a growing body of studies in the voting behavior literature that help to dispel the fear that reliance on archival data might lead to a homogenization or conformity of research orientations around the paradigms held by those who designed the source studies and contributed the data. Since the voting behavior field has been so dominated by data emanating from Michigan's Center for Political Studies, it is particularly important to note the intellectual challenge and conflict these data can sustain and encourage.

ADA W. FINIFTER
Michigan State University

New Theories of Revolution. By Jack Woddis. (New York: International Publishers, 1972. Pp. 415. $10.00, cloth; $3.65, paper.)

What is the cardinal sin? It is writing off the working class as a nonrevolutionary force. Mr. Woddis, an old Marxist hand at analyses of African and neocolonist politics, provides ample evidence that Frantz Fanon, Régis Debray, and Herbert Marcuse are guilty of this transgression. The question remains whether these theorists are nevertheless accurate in discounting the revolutionary potential of the working class. Woddis thinks not. In support of his opinion he offers a prodigious collection of data, amounting to overkill, affirming the vitality of the working class—and the Communist parties.

The focus of the inquiry is the role of social classes in the revolution and the subsidiary questions of the form and locus of the struggle, peaceful or armed, town or countryside. Since a socialist revolution requires the transfer of power from the capitalist class to the working class and its allies, the working class, as the advancing class, constitutes by objective necessity the revolutionary instrument. By definition, then, or by the canons of scientific Marxism, if you prefer, the theories of Fanon, Debray, and Marcuse provide no guide to revolutionary action and, indeed, may play into the hands of the ruling classes.

The author's critique takes the form of a class analysis of the ideas of Fanon, Debray, and Marcuse as they apply respectively to Africa, Latin America, and the western world. He attacks Fanon's view that in Africa the peasants with an assist from the *lumpenproletariat*, the uprooted in the urban areas, constitute the only revolutionary class, spontaneously ready to turn to violent action. He rejects Fanon's notion of violence as a catharsis through which the masses come to understand social truths. He rejects as well Fanon's dismissal of the working class as a privileged and hence nonrevolutionary classs, offering in rebuttal evidence of its exploitation and degradation. What appears to concern the author most is that Fanon's denigration of the working class impedes the revolutionary process toward socialism and lends " . . . support to all those who wish to foster a stunted form of capitalism in Africa and maintain the working class in a subordinate position" (p. 169).

So, too, the author attacks Debray for ascribing to the peasantry, together with students and intellectuals, the main potential revolutionary role in Latin America. He hits where it hurts by providing a head count of those who took part in the attack on the Moncada barracks and sailed in the "Granma" to show that they were overwhelmingly wage earners and of lower-class origin, granted the organization of the actions by a group of intellectuals. Much of his discussion is devoted to a defense of the Cuban Communists and the working class in opposing Batista, suggesting that the struggles of the Communists were important in preparing the conditions both for Castro's seizure of power and for the revolutionary process after Batista's flight. As Woddis laid it on Fanon for ascribing a central role to vio-

lence, so he also attacks Debray for stressing violence as a necessary experience in transforming oneself into a dedicated revolutionary (p. 398). Woddis is also critical of Debray's concept of the "foco," the isolated guerrilla group which, through expanding military action, grows into a revolutionary vanguard. Che Guevara's misfortune in Bolivia gives the author some ammunition. But the case of Chile is not all that clear; it could be argued that preventive violence against the bourgeoisie was indispensable for securing the Marxist revolutionary process.

Fanon's and Debray's "underestimation" of the working class in developing countries is one thing; Marcuse's characterization of the working class in the western world, particularly in the United States, as a willing collaborator in the *status quo* is quite another. Woddis concedes that Marcuse is right in pointing to the students (and other groups) as constituting a dissident factor, but he argues that they are of mixed class origin, not a social class in the Marxist sense, and serve rather as a "flanking" movement to complement the action of the working class. Conceding also the enormous co-optative magnet of American capitalism, Woddis nonetheless argues that it is ever more necessary to support the revolutionary elements of the working class because there can be no fundamental change in the present system unless it is based in the working-class movement. And there it is. Calling on Lenin, Woddis writes that it is always necessary to have faith in the workers, even when they apparently no longer have faith in themselves. Woddis is no less guilty of the "elitism" with which he charges Marcuse.

What the author's polemic comes down to is that by denying the revolutionary role of the working class, Fanon, Debray and Marcuse are un-Marxist and are objectively playing into the hands of the capitalists. The Marxist, more accurately in Woddis's case, the Marixst-Leninist, premise is that there can be no revolution, conceived as process, without the working class. Theoretically, he has one strong argument: in confrontation with those who control the means of production, no other class or social group in American society disposes of the power comparable to that held by the working class. Yet it is precisely because of their different readings of working-class revolutionary potential that Fanon, Debray, and Marcuse (whose command of Marxist theory will at least match Woddis's) have attempted to develop new revolutionary perspectives. They may be wrong, but at least they treat theory as a creative tool instead of clinging, as Woddis does, to the conventional tenets of Marxism-Leninism in a radically changed world.

BERNARD S. MORRIS
Indiana University

Establishing Ombudsman Offices: Recent Experience in the United States. Transcript of the Ombudsman WORKSHOP, Honolulu, Hawaii, May 5–7, 1971. Edited by Stanley V. Anderson and John E. Moore. (Berkeley: Institute of Governmental Studies, 1972. Pp. 293. $3.00, paper.)

This book is a nearly verbatim report on an ombudsman workshop held in Honolulu in May 1971. It should be of considerable interest not only to incumbents of new ombudsmen offices and ombudsmen scholars but also to political scientists interested in state and local government or the institutionalization process. Verbatim reports of such discussions run the risk of superficiality, repetition, inaccuracy, and irrelevancy. On the other hand, they can have an advantage over other forms of exposition if they allow the reader to eavesdrop on intelligent and careful questioning of knowledgeable political actors. Fortunately, *Establishing Ombudsman Offices* for the most part avoids the defects and enjoys the advantages of a recorded discussion. This happy situation is attributable in part to the calibre of many of the participants and in part to the detailed agendas of the meetings and the tactful guidance given to discussion by Professors Anderson and Moore.

Because the round table took place in Hawaii and because Horman Doi, the Hawaiian ombudsman, was at the time the only incumbent American ombudsman, many of the participants were concerned with the handling of complaints against government administration in Hawaii. Doi's own account of how he dealt with various problems encountered during his first months in office is very interesting, especially since he is generally considered to have been an immense success. Apparently an important reason for that success has been Doi's political acumen in handling administrators and legislators. The discussion of the Hawaiian ombudsman casts light on what objectives Doi set for himself and how he handled jurisdictional, staffing, publicity and record keeping problems. There is a lucid account of how the office investigates complaints and secures redress for persons with justified complaints, an account further enhanced by Doi's excellent flow chart of his operations reproduced in Appendix I. Significantly, most redress is secured through informal discussions with agency heads. Formal written recommendations are very rarely employed, and Doi had not used and never wanted to use his ultimate weapon of publicity to coerce an agency.

It is generally agreed among scholars that ombudsmen are able to operate effectively only when complementing the activities of other complaint-handling organizations. Appropriately, Doi's account of his office is followed by descriptions of the Honolulu Office of Information and Com-

plaint by its director and assistant director and of the University of Hawaii ombudsman by the incumbent. Both of these organizations cooperate with Doi, and all three have more than enough citizen inputs to keep them busy. Indeed there is evidence for believing that the more complaint-handling agencies there are and the more effectively they perform, the more complaints will flow from the public.

Since the cooperation of administrators is necessary for the success of an ombudman, the deputy chief of the Honolulu Police and the director of the State Department of Taxation were questioned closely at the workshop. Their answers and Doi's comments reveal that the Hawaiian ombudsman's tactful handling of government agencies has generally won their full cooperation.

The book's two editors also made significant substantive contributions to the workshop. Stanley V. Anderson, who has done more than any other political scientist to promote American ombudsmen and ombudsman research, presents a useful analysis of the qualifications to look for in appointing ombudsmen and closed the workshop with the suggestion that from the perspective of American constitutional theory the ombudsman perhaps should be seen as a necessary implementation of the First Amendment right of petition. His colleague at Santa Barbara, John E. Moore, who will shortly publish a full account of the Hawaiian ombudman, provides a useful sketch of the array of public and private organizations in Hawaii which complement the activities of the ombudsman.

WILLIAM B. GWYN

Tulane University

Government Publications and Their Use, 2nd revised edition. By Laurence F. Schmeckebier and Roy B. Eastin. (Washington, D.C.: The Brookings Institution, 1969. Pp. viii, 502. $9.95.

Annotated Bibliography of Bibliographies on Selected Government Publications and Supplementary Guides to the Superintendent of Documents Classification System. By Alexander C. Body. (Kalamazoo, Mich., Western Michigan University, 1967. Pp. 181. $2.50, paper. First Supplement, 1968. Pp. 115. $2.50, paper. Second Supplement, 1970, 1970. Pp. 138. $2.50, paper. Third Supplement, 1972, Pp. 143. $2.50, paper. Fourth Supplement, 1974. Pp. 142. $2.50 paper. For sale by the author, Alexander C. Body, 925 Westfall, Kalamazoo, Mich. 49007.)

Guide to U.S. Publications. By John L. Andriot. (McLean, Va.: Documents Index, 1973. 3 volumes. Pp. 1,000. Price not listed).

Library collections of U.S. government documents have been called the despair of researchers. They constitute a labyrinth of publications, enormous in their number, variety of form and content, immense complexity, and lack of uniformity in classification. The richness of these source materials over a publishing history of two hundred years is unbelievable, and most gratifying.

Schmeckebier and Eastin is a reference classic in the field of U.S. government publications, although it also provides limited coverage of state and county material. The new edition annotates the basic guides to government publications—indexes, catalogs, and bibliographies—and provides references to valuable compilations and serials. It explains the complicated system (and sometimes lack thereof) of federal government document classification. In the process it covers the executive, legislative, and judicial branches, constitutions, laws, court decisions, administrative regulations, departmental rulings, presidential papers, foreign affairs, reports on operation, organization and personnel, maps, technical and other departmental publications, periodicals, and microfilm editions of government publications.

Frequently, the book devotes half a dozen or more pages to helping the researcher find his way through the *terra incognita* of old reference works, issued over a period of generations with varying publishing standards, indicating their specific character, scope, and value, or their shortcomings such as incomplete coverage, poor indexes, or interrupted periods of publication. The book greatly helps the researcher to ascertain the usefulness of a particular reference work or government publication for his special needs and guides his way through the maze. Studying pp. 139–43 will save students hours of confusion when they seek their way through the changing pagination and indexing practices of the daily, bi-weekly and session edition of the *Congressional Record*, both before and since 1968.

This book is not always well organized, perhaps because of several revisions since its original publication in 1936, nor is it entirely consistent in its format for listing publications. Its value would be greatly enhanced by a more thorough index. But, on the whole, Schmeckebier and Eastin is very valuable. While it is especially useful and indeed indispensable for advanced work it also offers a good deal to the beginner in government document research.

Dr. Alexander C. Body's work contains: (1) 300 annotated bibliographies, representing more than half a million entries from governmental and nongovernmental sources on the sciences, social sciences, and humanities, published by the U.S. government from May 1963 through June 1967; (2) an alphabetical list of 700 officially acknowledged abbreviations and symbols used by government agencies simultaneously serving as an alphabetical list of government series, circu-

lars, handbooks, manuals, pamphlets, periodicals, reports, and government agencies; (3) a classified list of the current government authors, departments, and agencies—all arranged according to the Superintendent of Documents Classification System. An index of bibliographies, a combination of department-agency, author, title, and subject index complete the compilation.

The *Supplements* follow the pattern of the main bibliography in regard to arrangement, treatment, and format. They include 470 additional exhaustively annotated bibliographies, both retrospective and current, covering several periods between 1958 and 1974.

Dr. Body's work is most useful and welcome, and it is hoped that other *Supplements* will follow.

Volume I of Mr. John L. Andriot's *Guide to U.S. Government Publications* provides annotated references to the publications of government agencies in existence in January 1973, while Volume II covers the publications of abolished agencies and discontinued Superintendent of Documents classes of current agencies. Volume III furnishes an explanation and outline of the SuDocs classification system, as well as agency and title indexes. Additions and corrections will be published in quarterly additions to these basic volumes.

This book is a continuation and expansion of Andriot's *Guide to U.S. Government Serials and Periodicals* published in several editions between 1959 and 1972. It provides an annotated list of some 4,000 U.S. government periodical publications, including current serials, periodicals, releases, reports, field agency publications, and ephemeral material. Subseries are included. Dates of first publication and interruptions in publication are furnished. The earlier volumes are of value in tracing government publications which have been discontinued or have changed their contents. The 1972 volume should be used together with the 1969 and 1970 editions, of 1,683 pages (in three volumes) and 609 pages, respectively.

Both of Andriot's *Guides* are indispensable for advanced research work in the field of U.S. government publications. They are also very useful for beginners.

JOHN BROWN MASON
California State University, Fullerton (Emeritus)

Social Mobility and Voting Behavior. By James Alden Barber, Jr. (Chicago: Rand McNally, 1970. Pp. 280. $5.95.)

The purpose of Professor Barber's book is to examine how the socially mobile behave politically and what the consequences of such behavior are for the stability and flexibility of the political system. Basically, Barber believes that the socially mobile are influenced politically by the socializing experiences connected with both their past and their present status levels in life, causing them to exhibit political behavior somewhere in between that typical of the two levels. This "intermediate response" is supposedly characteristic of both the upwardly and the downwardly mobile and is assumed to serve as a moderating force in politics, decreasing class conflict and class politics in society and thereby allowing democracy the flexibility it needs to function effectively.

Barber's social mobility theory depends on the larger theory of class influences on political behavior. For the mobile to undergo change, the political influences of the various social classes must be different. While Barber seems to take this for granted, he also cautions us that the correlation between class and political behavior may not reflect any serious "class consciousness" or even any political salience of class at all, but instead the possible influence of related third factors. These "third factors" seem to be attitudes and behaviors acquired from conscious class experiences that are themselves later forgotten or deemed unimportant, or attitudes and behaviors inherited from others having such conscious class experiences. One would think, however, that for the crosscurrent influences of the social classes to be effective on the mobile, the influences themselves would have to be more directly tied to class. To maintain Barber's line of reasoning seems partly at odds with his own social mobility theory, for if class itself is not politically salient to the different levels in society, one would hardly expect these levels to be able to affect the mobile politically in the manner he predicts. In my opinion, Barber unconsciously realizes this, relying implicitly in most of his book on the theory of salient class influences on the mobile. From this perspective, Barber sees society as divided into three main classes each exhibiting different political behavior: the business/professional, white-collar, and blue-collar classes. To test his hypothesis that the socially mobile take intermediate positions between the socially stable individuals of these classes, Barber divides the 1952, 1956, and 1960 Michigan SRC election samples into the mobile and nonmobile components for each of these classes by comparing the respondent's occupational level with that which he reported for his father when he, the respondent, was growing up. Chi-square tests and percentage comparisons are then made between the mobile and the nonmobile on a series of political variables.

Barber believes his findings largely corroborate his social mobility theory. A close look at these findings, however, reveals two possible problems in drawing such a conclusion. First, his basic notion of class differences in political behavior is not reflected as a monotonic pattern in the data

as he would have hoped. The white-collar class often registers higher values on political variables than the business class, and, when this occurs, Barber's theory is confounded, since the very groups he intends to use as reference points for the socially mobile are not behaving as predicted. As a consequence of this flaw in the data pattern, the prediction of intermediate behavior for certain mobility groups is often not met. A second problem is that for those mobility groups in which the predictions of "intermediate behavior" are fulfilled, the data findings are usually small; this is because the mobility group in question is usually much nearer to one of its class reference points than to the other, instead of near the middle as a crosscurrent class-influence theory would assume. Of course, the time when people's mobility occurred might be a key factor in determining their exact position between their class of origin and their present status, especially since the political socialization literature provides the underlying rationale for these different class experiences. As plausible as this seems, Barber finds little empirical confirmation for this crucial "time" hypothesis. While not having direct information on when people became mobile, Barber uses "age" as a surrogate measure and finds that the political position of the mobile vis-à-vis the nonmobile changes little across the several age groups.

One naturally wonders why Barber's findings are often weak and sometimes nonexistent. Technical considerations might provide part of the answer—the grossness of the class categories used, the definition of mobility as a comparison between father's and son's status, and the use of recall data to establish that definition. But surely the bulk of the answer goes beyond a discussion of technical problems. The viewpoint of the Michigan team of voting scholars would probably be that class and mobility matter little in the determination of political behavior, at least in modern times. The elections of Barber's study were not dominated by class and mobility factors but rather by Eisenhower's personality, by the "valence" issues associated with his campaigns, and, in 1960, by the overriding factor of religion. As a strange change in position, Barber, on the last page of his book, seems to acknowledge that mobility has little effect statistically in explaining voting behavior, although his earlier contentions with regard to his empirical results leave exactly the opposite impression. As a last point, Barber argues that mobility as a variable is important in the study of political behavior, regardless of what statistical effects it currently has. His reasoning is based on the notion that the simple existence of mobility provides society with both the flexibility and the stability it needs. While one would have to grant the importance of mobility existing in a society, this matter seems academic today. The question of an "open" society was settled early in our history, and there seems little chance that such a condition would be reversed in present times even if it could be. It would seem that class and mobility were both more important and more salient politically in our earlier history than they are today. One can only conclude that Barber concentrated on a variable that has little statistical or substantive importance for contemporary political behavior, and, as a result, the significance of his work for political scientists is not successfully demonstrated.

JERROLD G. RUSK
University of Arizona

Righteous Conquest: Woodrow Wilson and the Evolution of the New Diplomacy. By Sidney Bell. (Port Washington, New York: Kennikat Press, Inc. 1972. Pp. 209. $10.95.)

The re-evaluation of the Wilson Era has called into question the President's role as a diplomatist. Professor Sidney Bell's *Righteous Conquest* asserts that Wilson's foreign policy was primarily aimed at the acquisition of markets in order to enhance the American free enterprise system. Wilson's motivation in most matters has usually been interpreted as an idealistic notion of a democratic world grounded upon international law with the implicit assumption of Anglo-Saxon supremacy. Revisionists like Bell, however, emphasize a pragmatic executive who never wavered from an America-first economic plan despite the fact that local nationalism and world revolution tended to counter his program. To Wilson, international trade was all-essential in maintaining American democracy.

Despite the author's emphasis on Wilson's commercial orientation, the president did not differ in outward appearance from the other post-Appomattox presidents. Bell, however, insists that Wilson's uniqueness lay in blending the idealistic with the pragmatic. Of course, Wilson did not consciously adopt these policies but his interference in the Caribbean countries and across the Atlantic did in essence seek to insure the stability of America's markets. In this strategy, Wilson did not waver substantially from the interventionism of the first Roosevelt and Taft, except that the commercialism was now wrapped in Democratic clothing. The Wilsonian system of collective security was in revisionist eyes another example of forwarding America's commercial power. Thus, for Bell, Wilson's quest for markets was a "righteous conquest" based on international order, Anglo-Saxon détente, and the liberating effect of America's supposed moral leadership.

For all its emphasis on economic matters, Bell's analysis differs only slightly from the work of

others. Selig Adler (*Hispanic American Historical Review*, 20 [May, 1940], 198–226), has pointed out the similarity of American policy south of the border under Wilson to the imperialistic adventures of his Republican predecessors, though Wilson did veto Bryan's attempt to clamp a Platt Amendment upon Nicaragua. Arno T. Mayer (*Politics and Diplomacy of Peacemaking: Containment and Counterrevolution at Versailles, 1918–19* [1967]) noted that Wilson's aims, especially in founding the League, were intended to neutralize the effects of the Bolshevik Revolution, and that there were more practical reasons for his achievements than the moral precepts Wilson was so adept at repeating. Bell fails to refer to N. Gordon Levin's, *Woodrow Wilson and World Politics: America's Response to War and Revolution* (1968)—a serious omission. In more detail than Bell, Levin weighs the economic basis of Wilsonian internationalism and the way in which the Communist Revolution influenced the Princetonian idealist, especially his plans for a world polity.

While Bell's views are often provocative, they actually only buttress the work of the post-Link revisionists—describing how the Wilson *leitmotif* stressed American economic might in order to gain leverage in *Machtpolitik*. The spate of recent writings has revealed that behind the rhetoric of righteousness lay the pedestrian incentive of the dollar.

Bell's sources are adequate (except for omitting Levin's volume). Unfortunately, the notes are located at the conclusion of each chapter and an annotated bibliography would have been useful. Nevertheless, this generally well-researched and well-written work is a welcome addition to our understanding of one of this century's most enigmatic presidents. Bell makes his point and states the case effectively.

MILTON PLESUR
SUNY, Buffalo

Reform and Continuity: The Electoral College, the Convention, and the Party System. By Alexander M. Bickel. (New York: Harper Colophon Books, 1971. Pp. 122. $1.75, paper.)

The Convention Problem: Issues in Reform of Presidential Nominating Procedures. By Judith H. Parris. (Washington, D.C.: The Brookings Institution, 1972. Pp. 194. $5.95.)

The People's President: The Electoral College in American History and the Direct-Vote Alternative. By Neal R. Peirce. (New York: Simon & Schuster, 1968. Pp. 400. $8.95.)

Each of these three books could take as its theme the proposition: "Resolved: any system that three times nominated and twice elected Richard M. Nixon can't be all good." Each author, in his or her own fashion, stakes out a position for liberal reform of that system.

While the theory stated above seems to explain what inspires the writers of this kind of reform literature and is certainly a very good one to account for its present popularity, it will not hold. Each of the books was written before the 1972 national conventions; before Watergate was explored; before the effect of quotas was fully appreciated; and pre-Eagleton and the Agnew of Maryland politics. In fact, Neal Peirce's book on the electoral college system predates the 1968 conventions. Let me begin by briefly summarizing the thrust of each work.

Neal Peirce in *The People's President* has written what he hopes is the basic account of the electoral college system and it may well be so. His audience is those working in Congress and state legislatures who wish to move to a form of direct election. Peirce would provide the arguments and factual ammunition. When he wrote it, he was political editor of *Congressional Quarterly* and this no doubt leads one to expect, look for, and in fact, find those tabulations of statistical detail reminiscent of a well-written sports page. In his relatively long historical section, he is also predictably thorough and painstaking and at points, fascinating. He ends up attempting to state fairly all sides of the electoral college argument and pumps for reform and "a people's president." The book is neither as outdated nor onesided as its mod title suggests. Peirce borrowed it from a *Washington Post* editorial.

The Convention Problem by Judith H. Parris may just as surely be classed as a typical Brookings product. If so, this should be taken as a tribute rather than a dismissal. Its audience is the party pros who in 1972 and after are wrestling with making the rules for presidential nominating conventions. The book is less bulky than Peirce's and more time-bound; in this case to current proposals for convention reform, some of such a minor nature as to be of interest only to the above audience. It is also less historical or statistical than Peirce, but is a good deal more sophisticated in its concern for what political outcomes will follow upon major or minor changes in convention procedure. The author states that the standard by which these procedures should be judged is the extent to which they are "fair and democratic." In actual fact, however, she applies other equally relevant standards since she is never unaware throughout the book that the purpose of a nomination procedure is to find a candidate who has a fair chance of winning, and select him in a way that will not end up tying a cement block around his gullet for the campaign ahead. She also, like all of us, has her own policy preferences which

she hopes the fair and democratic procedures will sustain. Throughout, the tone and most recommendations are conventionally liberal, and are stated in a common-sense and reasonable manner—Walter Cronkite fashion.

Reform and Continuity by Alexander Bickel is a paperback of under a hundred pages of text that deals with reform of the electoral college and the presidential nominating conventions, with concluding remarks on the judicial status of state statutes that discriminate against minor party activity. He aims his work at congressmen with perhaps an eye to the supplementary American government text market. Bickel adopts a familiar and increasingly popular style of today, that of the wise old man of experience who wants liberal goals; but hold it, George, not so fast, remember the wisdom of the founders. This stance was originally pioneered in his news columns by Walter Lippmann, but since Bickel's writing, has been elaborated into the full beauty of a crocheted quilt by Senator Samuel Ervin. Bickel's plea is not to throw out you know whom with you know what.

The presidential nominating procedures are so closely intertwined with the election method that if the electoral college is changed, the convention system will probably also change as well—a point none of the authors, I believe, stresses sufficiently. Since I nevertheless think this is so, I will devote more attention to the electoral college reform and note secondarily how its fate will in turn probably affect the fate of the convention reform.

The standard I think relevant in such discussion is not that of slogans such as "power to the people," but the probable political effects of reform on decision outcomes. The questions to be examined are what kinds of politicians will exert greatest influence, and on whose behalf, if the presidential nominating or election procedures are changed.

There is little debate among these writers or, I trust, any one else out of the Wallace camp, about the desirability of getting rid of electors as independent actors, and the sooner the better. Beyond this, the alternative to the electoral college system is not just straight out popular vote, but popular vote with gimmicks. The most dubious gimmick is that which insists that a candidate must get at least 40 per cent of the vote or face a good old-fashioned runoff in the style of the southern primary or the French presidential system. Bickel is correct, I think, in arguing that this would result in a multiparty system. The 1968 result, in which both major party candidates received in the neighborhood of 43 per cent of the vote, would encourage more minor party candidates. Certainly with the 40 per cent rule, Senator Eugene McCarthy would have been in to force a runoff. It would thus make nomination by a major party convention worth a good deal less than now, and in turn make their procedures more responsive to groups not in the center of the political continuum. After a few such elections, I suspect, we would abandon the convention for a national primary with its runoffs. Let me add that unlike Bickel, I do not see a straight out popular vote without gimmicks producing the multiparty result, and for some of the reasons Peirce states, see less reason to oppose it.

Peirce trots out all the electoral college horror stories, with the examples of how, by shifting X thousand votes in states A, B, and C, candidate Q_1 would be elected instead of Q_2, but with less than a popular majority! The trouble with such arguments is they always contain the implied statement, "other things remaining constant." But if you juggle with Q_1 votes in states A, B, and C, similar votes in the other states will also get jiggled around and now you've created a substantial popular electoral college victory for Q_1; at least that's the way it has worked out, except for that other moth eaten horror story: that tragically significant and politically earth-shaking election held back in 1888 when Benjamin Harrison crept in without a popular mandate and so determined all our fates for the next century or two. (The same result incidentally occurred in the February 1974 three-way election in Britain. Without an electoral college, Wilson, who got fewer popular votes than Heath, became Prime Minister.)

Peirce's best argument, it seems to me, is that while the present system gives certain liberal minorities advantages, there is no guarantee that this outcome will remain unchanged. A few elections from now, the hard hats may be the big urban swing vote, or some other group that we professors may not approve of. Defending the electoral college on the basis that we like the way it discriminates now is as risky as defending the Supreme Court because you like its present policy stands.

The most profound effect of changing the electoral college is not sufficiently emphasized in these books—i.e., that it will encourage a further nationalizing of American politics. This trend is already well begun. Under reform, you will be able to recruit your presidential candidates from South Dakota and Arizona without apology. State politicians in national conventions from Daley on, up or down, will lose even more influence in the selection process, as they already have in those states with presidential primaries. But also they will then lose influence in the election process (no more the scare stories built on how Hiram Johnson of California was snubbed by

Charles E. Hughes and got Wilson elected in 1916). The election campaign will also change. Parties will be motivated to get out every potential supporter wherever he, or she, or it, might be holed up. Inevitably, a national registration and election system free of suspicion of fraud will become an absolute necessity. No more state rigging of the countdowns. Again it seems obvious that these changes would be reflected in the nomination process.

None of the books discusses who the new political kingmakers are or will be under a more nationalized system. These national political groupings are already half formed in response to the present need to win presidential primaries, and are presently only partially restrained and frustrated by local politicos. Now they will come to center stage. Their advisors are apparent and are already active in party councils; they include personnel from the large universities and polling groups but yes, also from such research organizations as Brookings, and *Congressional Quarterly*.

But a most important effect of a straight out popular vote system will be an increased influence of the national media. Only one of the authors, Judith Parris, deals with this issue. Very perceptively she points out the conflict in goals between politicians who want to win elections, and media types who want to keep up viewer and reader ratings. She outlines the degree to which TV needs have already reshaped national convention procedures, noting such small but significant points as that the media demand space for equipment even though it blocks the view of convention participants, resisting the suggestion that they use the slightly less effective but serviceable telephoto lens developed for events which will not permit camera platforms on the scrimmage line and in the huddles. The media seek drama and human interest, i.e., entertainment as on any other prime time program. In the exact sense of the words, media men and women are troublemakers for the politicians, since they profit by encouraging conflict and excitement and playing hearts and flowers at odd moments. The state politician has already been banished from the rostrum as too boring for David Brinkley to attend to. Far better to interview the delegate running for vice president in the hotel lobby, with one-half of one convention vote pledged to him on the third ballot. The political prejudices of the media also will be more significant to the outcome. At present they are clearly liberal, since this is conventionally chic, but that this posture will continue given the tremendous investment in facilities for media operations seems to me to be questionable at best. For example, none of the media or, incidentally, none of the authors in the books reviewed who wrote after the event, hammered home the point that to have the chairman of the committee who reforms the delegate selection process at the same time be a major presidential candidate benefiting from those changes, is a hell of a way to get the appearance of "fair and democratic procedures."

It is hardly necessary to go on cataloguing the effect of media needs and concerns on campaigns and candidates—the-look-what-they-did-to-McGovern or how-about-the-Nixon-Kennedy-debates kind of argument. What this example suggests is that people who write books favoring reform should as much as they can spell out all of the political effects on major party groupings of what they suggest or oppose rather than giving us for comfort "the cure for the ills of democracy is more democracy." Bickel comes closest to doing so, though for my taste he is too hung up on preserving the present balance of political forces as if it will never change. Judith Parris makes incisive comments when she shakes free of the pure democracy kick. But none of these books have fully treated such problems. A significant issue overlooked is the desirability of accelerating or slowing down the effect of replacing state-based veto groups with nationally-based veto and control groups.

CHARLES PRESS
Michigan State University

Social Research and the Desegregation of the U.S. Army. Edited by Leo Bogart. (Chicago: Markham Publishing Company, 1969. Pp. 393. $12.00.)

Desegregation of the U.S. Armed Forces: Fighting on Two Fronts, 1939–1953. By Richard M. Dalfiume. (Columbia: University of Missouri Press, 1969. Pp. 252. $6.00.)

Integration of the Negro in the U.S. Armed Forces. By Richard J. Stillman, II. (New York: Praeger Publishers, 1968. Pp. 167. $10.00.)

The Armed Services and Society. Edited by J. N. Wolfe and John Erickson. (Chicago: Aldine Publishing Company, 1970. Pp. 170. $8.95.)

If much remains to be done to accomplish complete racial integration in our society today, the American military experience with racial integration offers some encouragement and not a little basis for pride. No doubt the military would be the first to admit that it is far from reaching the ideal of racial integration and that its record has perhaps not been any better than that of many other institutions in American life in this regard, but the military can also say that the armed forces are more integrated than any other institution today. The studies by Dalfiume, Stillman, and

Bogart et al. provide us with an opportunity to examine this phenomenon and to determine whether or not any instruction can be gained for future actions. The collection of essays from the annual University of Edinburgh Seminar edited by Wolfe and Erickson presents several other ways to view the interaction between the armed services and society primarily in the United Kingdom.

How far we have come and how far we have yet to go on the road to complete racial integration are illustrated in the three books which focus on that problem in the American armed services. Dalfiume gives us the detailed historical treatment of what he calls the "war on two fronts" (foreign enemies overseas and segregation at home). The paradoxes are richly carved (fighting a war for democratic principles with a segregated military force against an authoritarian enemy with a master race ideology) but he does not go beyond telling the story as an historian. Stillman does move toward an appreciation of the political forces, and a little further into the present era, that brought about racial integration in the American armed services during the late 'forties and early 'fifties but he falls short of a thoroughgoing political critique of the process of racial integration. Although he faults historians with too single-minded a purpose in analysis, he relies too heavily on a straightforward historical account of the process of change in military policy and attitudes toward racial integration to satisfy his goal of an exclusively political analysis of the policy decisions.

Bogart and his associates expand on the role social scientists can and did play in providing data in very short order and under a highly pressured deadline for major governmental decisions on this matter. During the Korean War Bogart and his team set out on official assignment from the Pentagon to determine how the military felt about integration in the armed services in Korea and how it was working where it was tried. The evidence from Korea and, shortly thereafter in 1951, from selected posts within the United States showed that the Army's standard of effective manpower utilization could be improved while meeting Negro aspirations for racial equality through integration of the Army. By contrast, and in a more diffuse manner, the Edinburgh series of papers suggests analytical insights mostly in the British context about (1) the armed services as a reflection of the society served, (2) the criteria for determining defense needs, (3) the relationships between successful military training and the individual application of that training to civilian jobs after military service, and (4) the question of whether or not the armed services constitute an embodiment of support for the aspirations of society.

Both Dalfiume and Stillman help us to realize how much the official policy and attitudes toward the practice of racial segregation can change in one institution over the course of a single generation. The strength of Dalfiume's study lies in his detailed description of how the change came about in America's military forces. Today it might be hard to imagine that an Army Chief of Staff could be opposed to racial equality as official policy, yet we find General George C. Marshall upholding quotas for Negroes in the Army, segregated units, and a separate-but-equal principle which almost always meant discrimination against Negroes. (Not all Negro servicemen disagreed with General Marshall, since, according to Dalfiume [p. 47] some black career soldiers felt their stature would be diminished if they were to compete on ability alone in an integrated situation.) If Dalfiume does not make entirely explicit the reasons for the change in military policy on integration within the American armed services, his chronicle of events does give the reader an adequate basis for making his own judgments. One of his major themes which agrees with a position taken by Stillman is that equal opportunity for Negroes in the military closely parallels the critical needs of the military for more manpower (the greater the need for personnel, the more the concessions made by the military). Dalfiume makes us wonder why we did not see the black militancy of the 'fifties and 'sixties coming, since he shows that Negro leadership in society urging integration in the armed forces in World War II was quite different from the "don't-rock-the-boat" rationale of Negro thinking in World War I.

Dalfiume is more optimistic about the effect of the actions of the armed services as a forerunner of action in the larger social context. On this point Stillman provides the most useful insight into the future but one which is subject to question. His feeling that the Negro could fight on the battlefield but not in the political arena offers a rationale for changes in official policy through the early 'fifties, but his pessimistic estimate of the future capability of blacks to press for thorough implementation of integration policies is subject to possible revision when the achievements of the civil rights movement are examined. His prescription for continued political action to gain full implementation of official policy for Negro rights and opportunities is tempered by his appraisal that priorities are being shifted away from this area of public policy. His excellent annotated bibliography is especially helpful for those seeking a variety of sources on such issues among others as the continuing struggle for off-base enforcement of racial equality for armed services personnel in civilian communities.

For those who wish to see how social scientists gathered and interpreted data on efforts to end racial segregation in the U.S. Army in what became a turning point for policy makers who sought a sound basis for reaching decisions, the work of Bogart and his colleagues during the Korean War is most enlightening. Although much is to be gained from a review of their research design and data-gathering techniques, perhaps the most fascinating aspect of the work is the open-ended comments of respondents in the direct, earthy language of the GI in combat. From a humanitarian point of view one can be encouraged by these personal accounts. Military experiences frequently make a lasting impression on those who share them and the testimony in this book makes one feel that the willingness to try can overcome long-standing racial prejudices. Without doubt the study succeeded in proving to the military that racial integration in the armed services gives great assistance in solving manpower problems once the policy is clearly established and understood.

The attempt by Bogart and his associates to get finally the results of the study published represents an intriguing and important sidelight. For eighteen years the military placed roadblocks in the way of efforts to make the data available to the general public. Bogart raises the question of how much influence the data would have exerted on such movements as the drive to repeal the separate-but-equal doctrine before the Supreme Court. At least now we can understand the possibilities such evidence offers for changing attitudes and opinions about racial integration.

It is difficult to weave the ideas of the University of Edinburgh volume edited by Wolfe and Erickson into a review of the works of Dalfiume, Stillman and Bogart because the issues discussed are more far-ranging and particularly applicable to the United Kingdom's defense posture and its relationship to British society. The focus is placed on such topics as a general appraisal of British defense strategy and posture for the 1970s (W. Correlli Barnett), world power and population (Colin Clark), problems of alienation (Philip Abrams), social integration in the Deutsche Bundeswehr (R. Warnke), the impact of services on society (Sir James Dunnett), U.S. Navy recruiting programs (Franklin Fisher), management and manpower (Air Marshal Sir Andrew Humphrey), and finally the scope and pressures for change within United Kingdom institutional arrangements for defense (D. Greenwood). Contributions by discussants at the end of each essay do point up discrepancies and differences of opinion quite clearly. In a way many problems discussed in this volume from a British perspective may forecast what is in store for American military pioneers as they contemplate more restrictive roles for the American armed forces in the future international environment.

WILLIAM F. LEVANTROSSER
Hofstra University

The Conscience of the Corporations: Business and Urban Affairs, 1967–1970. By Jules Cohn. (Baltimore: The Johns Hopkins Press, 1971. Pp. 122. $6.00, cloth; $1.95, paper.)

Whatever happened to the corporations' attack on urban problems? Professor Cohn knows. In this concise treatise he presents a thoughtful analysis and assessment of their participation during the period 1967–1970.

Cohn starts out fast. No messing around with abstract principles. He wants to find out the extent and nature of involvement of corporations in urban problem areas and the reasons for their success or failure. After citing four factors which he believes account for the declining interest of corporation executives—diminished government and public pressure, unexpected economic costs, unexpected skill and expertise requirements, and unanticipated, "sometimes unwelcome" effects on corporations—Cohn lists the motives that executives claim got them involved in the first place. These include "conscience" (the meaning of which remains unclear), enlightened self-interest, public appearance, compliance with government requirements, insurance against destructive tactics by minority group members, and the hope for new markets.

The major types of programs of the corporations that Cohn studied were donations, minority-group hiring, programs for the disadvantaged, and "other." Each type of involvement receives a chapter-length analysis and discussion. The author then presents a case analysis of one corporation's project in employment and training. The book ends with conclusions, followed by an appendix on "guidelines for initiating subsidiary training programs."

Cohn's analysis is based upon survey data which were drawn from 247 of the nation's largest corporations. We learn that corporation donations are the dominant form of aid. Such aid, of course, avoids the problems associated with more active involvement. Yet, the author points out, the gifts have been meagre by any accounting. In 1950, a mere .59 per cent of net profits before taxes were donated to charitable causes, and in 1967 the figure was still only 1 per cent. Unfortunately, Cohn does not give us any figures for the period of his study.

The author does state that the pattern of giving changed slightly. One-third of his survey respond-

ents reported that they had shifted to include organizations more directly involved in urban problems. The proportion given to such established groups as the Salvation Army, the Red Cross, and Community Chest/United Fund (now known as the United Way) was subsequently less than that prior to 1967. Still, the gifts tended to flow to the more conservative new groups.

As for corporate minority employment practices and training programs, Cohn notes that these have been adversely affected by favoratism toward youth, recruitment at entry level jobs with restricted channels of mobility, credential requirements (especially educational achievement), and religious, ethnic, and sexual discrimination at middle- and higher-level jobs. On this last point Cohn points out the exclusionary tactics still employed against Jews at the executive levels. The reader may wonder why this excursus was included, given the seemingly greater magnitude of the problems with respect to nonwhite minority groups.

One-third of the companies in the survey conducted training programs for the disadvantaged. But Cohn believes that these companies had an exaggerated belief in the thesis of cultural differences. As a result, they directed too much attention toward programs designed to smooth over personality differences rather than concentrating upon outright job training. The author points out that it is not as difficult to motivate and train the disadvantaged as many people think; indeed, their employment retention rates are not markedly inferior to those of other new employees.

Cohn briefly discusses other programs and then considers the effects of involvement upon the Corporations themselves. We learn from his discussion that executives always keep in mind the returns to be gained from their corporation's participation and that individuals tend to see their involvement as one means to move up the corporate hierarchy. In his concluding chapter, Cohn makes the depressing observation that conditions are not improving. In Watts, for example, unemployment was 10.2 per cent at the time of the riots in 1965. In 1969 the rate had climbed to 16.2 per cent. Further, constraints upon continuing efforts have been imposed by political snarls, declining interest of governments, and cost limitations. But the principal thesis in this chapter is that business executives simply lacked political and sociological savvy in dealing with the disadvantaged.

For those who wish to find out what has happened to the highly vaunted involvement of corporations in solving urban problems, this book provides a short, no-nonsense account. It is a balanced presentation; but in the author's short editorials, which are interspersed throughout his presentation, there are hints of a stronger passion.

JOSEPH SMUCKER
Sir George Williams University

The Politics of Belleville: A Profile of the Civil Community. By Daniel J. Elazar. (Philadelphia, Pa.: Temple University Press, 1971. Pp. 165. $8.50.)

Professor Elazar's brief study of Belleville, Illinois, a small city of 40,000 people within the metropolitan ambit of St. Louis, is an extended footnote to his larger study, *The Cities of the Prairie* (Basic Books, New York, 1970). The themes and the principles of selection and interpretation are set out more amply in the larger work. While it is a self-contained account, anyone with a serious interest in the subject matter and approach should read the longer work as well.

Dr. Elazar's most original contribution in these two works, as in his earlier studies of U.S. federalism, is to bring together contemporary descriptive materials, a conception of stages in the historical development of local, state and federal political institutions, and a conception of political culture which is derived from migration patterns and the materials of social and intellectual history. In this study, as in *The Cities of the Prairie* a series of developmental stages are asserted, marked by particular combinations of migration, cultural patterns, and social and economic development, of which the "metropolitan frontier" is the most recent and the most contemporary. The particular politics of a locality is interpreted as a function of its developmental stage and the contribution of its "migration streams" to its particular "political culture." These matters are set out with some concreteness in terms of population trends, economic trends, the development of religious, intellectual, and educational institutions, city services, and the character of local party divisions and contests for local office.

The result is a persuasive account, especially to someone who has worked with similar materials and who has had a direct exposure to the political personalities and practices of similar places. Since the evidence presented is highly selective and often very summary it cannot be said that the conclusions are established beyond doubt. The data the author uses are drawn from standard sources, such as the census, election returns, published works and unpublished manuscripts, the study of the local press, and substantial interviews with office holders and community leaders. Essentially the selection and combination of these data are highly personal, the plausibility of the result rests on whatever confidence the narrative inspires and

its congruence with the reader's own experience. This reviewer is willing to believe that Dr. Elazar's account of Belleville is both reliable and predictive of the state of affairs in other local communities which have similar histories and similar social and economic characteristics. Given Dr. Elazar's assumption that independent cities of intermediate size which are not functionally specific segments of a metropolitan complex, are distinctive entitites, and that a substantial part of the population of the United States lives in such cities, the study is of a general interest.

A few difficulties in the structure of the work should be noted. The brief account of "migration streams," which is presented primarily in the form of a chart, misrepresents the more careful definitions provided in the *Cities of the Prairie.* Migration streams are recognized as originating in the three major divisions of the East Coast (New England, Middle, and South) and from Europe and Latin-America. For each, certain orientations which constitute elements of "political culture" are asserted. In the *Politics of Belleville* the basis for these attributions is certainly not provided. Indeed a series of European migrant streams are condensed into the Southern Stream, though all they seem to have in common is a predominant attachement to the Democratic party. It is not at all clear to me that migrants from New England were more community oriented and those from the Middle States more "individualistic." There are very visible differences resulting from migration patterns cited for Belleville, and to be found universally, in the particular denominational churches founded, and in the socioeconomic attributes of congregation members. That these are correlated with a particular political outlook is not clear, at least between the Presbyterian and Congregational denominations. The general conclusion about Belleville, that a rather small area is recognized as public or governmental, and a rather large one as private or business, is applicable to all Illinios cities known to this reviewer.

Another conception which needs greater explication is the idea of a "civil community" as the appropriate unit for local political studies, as opposed to the more narrow legal structure and area of the city government. That city politics cannot be treated as a set of variables arising only within the formal structure and area of the city is far from a new discovery. That city policies are more or less coordinated with those of major nongovernmental institutions and other special and general governments having jurisdiction in the city boundaries is not surprising either; that such a coordination, whether deliberate and planned or adaptive defines a "community" is another matter.

The most striking concrete political conclusion is that Belleville is governed by a "representative oligarchy" of three elements—labor, business and politics—political office-holding being a specialized and independent occupation. To each of these elements Elazar attributes the capacity for adjustment to change and for accepting new participants. The notable exception he sees is the small group of blacks, since black settlement is discouraged in Belleville as it is in the other East Side cities which have managed to preserve predominantly white populations. The processes of slate making and the isolation of municipal contests from the party-based contests for county, state, and national office are well described. The roots of these arrangements are ascribed to the assembling of the several migration streams which shaped the city political culture. An account is given of interrelationship with other governmental levels, particularly the management of state and federal resources to support developmental projects of importance to the city.

In short, *The Politics of Belleville* is a satisfactory account, problems of proof aside, and the general scheme of inquiry and description is applicable in principle to other cities, as it is intended to be. The general tone of this, as of Elazar's other works on federalism and city politics is optimistic, the optimism of what so many younger students of politics consider to be "establishment political science." Elazar also provides the material for quite contrary assessment; he makes it obvious that Belleville has a mixed record with respect to both ideals and accomplishment, even if he chooses to stress its more optimistic aspects. Elazar's account of Belleville is of particular interest to students of local government and politics since it demonstrates the ability of a small city to maintain its independent political style and preference despite its position in a metropolitan area presumably dominated by a large central city. It should be of interest also to those who would like to see the current trend to cross sectional quantitative studies balanced by longitudinal studies which search for developmental factors shaped by shifting interrelationships of social and political processes.

PHILLIP MONYPENNY
University of Illinois, Urbana

The Water Lords. By James M. Fallows. (New York: Grossman Publishers, 1971. Pp. 294. $7.95.)

This volume is one of a series of studies produced by members of the staff of Ralph Nader's Center for Study of Responsive Law. It is primarily a study of the pollution problem of the Savannah River in and around Savannah,

Georgia, although it branches into the problems of air pollution and land-use planning as well. It is a detailed, thoroughly researched presentation, bristling with hard data as well as forthright opinions about who are the bad guys, the weak guys, and the good guys in dealing with these complex environmental problems. It is, therefore, in the best Nader style and approach and contrasts with some Nader Center efforts that have been less perspicacious and simply more opinionated.

Fallows, an A.B. in history and literature from Harvard, and his associates lay out in detail a picture of general disregard for the vital water resource by nearly everybody during most of the twentieth century. Water in the volumes coming down the Savannah and in the aquifer near the seacoast was viewed as limitless. It is also a picture of both corporate and public negligence in destroying the water resource and corporate efforts—usually successful—to use its political muscle to coerce or buy local political leaders into accepting their waste contributions to the river as part of their more general contribution to the community.

The record appears to demonstrate conclusively once again the capability of industry and cities to resist measures designed to rectify pollution. Dates for achieving certain goals for cleaning up the river came and went with only modest and clearly inadequate steps taken to meet those goals. Only the Georgia Water Board appears to have had the dedication and the perseverance to make any serious impact on the polluters—including the city of Savannah.

But there is a time dimension to the problem that unfortunately is frequently lost in the beneficial muckraking of the Nader movement. This book concludes its story in 1970 with little positive comment about the efforts to clean up the air and the river. In a letter to me Mr. R. S. Howard, Jr., Director of the (now) Environmental Protection Division of the Georgia Department of Natural Resources, states, "The book is a reasonably accurate picture of the situation (political and Savannah River pollution) as it was in *1965* (emphasis added) when this Agency became involved in cleaning up interstate pollution of the Savannah River Estuary." He adds, however, that "at publication time considerable progress had been made in the planning, designing and construction process of the interstate program. I was somewhat disappointed that 'the study' failed to report this."

Illustrative of this time dimension problem is the record of one of the worst malefactors in the Fallows account: Union Camp, the largest Kraft pulp and paper mill in the world. Fallows reports a daily average biological oxygen demand reading of 123,000 pounds for approximately one year from August 1969 to July 1970 (p. 86). By February 1973 the average was 20,300 pounds with the completion of a secondary treatment clarifier and aeration pond. Others such as Continental Can and Savannah Sugar Refinery show a similar improvement. On the other hand, one egregious example of failure to gain corrective action concerns American Cyanamid, which continues to contribute 30,000 pounds/day BOD despite enforcement conference decisions calling for substantial reduction. The situation is now in litigation. It is interesting also that Union Camp recovers 95 per cent of the chemicals formerly discharged into the river. Thus the Environment Protection Division can report that "although the Savannah River is not yet clean and clear, there are data and obvious signs that its quality is improving."

It may well be that the Nader team report helped precipitate this change. But it is unfortunate that the story ends with so negative an impression that Howard reports he continues to receive mail "from all over the U.S.A. demanding that we clean up the Savannah River." Those who might use this report as evidence about the responsiveness of the political system or as textbook material would do well to look a little further so as to report not only the horrors but also the successes.

The study concludes with a useful discussion of alternative remedies for dealing with the pollution problem. Unfortunately the treatment of these remedies is fairly abstract with little discussion of their specific application to the Savannah problems. But the author clearly accepts the view that a multiple approach is necessary, rejecting simplistic solutions for a complex set of problems.

I am struck by one other feature of this book: the audacity of the researchers themselves. It is clear that they went to incredible efforts to get their data, including embarrassing people involved, doing technical field research, and imaginatively probing public records. This approach is perhaps more in the tradition of investigative reporting but one wonders what the dividing line is between scholarship and such reporting. How pallid and lifeless and even meaningless some of our research is because we simply lack the guts, imagination, and technical knowledge to obtain the necessary data. Probably all too often we avoid such things as corruption because we might offend our sources or because the job of gathering the data is too difficult. Applause for the Nader team for determined bird-dogging that turns into scholarship!

DEAN E. MANN
University of California, Santa Barbara

The Revival of American Socialism: Selected Papers of the Socialist Scholars Conference. Edited by George Fischer. (New York and London: Oxford University Press, 1971. Pp. xiv, 330. $10.50.)

Socialism in America: From the Shakers to the Third International. Edited by Albert Fried. (New York: Doubleday & Company, 1970. Pp. xi, 580. $3.95.)

Professor Fried explains that his comprehensive collection of documents on the history of American Socialism ends with the creation of the Third International (1919) because since then there has been no important movement. The "New Left," he says in particular, is either not intellectually important ("It is still only a mood") or it is not historic socialism ("It is indifferent to the issues [reform vs. revolution, the nature of the Soviet Union, the viability of capitalism, the organization of the working classes, etc.] that meant so much to previous generations of radicals.") Furthermore, the New Left "favors forms of socialism that are at once extremely libertarian and extremely authoritarian" (pp. 15–16, 520). The New Left, then, is a kind of socialism but as yet inchoate contradictory socialism.

Fried's collection includes religious socialists, secular communitarians, "political" socialists (Debs, London, Sinclair) and quasi socialists. The latter includes the famous exchange between Henry George and Serge Schevitch over the "Single Tax." There are in all some fifty selections, most of which have not been reprinted before. They deal to some extent with dated issues of interest only to historians (e.g., the relation between American socialism and organized labor) and with the petty internecine squabbles to which the whole socialist movement was historically prone (e.g., whether Oceania or Kaweah colony is the true socialist order). There is also some discussion of permanent socialist principles which are, however, more authoritatively available in European writers. From this collection, it seems that this old American Socialism is of interest primarily to antiquarians.

Fischer's proffered alternative to Fried's historical debris is a recent socialist revival which he identifies with the New Left. The book consists of papers from the Socialist Scholars Conference which met from 1965–1970 and is now defunct. The papers deal with those issues to which Fried says the New Left is indifferent.

Fischer's book is divided into three parts entitled *New Paths to Socialism*, *Late Capitalism*, and *Neo-Marxism*. Of the three articles in Part I, John Cammett's study of Gramsci's "participatory democracy" and James O'Connor's attribution of causal connections between bourgeois capitalist values and the problems of our time, represent neither new nor persuasive analysis. Martin Nicolaus develops one of the two significant questions of the book, namely, whether the proletariat in advanced industrial societies is revolutionary, actually or potentially. He claims it is neither and is instead "parasitic" towards "underdeveloped" countries, having been bought off by capitalism's shrewd shift of exploitation to nonindustrialized proletariats. Hence, a rising living standard and a rising rate of exploitation have been made mutually supportive. This useful insight explains one of the major embarrassments of Marxist theory in a dialectical manner, that is, in a manner compatible with that theory.

In Part II Paul Sweezy accepts the Nicolaus diagnosis but salvages the theory of the revolutionary proletariat by the consideration that it exists outside the industrialized countries. The plausibility of the Nicolaus-Sweezy view rests on the assertion that capitalist countries "raise further the standard of living of their working classes, [and] bring poverty and misery to the great mass of working people . . . in the dependencies" (p. 167). Demonstrating this assertion would require showing that "poverty and misery" have increased as a consequence of the intrusion of advanced capitalist economies. Sweezy-Nicolaus omit this demonstration, implying that their audience is composed not of potential but of actual believers. For merely potential believers the capitalist assertion that the already poor in underdeveloped countries are better off than they would be in the absence of capitalist economies may seem equally plausible.

Ernest Mandel and Stanley Aronowitz deny the allegedly non-revolutionary character of the modern industrial proletariat. Mandel, citing in particular the "May 1968 French Revolution," argues for "the greatest confidence in the world which will emerge when the working classes, rejuvenated after two decades of slumber, will move to take power" (p. 186). Mandel's argument is more intransigent than Nicolaus-Sweezy's and is supported by some evidence. The persuasiveness of that evidence can be tested by any teacher who asks his students what the "May 1968 French Revolution" was or indeed how many ever heard of it. These incidents have already been forgotten or become history for the current generation. Only the believer can see them as portending an industrial proletarian uprising.

Aronowitz accepts Mandel's evidence but argues that it points, not to a resurgent industrial proletariat, but to a "New Working Class" consisting of "professional, technical and scientific workers." The student-white collar revolts "re-

flected a much deeper challenge to capitalist institutions" than was explicitly the case. No evidence is offered. Of those assessing the revolutionary potential of the three different proletariats, only Mandel need be taken seriously by the potential believer.

Part III ("Neo-Marxism") deals with the second important question of the book, namely, how adequate is Marxism as a system of thought? Whereas Marx claims his analysis provides a sufficient framework for understanding modern society, Irving Zeitlin argues that Marx needs to be "supplemented" by non-Marxist analysis and uses C. Wright Mills to show how Weberian insights may be incorporated into a Marxian framework without "supplanting" it. Zeitlin does not make an overwhelmingly convincing case. For example, he notes that Weber saw socialism accelerating "what already appeared as a frightening degree of bureaucratization in many strategic areas of social life" (p. 237). To show that this supplements without supplanting Marx, one would need to show how such insights can be squared with the disappearance of "the domination of men over men." One can accept such Weberian insights either by denying the goal of Marxist history or by identifying that goal with bureaucratization. Either solution is destructive of the Marxist framework.

Paul Breines and Ronald Aronson attack Marxist science. Marx claims the individual is produced by objective material conditions and that these conditions are the subject of science. Breines argues that "a coherent and unitary critique of modern society *begins* with a critique of individual existence" (p. 294 emphasis added). This is not necessary if the individual is a product of objective conditions. If the conditions ("modern society") are to be understood in terms of "individual existence," Marxist science is turned on its head. If the individual is to be understood autonomously, then Marxist science has to be replaced by individualist psychology. In either case, science, but not Marxist science, is still possible.

Whereas Breines's essay recommends Marcuse to the New Left, apparently on the grounds that Marcuse's "existentialism" is a remedy to New Left "authoritarianism," Aronson criticizes Marcuse on the explicit ground that he "takes his bearings and his refuge in a truth outside himself." Such "objective truth" is alienating, old Marxist, stuffy, joyless, etc. But denying such truth makes impossible any science. Marx denies that "objective truth" exists unchangeably and asserts it exists dialectically. Aronson denies that it exists at all. His argument means that Marxist science is no longer possible but its goal, "socialism," still somehow is.

Trent Schroyer attacks contemporary science as a "new strategy for legitimating power and privilege" through the doctrine "that knowledge is inherently neutral" (pp. 297–98). Positivist science, he asserts, is the new and "fundamental false consciousness of our epoch" (p. 301). Whereas Marx thought science would destroy capitalism, Schroyer holds that positivism (and a pox on "neo-Marxists" sympathetic to Weber) protects it. This dangerous argument is only half the story. Positivism, with its superstructure, academic freedom, also protects the ability of Marxists and socialists to teach in the universities. It thus permits fulfillment of Lenin's request to give him the minds of the young. The revival of American socialism is better served by a discreet silence regarding positivism than by assaults upon it. By his indiscretion, Schroyer's committment to the truth is shown to be greater than his commitment to socialism. For that reason, his essay is both more instructive and elevating than weak and unconvincing allegations of a "socialist revival" in America.

GARY D. GLENN
Northern Illinois University

Metropolitan Political Structure: Intergovernmental Relations and Political Integration in the Quad-Cities. By H. Paul Friesema. (Iowa City: University of Iowa Press, 1971. Pp. 158. $7.95.)

Prof. Friesema's book is an unpretentious "exercise in political mapping," (p. 140) which, within its carefully defined parameters, makes a valuable contribution to the rapidly expanding literature on U.S. metropolitan politics. The eight chapters of the book purport to describe "patterns of intergovernmental activity and other processes of political integration which occur among municipal governments in the Quad-City metropolitan area of Illinois and Iowa [Davenport, Rock Island, Moline, East Moline, Bettendorf and smaller towns]" (p. 1). *Metropolitan Political Structure* is not a comparative study, it deals only peripherally with public policy issues, and its conclusions about "integrative indicators" are derived almost wholly from 49 tables of data which were secured from close-ended, multiple-option interviews conducted with Quad-City public officials over a six-month period.

The virtues of this book are many, and include brevity and clarity, tightness of organization, and wealth of empirical data. The tabular data, it should be noted, are placed next to the explanatory textual materials, and they are always precisely labeled. Footnotes are placed at the bottom of the pages on which they appear, which is exactly where they belong in a book of this sort. A brief but excellent introductory chapter summarizes the recent literature on metropolitan

structure and appraises the pioneering work of such metro analysts as Robert Wood, Roscoe Martin, Winston Crouch, John C. Bollens, Henry J. Schmandt, Scott Greer, Frank Smallwood, Edward Sofen, and David A. Booth. The book's conceptualization is handled with admirable skill, and throughout the text empirical process analysis is quite properly given priority over abstract model building. Especially commendable is the virtual absence from the book of that fashionable jargon which disfigures and renders unreadable so much contemporary social science scholarship. At the end of the book there is a no-nonsense methodological appendix, which says exactly what needs to be said and no more. The appendix is followed by an extensive, four-section bibliography. Finally the author chose, wisely I think, not to clutter up his "exploratory" study with statistical tests of significance, the value of which would have been minimal in the context of his limited objectives.

Although the virtues of Mr. Friesema's book far outweigh its defects, a few of the latter should be mentioned here for balance. The book does not have an index—a shortcoming that is normally serious, but less so in this instance because of the book's brevity and the absence from the text of a large number of proper names. The book's style, while clear enough, is generally lacking in grace and vigor. Also the author tends, here and there, to belabor the obvious and repeat himself. And, though this will be disputed, I am inclined to believe that his decision to venture rarely if ever beyond the interview data in drawing conclusions was an example of excessive scholarly caution.

At least some of the merits of this modest but excellent study should probably be attributed to the editorial staff of the University of Iowa Press which, though relatively young as publishers go, is already proving to be an imaginative, venturesome institution. This reviewer spotted only three or four minor errors in the book, a record that contrasts most favorably with the sloppy editorial standards that increasingly are characteristic of the work produced by the most prestigious commercial and university presses.

Of the book's conclusions, three seem to be central. One, a metropolitan political system composed of many separate jurisdictions can be maintained in a functioning state in a wide variety of formal and informal ways. Two, the survival record of a political system, while not the only criterion, is a significant basis for judging the merits of reform proposals. And three, "this study should provide a warning to social engineers that the present political system in metropolitan areas is probably well-entrenched, and operating more rationally and effectively than many will acknowledge" (p. 140). All in all, Mr. Friesema's book is short, readable, timely, and thoroughly documented. It deserves a wide audience.

FRANCIS M. WILHOIT
Drake University

Participation, Planning, and Exchange in Old and New Communities: A Collaborative Paradigm. By David R. Godschalk. (Chapel Hill: Center for Urban and Regional Studies, University of North Carolina, 1971. Pp. 318. $6.00, paper.)

Citizen participation in planning is difficult to conceptualize. In his monograph, Professor David R. Godschalk tries, but is more successful in creating a normative ideal than a descriptive model.

Godschalk begins with the observation that "participation embodies different sets of values, goals, operating principles, and political feasibilities when it is viewed from the side of established authority than when it is seen from the side of partisan groups" (p. 8). Following Gamson, he distinguishes between the social control and the partisan influence perspectives.

> Gamson has shown that there are two major ways of looking at the relationship between citizens and government. His "influence" perspective takes the viewpoint of partisan groups, who see conflict as an opportunity to increase their influence through the use of the proper strategy. His "social control" perspective takes the viewpoint of authorities trying to achieve collective goals, who see conflict as a problem to be managed in order to maintain citizen commitment (p. 71).

Godschalk argues:

> Definitions of participation from either the social control or the influence perspective stake out and clarify extreme positions, one at the "macro" system level and one at the "micro" group level. By the nature of its role, however, local public planning typically must work with and between both levels. The planner needs a framework for integrating the social control and influence perspectives, based on a more operational concept than that of an invisible hand of political democracy (p. 18).

To the planner he recommends "a collaborative paradigm" based upon "exchange theory".

Somewhat murkily, Godschalk defines citizen participation in planning as a process of exchange between citizens and government, and identifies three exchange processes occurring at various levels of the social structure: participation, innovation, and conflict. In participation, citizens and government exchange influence and control; in innovation, they exchange communication regarding proposed change; and in conflict, they exchange threats (p. 74). He further distinguishes three types of exchange, depending upon the

expectations of the parties to the exchange: in collaboration, the parties expect to mutually benefit; in competition, to individually benefit; and in conflict, to reduce each other's benefits, regardless of individual gain (p. 23).

"Ideally, Godschalk observes, "the form of exchange taken by participants in planning will be one of citizen-government collaboration" (p. 24). He notes: "Exchange between citizens and government can be visualized as a continuum in which non-participation would be equated with extreme social control, active cooperation and competition would be equated with reciprocal exchange relationships, and conflict would be equated with partisan influence" (p. 127). His implicit argument is that planning suffers under conditions of social control or partisan influence—too little citizen participation or too much. Planning thrives when parity exists between planners and citizen participants, because planners innovate more and citizens accept more innovation. "By activating the community," Godschalk writes, "collaborative planning contributes to the achievement of the process goals of community self-guidance, transformability, and conflict regulation. By involving the community, collaborative planning seeks to escape the false distinction between political decisions based solely on community consensus and planning decisions based solely on professional expertise" (p. 148).

The difficulty with Godschalk's analysis lies in his ambiguous use of the social control, partisan influence, and exchange "perspectives." On the one hand, he uses them to describe the evaluative orientations that social scientists bring to their interpretations of citizen participation experiences: they identify with the citizens or the authorities (or with the planners as Godschalk does) and evaluate events accordingly. On the other hand, he uses them to describe the relative influence that authorities and citizens have in a planning situation. If the balance of power is tipped in the direction of authorities, he notes the dominance of the social control perspective, or if in the direction of the citizens, the dominance of the partisan influence perspective. By obscuring the difference between evaluative orientations of observers and relative power of participants, Godschalk recommends a change in evaluative orientation and expects a change in relative power. In other words, he recommends parity between planners and citizen participants without specifying the social and political conditions under which this parity has occurred or is likely to occur.

In Godschalk's paradigm, there are no economic inequities, no racial conflicts, no political history, no federal programs, no mayors, city councils, or city managers, no burgeoning urban bureaucracies, no neighborhoods deprived of "local liberty." Thus, it is not surprising that to test the usefulness of the collaborative paradigm, he briefly studies two new towns, Columbia and Reston—communities recently founded and remarkably uniform by class, composed of persons who elected planned integration, and administered by a corporation rather than a public body. Lacking an analysis of the determinants of the relative influence of planners and citizen participants, Godschalk labels the most marginal citizen contributions (reponses to surveys regarding the facilities that should be included in the next village center) as evidence of collaborative planning, and places Columbia and Reston near the middle of the continuum between social control and partisan influence—i.e., near the ideal. Such a test of the paradigm of collaborative planning ignores the political and historical context of citizen participation and trivializes the transformative citizen participation movement of the 1960s.

JUDITH V. MAY
Rutgers University, Newark

Hugo Black: The Alabama Years. By Virginia Van Der Veer Hamilton. (Baton Rouge, La.: Louisiana State University Press, 1972. Pp. ix, 330. $10.95.)

In writing judicial biography, one must decide how much attention ought to be devoted to the subject's pre-court origins and career. Insufficient development of the judge's antecedents reduces the biographer's ability to explain why the subject acts in a particular fashion, while too extensive attention to this dimension can result in a more superficial treatment of the judicial career. A number of effective biographies, such as Mason's *William Howard Taft* (1965), give scant attention to pre-court career; other substantial efforts, including Howard's *Mr. Justice Murphy* (1968), devote as much as a half of the text to analysis of the judge's formative stages. This dilemma is well reflected in the literature on Hugo Black, with volumes by John Frank (1949) and Charlotte Williams (1950) concentrating almost exclusively upon Black's Supreme Court period. Even the most complete analysis of Black, Strickland's symposium *Hugo Black and the Supreme Court* (1967), allots slight consideration to Black prior to his ascension. Professor Hamilton's volume is, therefore, unique in confining itself exclusively to Black's pre-Court activities, and thereby it nicely complements the existing literature on Black's Supreme Court career.

Hamilton frankly warns the reader that her focus is limited to "political biography," that is, the analysis concentrates upon the influences that shaped Black and his activities prior to judicial appointment; it is not meant to encompass "personal biography" which delves into the subject's

thoughts and innermost private matters. Nonetheless, the picture of Black which emerges is facinating. Black is portrayed as the typical ambitious lawyer seeking to maximize his potential within the framework of Alabama politics. Interspersed with periods of often lucrative practice, Black undertakes stints as criminal judge and later prosecutor, always discharging his reponsibilities with one eye on the media. At the same time that he defends labor unions and advocates government electric power, he also projects an image of prohibitionist, conservative Protestantism, and an aversion to open immigration. Black secretly joined the Klan in order to develop a power base, even more covertly resigning before the 1926 campaign for the Senate.

Black's Senate career was characterized by numerous publicity-seeking investigations, speeches in support of court packing and limiting federal power to investigate allegedly corrupt Senate elections in Alabama, and persistent opposition to House reapportionment. Yet some flashes of the later Justice Black do emerge: opposition to customs seizure of obscene books, support for compulsory lobbying disclosure, and espousal of New Deal labor regulations and relief measures. But overwhelmingly, however, the picture which develops is of a primarily conservative southern Senator, carefully protecting constituency support with his opposition to anti-lynching bills and his conduct of colorful utility holding company investigations, and occasionally involving himself in mildly progressive reforms. Roosevelt's decision to nominate Black for the Court (his first choice was an unwilling Sherman Minton) apparently rested upon the slender criteria of loyalty to the New Deal, Senate membership, and geographic representation.

A complete picture of Black is somewhat puzzling: Mr. Justice Black appears very different in action and values from the earlier Black. But since Hamilton's analysis ceases upon Black's appointment, we cannot tell whether Black had more liberal ideas all along or whether he underwent some intense re-examination after joining the Court. Hamilton seeks to discount some of Black's more conservative actions as simply skillful politics. She argues that Black's ten years as a senator did broaden his values, injecting a considerable dose of nationalism, and this, coupled with the Depression and some extensive reading, laid the foundation for a Black with new perspectives. A future integrated biography may clear away some of the mystery. The Hamilton volume stands as a well-researched, clearly written, and comprehensive analysis of Black's pre-Court period. If Professor Hamilton does on occasion offer some rather strained rationalizations for certain of Black's actions (he joined the Klan perhaps to moderate it from within), the author usually maintains the correct perspective for a biographer—skillful blending of a detailed familiarity with the subject with an appropriate measure of objective detachment. The book would seem indispensable for any complete understanding of Hugo Black, the man and the Justice.

R. H. CLARK
Denison University

Young Men Look at Military Service: A Preliminary Report. By Jerome Johnston and Jerald G. Bachman. (Ann Arbor: Institute for Scoial Research, The University of Michigan, 1970. Pp. 115. $5.00, paper.)

Public Opinion and the Military Establishment. Edited by Charles C. Moskos, Jr. (Beverly Hills, Calif.: Sage Publications, 1971. Pp. xvi, 294. $12.50, cloth; $7.50, paper.)

Can a democratic polity tolerate the long-term presence of large professional military forces? Might not the presence of such powerful, conservatively oriented institutions eventually cause the polity to sicken and die, or to lose its democratic ethos? On the other hand, might not such a presence overstimulate other interest groups to penetrate and reshape military institutions and make them no longer competent to perform their intended tasks?

These alternatives encompass some worst fears of "antimilitary polemicists" on the one hand, and "apologists of the warfare state" on the other—two groups who, Charles Moskos says, too often dominate discussions of military roles in American society. Moskos, editor and co-author of *Public Opinion and the Military Establishment*, urges that: "If our society is ever to fulfill its democratic promise, the relationships between its civilian and military structures require especially informed and sustained attention" (p. xiv). The ten papers collected in that volume, all written before 1971, do not put to rest those worst fears; but they do address some crucial problems and issues that need to be faced as the nation and its post-Vietnam military system learn to live with each other.

Two concluding papers, by Moskos and Morris Janowitz, deal most directly with large issues in the confrontation between a disillusioned society and its shrinking, all-volunteer military forces. Moskos proposes that after two decades of increasing institutional convergence of the armed forces with civilian society, the trend has reversed and the armed forces will increasingly turn inward. He explores the relative degrees of isolation induced in enlisted and officer ranks by current trends in recruiting, race relations, skill-transferability, and military family life. He is especially

concerned that reduced middle-class representation in the enlisted ranks, together with increasing emphasis on college and higher education for the officer corps, will promote caste-ridden and authoritarian military structures, even as civilian institutions are adapting to pressures for more participative forms of control. Janowitz sees similar trends, and also underlines the role of civilian pressures in contributing to both civilian society's and the military forces' sense of separateness.

Janowitz sees some hope for relief if the military services can exploit opportunities afforded by their new all-volunteer base to restructure the military profession. The aim, as he sees it, ". . . will be to develop a military [officer] profession which is closely articulated with civilian society" (p. 260). This will not be easy. All-volunteer services will mean reduced opportunity to recruit officers from the enlisted ranks. Both antimilitary and cost-cutting pressures on ROTC will shift that recruiting base even more toward surviving units located disproportionately in the South and West, at schools with modest academic standards. And a larger proportion of new officers will come from the service academies. Even so, as other contributors to the Moskos volume suggest, the impact on the true officer recruiting base may not be all that constrictive. William Lucas, in an impressive analysis of "Anticipatory Socialization and the ROTC," Lawrence Radway, in an intelligent review of "Recent Trends at American Service Academies," and less friendly and less carefully argued papers, all stress the importance of *self-selection* in recruiting youths to officer training programs and in shaping the product of these programs. It is not clear from any of these studies what will be the real impact of narrowing the officer recruiting net, of attenuating its reach into the upper and lower ends of middle-class recruiting. But it seems unlikely that these changes will have anything like as great an effect on the character of the officer corps as did those reflected in the broadening of military leaders' social backgrounds that took place in earlier decades of this century (see Morris Janowitz, *The Professional Soldier: A Social and Political Portrait*, Glencoe, The Free Press, 1960).

Janowitz is, in fact, less concerned to counter recruiting trends than he is to reform officer education and career development. "Like professional development in a wide area of contemporary civilian society, the officer will require both higher degrees of expertise in his particular area of competence and at the same time heightened consciousness of the responsibility of the military establishment as a whole" (p. 266). Some of his suggestions, such as admission to the service academies after three years of high school and the structuring of officer careers around geographic as well as functional specialities, might prove counterproductive to closer military-civilian articulation. Other proposals seem better directed toward the goal: send service academy students to civilian colleges for their junior year; encourage ROTC cadets to break their college years with a year or two of enlisted service; continue placing a large proportion of officers into higher degree programs at civilian universities; make social science education and politico-military experience a normal part of career development at all levels of the profession, rather than tying them to development of a few staff specialists and to the shallow orientation of officers marked for higher command.

All these proposals are relevant to Janowitz's aim of encouraging military professionals to accept " . . . the notion that a successful officer can be one who does not fight, but contributes to deterrence and the resolution of international conflict" (p. 258). Similar calls for reforming the profession are being heard from other quarters as well, including thoughtful military professionals. But there are problems in relations between professionalized military services and a liberal-democratic polity that go beyond the need to encourage military leadership to perfect its rational expertise and avoid resentful inversion to traditionalism, absolutism and authoritarianism. As Samuel Huntington argues: "The parallels to the modern problem of civil-military relations are to be found not in the Praetorian Guard but in the relations that exist in modern states between political leaders, on the one hand, and such specialists as diplomats, civil servants, scientists and economists on the other." ("Civil-Military Relations" in *International Encyclopedia of the Social Sciences*, ed. David L. Sills [New York: Free Press, 1968], Vol. 2, p. 490). Surely these aspects of relations between a new military professionalism and American society also require "especially informed and sustained attention."

Among other issues raised in the Moskos collection, mention should be made of Bernard Beck's intriguing thesis on "The Military as a Welfare Institution," and David Sutton's sobering (but outdated) study of "The Military Mission against Off-Base Discrimination." Both pose interesting questions about extramilitary aspects of relations between the armed forces and society.

A number of the papers incorporate exercises in analyzing survey and other forms of aggregate data. Except for William Lucas's study of anticipatory socialization, these exhibit serious weaknesses that seem to reflect their authors' political and social biases. Similar bias seems to color some interpretations in the otherwise impressive analysis by Jerome Johnston and Jerald Bachman in *Young Men Look at Military Service: A Prelimi-*

nary Report. The report is on a part of the third phase of a longitudinal national survey involving more than 2,000 boys entering the tenth grade in 1966, and explores their employment plans and their political attitudes as they completed high school in 1969. The authors find that a majority of their respondents did not dissent from their government's policy in Vietnam, but that a significant minority, some twenty percent, did. At the end of their imaginative and resourceful analysis, the authors conclude that " . . . dissent over the U.S. Government's Vietnam policy is, for some young men, a major deterrent to military service" (p. 34). But all the data and analysis show is an association between respondents' attitudes toward the Vietnam War and their perceptions of the likelihood of entering military service, an association that supports several different causal hypotheses. Aside from this brief lapse, Johnston and Bachman are most careful to point out and not exceed the limitations of their data. Their intelligent caution might serve as a corrective model for some of the less successful pieces in the Moskos collection.

It is difficult to understand what further policy-shaping or public-informing purposes the Johnston and Bachman report might serve this late in the commitment to all-volunteer military forces. The Moskos volume, on the other hand, does have enough provocative ideas to make it a useful addition to any private or institutional collection on contemporary military affairs.

James M. Gerhardt
Southern Methodist University

Race, Class, and Political Consciousness. By John C. Leggett. (Cambridge, Mass.: Schenkman Publishing Co., Inc., 1972. Pp. 243. $7.50, Cloth; $4.95, paper.)

At least since the 1930s, social scientists have tried to explain discrimination against ethnic minorities. The most popular explanatory variables used have been personality; attitudes; fears (rational or otherwise); childhood socialization; peer group, reference group, or subcultural group influence; social status; psychological dislocations caused by upward or downward social mobility; as well as combinations of any of these variables. In *Race, Class, and Political Consciousness*, John C. Leggett takes the vote against housing integration in the 1963 Berkeley, California, fair-housing referendum as symbolic of discriminatory behavior, and attempts to explain it in terms of several demographic and attitudinal variables. The reader infers that the negative vote is intended to symbolize discriminatory behavior because the rambling development of hypotheses in chapter two refers only to what Leggett variously calls discrimination, prejudice, and racism, terms which denote interpersonal behaviors.

The results are expected. People of higher occupational, educational, and income levels were more likely to vote for housing integration than people at lower levels, although when political party preference was held constant, it was found that Democrats at every level were more likely than Republicans to vote for housing integration.

Additionally, the less the amount of church attendance, the more likely the vote in favor of housing integration; while for those who expressed a religious affiliation, the ones belonging to the more liberal wings, e.g., Jews, Unitarians, Congregationalists, were more likely to favor integration. Similarly with university ties: employees and students of the University of California were more likely than others to vote for integration.

Further, the older the voter, the more likely to oppose housing integration, a phenomenon which Leggett attributes to differences in experiences among age cohorts, those over 50, for example, being labeled the Unruffled Generation and said to be mostly "committed to a racist perspective" (p. 124). Similarly, length of residence in Berkeley shows an inverse relationship with a vote for integration. Most of these relationships wash out, however, when political party preference is held constant, Democrats at all levels being more likely to favor housing integration than Republicans.

Finally, the relationship between what Leggett calls an authoritarian ideology, and the fair-housing vote was investigated. Although the ideology is measured by items from the Adorno F-scale, it is not the same as an authoritarian personality. Authoritarian ideology consists of ethnocentrism, politico-economic conservatism, and rural cultural traditionalism. Leggett believes he is measuring an ideology, rather than a personality, because of the similarity between the items and a supposed rural cultural tradition. As expected, the results show that the less authoritarian the respondent is, the more likely he is to vote for housing integration. An attempt to check the strength of the other variables when authoritarianism is held constant is marred in relying on only three items dealt with individually.

As this review has meant to convey so far, neither the book nor the study on which it was based, seems particularly inspired. Both the author and publisher claim that publishing the results of the study ten years after it was made throws light on events during the interim, but I fail to see it. It seems to me that the results would have been highly predictable in 1963 and at any time since. Perhaps the author's insistence that political party preference itself may determine attitudes toward political events shows originality and in-

sight. It is certainly an insight that deserves to be followed up.

Beyond this, the book has some methodological and stylistic flaws. We are never told the form of the questionnaire on which the study was based, nor how it was constructed. We are not told how, nor whether, the interviews were validated, although we are told that some of the students employed "quickly became excellent interviewers." An indeterminate number of people were interviewed for the study as much as a year or more after the election, meaning that there was likely to be a problem of recall for some respondents. Yet this issue is ignored. On the positive side, the sample was carefully chosen, and it was tirelessly pursued to minimize nonresponses.

I suppose it is unfair to fault a writer for stylistic flaws when it is presumably part of publisher's responsibility to correct them, so let them take equal blame for such inelegancies as: "innovational" for "innovative," "Second Holocaust" for "World War II," "heavenly pastures" for "death," "encharged" for "charged," "demarked" for "demarcated," "dressing apparel" for "clothes," "low ascribed ethnic honor" for "low ethnic status," "nucleated spatially," for "geographically divided," and "white dromedary cases" for what?

MICHAEL COHEN
Governors State University

Phil Swing and Boulder Dam. By Beverly Bowen Moeller. (Berkeley: University of California Press, 1971. Pp. 199. $8.50.)

Ms. Moeller has contributed to our knowledge about the formation of natural resources policy by spelling out the very significant leadership role played by Congressman Phil Swing of California in securing passage of the Boulder Canyon Dam Act. For this alone, her book deserves to be cited in the literature. Herbert Hoover, for whom the dam was named, and to whom much of the credit has been given by other writers, is portrayed as less than an enthusiastic backer of the project.

As a contribution to understanding policy formation, however, her book has two major faults. First, the author never seems to have grasped the place of the Boulder Canyon Dam fight in the entire conservation controversy and in the liberal-conservative struggle within the Republican party, a struggle which eventually drove the Progressive Republicans into support of Roosevelt's New Deal. She reports support from Senator Hiram Johnson and other progressives and reports Swing's visits to other areas such as Eastern Washington where a comparable fight was going on for Grand Coulee Dam, but does not appear to grasp the significance of the broad alliance which was being built.

Second, she fails to comprehend the essential facts about the conservation coalition itself. This coalition, which Henry Caulfield described in his "Three Thrust" theory more than a decade ago, involved development-oriented groups (in this case the irrigators) and regulationist groups (in this case the public power people). Los Angeles was a public power city and was a key to the final policy decision. Although Ms. Moeller hints at these ties, she never relates them to the central issues at stake.

The upshot is a strictly chronological report of undifferentiated events in which a dizzying whirl of new people and next steps are portrayed without any interpretation of significance, interrelationships, or strategy. Routine group meetings are given the same status as significant turning points. The author seems to be content to demonstrate that Swing was present or involved in much of the activity. She apparently either did not understand the substantive national policy issues at stake or felt no obligation to try to relate them to her story. A marvelous topic and much very good research are thus spoiled by dreadfully routine hop-skip-and-jump reporting.

DANIEL M. OGDEN, JR.
Colorado State University

Political Entrepreneurs and Urban Poverty. By Russell D. Murphy. (Lexington, Mass.: Heath Lexington Books, 1971. Pp. xi, 208. $12.50.)

The political entrepreneurs of this slim volume are administrators who developed and implemented a modestly successful antipoverty program for New Haven during the reign of Mayor Lee. They were prudent politicians who took it upon themselves to do something for the poor. They planned, secured support for, and administered a program without much initial political support from the community. Most of the important parts of the book show how these bureaucrats (Murphy uses this term without the usual negative connotations) came to develop support from various interests and how they were able to use local and national resources to succeed in their venture. In short, the study tells how clever and thoughtful people took full advantage of position, resources, and skills to build a program.

Part I lays out the author's methodology. Part II provides broadly narrative background pieces describing the local and national context of the New Haven antipovierty program. Included are interpretative essays on New Haven, its politics, Mayor Lee's style, and the bureaucratic genesis of the poverty program. All have a decidedly pluralist cast in the Dahl tradition. Instead of an economic elite, New Haven is pictured as having a multiple elite structure including political party factions, public agencies,

private organizations and citizen groups. Political competition existed among various groups and organizations with the Mayor's office serving a catalytic function. Bureaucrats performed crucial development and policy roles in the city government.

Part III is much more analytic (by design) and deals with the processes of development of support for the program. Apparently in the early 1960s, there was little local demand for clearly antipoverty programs; most of the city's concern took the form of conventional social welfare programs. Thus, the small group of administrator who took advantage of and encouraged the development of the national war on poverty had to carve out a place for the antipoverty program. "Bureaucratic Reciprocity" and "the Matching Game" are two devices used to establish support and resources for the program.

The New Haven group was able to ally itself with the federal government and the Ford Foundation. The local administrators formed alliances, with New Haven offering a proving ground in return for financial support. Their model antipoverty project became a focal point (as redevelopment had in the past) of cooperative enterprise.

Part IV is the most useful part of the book. It provides an excellent applied analysis of the processes and strategies used by agencies in the development of coalitions and constituency relationships. Much conventional analysis of clientele relationships tends to focus on perverse and dysfunctional aspects of agency-constituency behavior. Professor Murphy stresses the positive aspects. His analysis specifies conditions under which the entrepreneurial behavior of public administrators may contribute to the overall welfare of the community. Of particular interest are his analyses of coalition formation, linkages with the community, and community response. He shows how self-interested people playing bureaucratic roles may foster social change.

The closing chapter discusses the social and political role of these new men of politics. Here the author's ties with the heritage of Professor Dahl and his followers is most evident: instead of economic notables or social elites, exerienced and thoughtful administrative politicians took advantage of outside support, personal resources, and the fragmented structure of New Haven politics to fashion modest change in social policy. According to Murphy, these people laid the foundation for a lasting coalition which includes the poor—a notable accomplishment in America.

Although little evidence supports the last hopeful generalization, the volume is worth reading as a study of successful administrative behavior in an urban setting. Like many dissertations turned into monographs, the book is a little too narrative and case oriented. The last 150 pages justify the time spent reading through the preceding narrative. Much more pointed, this portion is one of the most insightful studies of administrative leadership done in the past several years.

LOUIS F. WESCHLER
University of Southern California

Government Management Internships and Executive Development. Edited by Thomas P. Murphy. (Lexington, Mass.: Lexington Books, 1973. Pp. 281. $12.50.)

Implicit in Professor Murphy's anthology is the assumption that administration acutely depends upon innate ability, common sense, and work experience, and not exclusively on book knowledge and classroom theory. While Murphy might not cavil at the order of importance, he would take umbrage at the mutual exclusivity implied by many of the critics of experiential education. He might further contend that internships suitably provide occasions for a kind of *verstehen* for the students, or an attempt to give them a feel for the reality of administration. Thus, intuition and practical experience need not be divorced from knowledge and analytical exercise. Each can effectively tutor the other. It is his firm belief that knowledge of administration derived from internships—insofar as they are purposefully oriented towards the testing of theory—affords the student the opportunity to view the organization as a dynamic composite of people communicating, interacting, and bargaining—in short, behaving in the richest sense of the term.

The currrent popularity of internships, says Murphy, is a bold manifestation of the continuing support for programs of professional education in the areas of public policy/administration. The internship is perhaps the best way to provide "hands-on" experience for students of administration.

Apart from treating some of the academic questions associated with internships, the book reviews various fellowship programs at the state and federal levels of government. For instance, individual chapters focus on such federal programs as Congressional Fellowships, the White House Fellowships, the HUD Urban Fellowships, and the Washington Semester Internships. What is conspicuously absent—save for a passing reference in the preface—is a chapter on the Public Administration Fellowship Program which provides fellowships in the executive branch of the federal government to college and university professors in the field of public administration and other supportive public policy areas.

Fundamentally, Professor Murphy's book provides us with a useful weapon in the campaign to

professionalize governmental manpower. The author gets expert assistance from seventeen contributors drawn from six separate perspectives, most of them academicians. This tightly knit collection of readings is adumbrated into four distinct categories: (1) Internship Dynamics, (2) Urban and State Internships and Fellowships, (3) Federal Internships, and (4) Executive and Organization Development.

The final section of the book seeks to analyze the nature and character of federal executive development programs as distinguished by the Federal Executive Seminar Centers for its middle management corps. Finally, the book concludes with chapters on the application of the behavioral sciences to the training effort in local government organizations, and the related effort behind the Intergovernmental Personnel Acts' proposal to help strengthen state and local governments through improved personnel administration and more effective recruiting and training.

If the book has any shortcomings it is that the editor has not given the intellectual direction that would shape the compilation into a coherent whole. It would seem that the instructions to the contributors were minimal and this is reflected throughout the book. Moreover, the reader looks in vain for sectional prefaces that would bind together the scattered observations of the contributors.

In sum however, while parts of the compilation vary in quality, insight, and substantive merit, taken as a whole, the volume is useful for those of us who seek to promote public service by mating scholarship with experiential learning in order to professionalize government administration.

RAYMOND POMERLEAU
San Francisco State University

Tammany: Evolution of a Political Machine, 1789–1865. Jerome Mushkat. (Syracuse, N. Y.: Syracuse University Press, 1971. Pp. 476. $15.00.)

For many years, historians who wrote about political machines tended to judge them moralistically from the perspective of the reformers who opposed them. During the last two decades, however, a new paradigm has evolved. This paradigm essentially rests upon a structural-functional analysis, although within the larger framework there are variations of approach. If Lincoln Steffens could be called the godfather of the first model, then sociologist Robert K. Merton (with a nod toward Edward Banfield & James Q. Wilson) would be the patron saint of the second. Historians today generally agree that political machines arose because of the inability of existing urban governments to meet the needs of cities that were growing larger and more complex. Machines supplied jobs and welfare services for the largely ethnic urban lower and working classes and provided a mobility vehicle for those who found avenues to success blocked (often for ethnic reasons) in the commercial world. They made alliances with businesses that needed either government grants, such as franchises or protection from police interference, and expedited urban decision-making processes. Some historians who have written within recent years have stressed the importance of examining the group characteristics of machine politicians and of their opponents, the relationships between demographic variables and voting patterns, and the ethnocultural context in which the machine operated.

In this volume, Professor Jerome Mushkat analyses the evolution of the nation's longest-lived political machine, Tammany Hall, throughout the first seventy-five years of its existence. In his forward, he announces that he will proceed with his study within the "normal science" approach (to borrow T. S. Kuhn's terminology) followed by other recent historians of political machines. Tammany, he tells us, evolved as part of the process of "nineteenth-century American urbanism" and "'boss' rule grew naturally as the city itself grew." "Far from being unique or reprehensible, [Tammany] was eminently a product of American civilization" (p. x, p. 2). Mushkat proceeds to trace the uneasy evolution of Tammany from a nonpolitical fraternal order to a well-disciplined political organization. He argues, however, that Tammany was more than a group of skilled political mechanics—that it actually had "a political philosophy that the majority of city voters found congenial" (p. 373). This philosophy rested upon the concept that the government derived its will from the people and that public opinion limited what government could and could not do. In its search for the most productive political issues, Tammany made a series of bewildering policy shifts as it sought issues that brought votes.

Mushkat attempts to lead us through the thickets of Jeffersonian and Jacksonian politics, the bank wars and the Loco Foco revolt, the crisis caused by the aid-to-religious-education issue in the early 1840s, the problems caused by the erratic mayor Fernando Wood in the 1850s, and the rise of Tweed to power during the Civil War years, but he often becomes mired in a swamp of names and details. Not only does his analysis lack discipline and order, but it also fails almost completely to answer systematically the questions posed by the present "normal science" approach to the analysis of machine politics—that is, to analyze Tammany's evolution in relation to the process of urbanization. Aside from the aid-to-education issue, Mushkat pays almost no atten-

tion to problems stemming from urban growth such as welfare and public health. Absent also is a systematic examination of either Tammany's leadership or the leadership of its opponents, as well as any attempt to relate voting patterns to ecological, demographic, or class variables. Even though the author's bibliography cites a vast number of secondary and primary sources, he seems to have learned little from the sophisticated work of such historians of New York as Frank O. Gatell and Edward Pessen.

In short, although Mushkat has obviously done an immense amount of work in the sources, he has failed to translate these sources meaningfully for his readers. He has neither met the standards set by today's "normal science" approach to the analysis of political machines nor suggested a different approach that could lead to the development of a new paradigm.

JOEL A. TARR
Carnegie-Mellon University

The Rights of the Accused. Edited by Stuart S. Nagel. (Beverly Hills, Calif.: Sage Publications, 1972. Vol. I, Sage Criminal Justice System Annuals. Pp. 320. $15.00.)

Following a short and very general overall introduction to the subject by Professor Nagel of the University of Illinois, who has written extensively in this area of our public law, nine specialists contribute essays in this book on particular segments of the subject of the right of the accused, ranging from pretrial arrest and search to such post-trial matters as punishment and double jeopardy. In general, each contributor follows a common pattern of describing some particular right, or bundle of rights, of those who are accused of crime, bringing the exposition down to date, and pulling together whatever empirical evidence is available as to how each right fares at the actual operative level. Thus, these essays combine traditional explication of constitutional law with a considerable amount of material on what is often called "law in action." On the whole, the essays are descriptive, and steer fairly clear of such overall philosophical questions as where to locate the proper balance between the rights of the accused and the rights of the victims of crime. In fact, the latter are scarcely mentioned.

The more general theoretical concepts, to the extent that they are treated at all, are spelled out in Professor Nagel's all-too-brief opening chapter. Here he asserts that the main purpose of those who have legislated into existence the rights of the accused has been "to achieve the effect of lessening the likelihood that an innocent person will be convicted or harassed" (p. 17). I think this is an overstatement, since even guilty people are entitled to due process of law, and when they are assured due process, that makes a difference. Thus, even a guilty person is entitled to representation by counsel, if for no other reason than that a skilled advocate may be able to plea bargain the accused into a more favorable situation. If there is an overall concept in our criminal justice system, it is to protect and expand the amenities of a civilized community. Thus we frown upon illegal searches and seizures not only because they invade the legal rights of the victims of such practices, but also because we value privacy and the right to be left alone, and we seek to avoid brutalizing the police.

Professor Nagel identifies three patterns of differential treatment based on the characteristics of the accused: (1) the disadvantaged pattern, dealing with indigents, blacks, and the less educated; (2) the paternalized pattern, dealing with juveniles and women; (3) the industrialized pattern, concerned mainly with urban rather than rural areas. This is an interesting model, but it is not utilized, except in a rather accidental and unorganized manner, in the specialized essays that follow. Of course, as Professor Nagel points out, the rights of the accused have been expanding, when viewed in the perspective of the last half century, and he connects this trend in part to the increasing affluence and middle-class character of the American society, and to improvements in education.

I suppose it is trite to say once more that a symposium is bound to be uneven. Some of these essays are excellent; some are of middling quality; and James Marshall's essay on trial testimony scarcely belongs in the volume at all. But on the whole, I found these essays well written, properly annotated, and presented with the authority of expertise. Professor Jacob Landynski, of the New School for Social Research, and author of our best book on the subject (*Search and Seizure and the Supreme Court*, 1966), contributes one of the most satisfying essays in the book on his specialty. The well-known civil liberties lawyer, O. John Rogge (author of *Why Men Confess*, 1959), supplies the essay on self-incrimination, in which he advances the extreme view that whenever a defendant repudiates any confession in court, for that reason alone it should be inadmissible in evidence. I know of no court which would accept this view, and I question its wisdom. The processes of accusation are described by Delmar Karlen, Director of the Institute of Judicial Administration (and author of many books, including *Anglo-American Criminal Justice*, 1967), and by J. Lawrence Schulz, who teaches at New York University Law School. Here prosecutorial discretion is analyzed in great detail. Professor William M. Beaney, of the University of Denver Law School, and author of our

best book on the subject (*The Right to Counsel in American Courts*, 1955), has prepared a learned essay on his favorite subject. Indeed, Professor Nagel believes that the right to counsel may well be the most important of all of the rights of the accused, though I think the point needs the support of more empirical evidence than is presently available. The remaining essays are concerned with: the right to bail, by Patricia M. Wald (author of *Bail in the United States*, 1964); the jury system, by Rita James Simon, sociology professor at the University of Illinois (author of *The Jury and the Plea of Insanity*, 1967), and Prentice H. Marshall, law professor at Illinois; the conduct of the trial, by James Marshall, a New York lawyer (author of *Law and Psychology in Conflict*, 1966); sentencing and correction, by Robert O. Dawson, Professor of Law at the University of Texas (author of *Sentencing*, 1969); and finally, double jeopardy, by Professor Jay A. Sigler of Rutgers (author of *Double Jeopardy*, 1969).

All in all, the editor did a good job of selecting experts to discuss their specialities, and this is an interesting, timely, and informative book. The level of scholarship is very high, and, except for occasional lapses (e.g., "K. Culp" for "Kenneth Culp Davis," at p. 297), the editing is well done. The tenor of the book is optimistic, but whether this optimism will survive a few more years of a Court presided over by Chief Justice Burger remains to be seen. Probably not.

DAVID FELLMAN
University of Wisconsin

Bureaucracy and Representative Government. By William A. Niskanen, Jr. (Chicago & New York: Aldine-Atherton, Inc., 1971. Pp. 241. $7.75.)

Bureaucracy & Representative Government, by William A. Niskanen, Jr., is one of the more impressive books I have read in recent years. There can be no question that it is the most significant work yet produced by an economist on the role of bureaucracy in the determination of the supply of public goods and services. I fully expect the book to attain the status of a classic in the study of bureaucracy and, more generally, public choice. Professor Niskanen has managed to combine a rare set of scholarly achievements: imagination, rigor, policy relevance, and a remarkable felicity for clear and apt expression.

Niskanen's major concern is to explain the supplies of public services as they are directly affected by the choices and behavior of bureaus and bureaucrats. The question is so obvious and so important that it has rarely been raised by political scientists and students of organizations and only "casually" or tangentially answered by those who, like Tullock and Downs, have addressed themselves to problems of bureaucracy. The only theorists who have consistently faced the problems of the supply and demand for public goods have been economists dealing with public choice, but their work has rarely inspired, let alone informed, political scientists and others who write about the general phenomena of bureaucracy and public administration. Public choice theorists, including Buchanan, Musgrave, Olson, Samuelson, Williams, and others have not stressed the role of bureaucracy. Instead, they have focused upon such other variables as voting rules, the dynamics of political competition, the calculus of voters and politicians, or they have ignored the empirical side and elaborated normative criteria for resource allocation. Niskanen does not ignore these other positive and normative questions; indeed, he devotes considerable effort to illuminating the political environment of public agencies in democracies as well as confronting policy dilemmas.

In concerning himself with the output of nonprofit organizations and, especially the governmental, Niskanen relies upon standard economic tools of analysis (price theory) to guide the inquiry and supply the answers. How successfully can hardly be decided in any "once-and-for-all-time" manner; a legion of critics will doubtless question the elegance and robustness of his model(s) as well as question his command over and application of economics and mathematics. And even those who have some difficulty in reading the book will rise to the occasion to find fault with his "conservative" policy positions. Liberals, in particular, will challenge his major contention that most if not all governmental bureaus "overproduce" their supplies of services to the public. Galbraith, Downs, and Olson had already advanced their contentions that we normally experience "undersupplies" of most public goods. Each did so via a different theoretical route but their conclusion was the same. None of them, however, formulated the problem in quite the same way as Niskanen nor did they achieve the same degree of analytical rigor and imagination in the testing of their propositions. Niskanen modestly refers to his tests as "subjective empiricism" (p. 8).

The basic argument of the book is this: Bureaucracies, i.e., bureaucrats being motivated by a concern for their own salaries, attempt to maximize their budget shares or allocations from the total budget. Accordingly, their output choices are governed by the size of the budget. While the budget is formally enacted by elective public officials (sponsors) the bureaucrats have a considerable say because of their superior bargaining position. The bureaucrats' superior bar-

gaining position is based largely on more and better information, as well as their greater willingness and capacity to pay the costs of bargaining with the sponsors. Just as a producer in the market can concentrate his energies and information so can the bureaucrat do so in the polity. Both the consumers and citizen-taxpayers find themselves at a relative disadvantage because they must disperse their concerns and resources over many more disparate decisions. One may well ask what, if anything, limits the demands of the budget-maximizing bureaucrat? Why are governmental bureaus not larger, and why is their output not still greater? The answer is simple: The bureaucrats are constrained by the politicians' reactions to competing demands for public services and the never-ending complaints about taxes. Since the bureaucrats must win the approval of the sponsors or politicians even to *have* a budget they must supply whatever the politicians demand. As Wildavsky pointed out in *The Politics of the Budgetary Process*, a bureaucrat must not promise more than he can deliver or he will pay the ultimate penalty of a smaller budget. On the other hand, if he does better than expected he may be rewarded with a larger budget or other concessions.

Niskanen's recognition of the considerable power of the voters and their representatives (p. 42), probably does some damage to his basic model, as he seems to recognize in Chapters 14–16 where he relaxes the basic model and inserts a number of more realistic institutional assumptions. I must confess that I have not, as yet, been able to decide whether the "aggregative" model in these latter chapters ie entirely consistent with the basic model. The problem may be an outgrowth of my more limited command of price theory. In any event, Niskanen develops his models far beyond anything yet done on public bureaucracies. While the basic messages are clear and simple, the proofs are considerably more detailed and elegant than is customary in writings on public administration.

Among the more clearly-stated and, perhaps, controversial themes are those pertaining to the policy implications of the several models. I should like to end on these matters as does Niskanen. Private, public, and mixed private-public goods and services are produced by a bewildering array of governmental and private organizations guided by mixtures of profit and nonprofit objectives. While Niskanen attempts to account for the production decisions of the nonprofit public bureau, one has the feeling that he does so chiefly as a means for assessing the optimality of those choices i.e., he is much more concerned with supplying policy answers to questions like, which types of organization should produce how much of which types of goods and services?" While Niskanen asserts strong preferences for eighteenth-century liberal ideals he opts for efficiency as the chief criterion by which to judge organizational performance. Unfortunately for the citizenry, resources are seriously misallocated by the bureaucracy as whole and by individual bureaus producing specific services.

Niskanen, accepting a self-imposed policy challenge, suggests ways of achieving greater efficiency in the public sector. Some of his proposals are conventionally conservative; others are not. Some are politically feasible; most seem less so. Most will irritate liberal political scientists. All proposals, however, flow from his positive models. More precisely, the proposals are of two types: one set pertains to reform of the bureaus themselves, while the others pertain to the political system within which the bureaus function. With respect to the former, Niskanen advocates more extensive competition among bureaus producing the same output thereby giving citizens consumer options as occur in the marketplace. While competition among local and state governments exists and public and private bureaus currently compete to some degree, as in the case of postal services and police protection, Niskanen wants the entire public bureaucratic world to assume some of the characteristics of the marketplace and thereby more closely and frequently approach Paretian optimality. Niskanen views such competition as helpful, but he views reform of the political environment or the polity as more critical if less likely to happen. Indeed, imposing competition on the bureaus is itself a political reform of no mean proportions. The last sections of the book (Parts VI and VII) spell out Niskanen's conception of representative government and briefly set forth his reforms. Like most public choice theorists, Niskanen takes a dim view of the electoral process and the behavior of politicians. The culprit is uncertainty, which is regarded as inevitable and far more pervasive than in the market; this being the base, Niskanen advocates reforms that do not so much remove uncertainty as attempt to minimize its impact. In short, Niskanen hopes to "reduce the chances of disastrous behavior by government rather than . . . induce optimal behavior" (p. 137). Niskanen is either a Waldian pursuing a maximin strategy or a strategist opting for Savage's minimization of regret. In either case he is pessimistic.

Niskanen places heavy emphasis upon strengthening the review process by which budgets are formulated and enacted within the legislative and executive branches. Unlike others who press for the same reform, Niskanen suggests that com-

mittee and cabinet assignments be randomized on a periodic basis. A variety of other organizational reforms are also advanced including, a policy research institute for the legislature as a whole. These and other such reforms are designed to minimize the role of strong advocates within governmental offices and the high-demand group for public expenditures among the citizenry. A more consequential reform proposal is that of adopting an extra-majority instead of a simple-majority rule in the legislatures for the adoption of spending proposals. Like Wicksell, and Buchanan and Tullock, Niskanen recognizes the difficulties of achieving unanimity or even near-unanimity on fiscal policies and, accordingly, proposes a two-thirds rule. Since he advocates the contraction of governmental activity as a paramount goal, the deadlocks such a rule entails are viewed as beneficial or a small price to pay for so valued an end. Niskanen also advances some novel and, perhaps, unexpected views and reforms of the tax system. He thinks that a progressive tax structure is preferrable to the current proportional system at least under certain specified conditions pertaining to the existence of bureaucratic competition, review boards, and majority rule. The size of government and the allocation of the tax burden is vitally affected by the varying combinations of decision rule, the existence of a review and the prevalence of majority rule. Space does not permit a detailed examination of these permutations.

Niskanen does not assess the political feasibility of his proposals. One can, however, suggest that most of them pose such serious threats to the existing power structure that they will not likely be adopted. The voters' recent overwhelming defeat of Governor Reagan's fiscal proposal—a proposal which Niskanen not only supported but helped to formulate—hardly bodes well for those who want a more limited government.

One does not always expect "wisdom" in more or less technical expositions, but somehow Niskanen provides readers and especially classroom teachers with some highly quotable observations. Two that appeal to me and that I have already included in my lectures are the claim that ". . . the most dangerous of all politicians is one who is indifferent to his own reelection or the future of his party . . . " (p. 137), and the play on Adam Smith—"There is an 'invisible hand' in government but it is a helping hand for some, a barely acceptable appendage to many, and a mailed fist for others" (p. 137).

It is obvious that my admiration for Niskanen's book is virtually boundless.

WILLIAM C. MITCHELL
University of Oregon

Fiscal Federalism. By Wallace E. Oates. (New York: Harcourt, Brace, Jovanovich, 1972. Pp. 256. $7.95.)

One of the reputed values, if not virtues, of economics is its explicitness in the face of a world of ambiguity. Professor Oates is an economist, and, consistent with one canon of his discipline, he is indeed explicit on at least two fronts, the purposes of his book and the character of the more discrete questions he confronts.

Fiscal Federalism "explores what economic theory implies about the division of fiscal functions among levels of government, and examines the extent to which such theoretical structure can explain the organization and workings of the public sectors of different countries" (p. vi). This statement accurately conveys the focus of the book as dealing with: (1) formal theory, (2) empirical fiscal practices, (3) comparison in a cross-national sense, and (4) positive rather than perjorative analysis. These are commendable features of a highly commendable book.

Oates offers an economic definition of federalism that is latitudinarian by comparison with ones offered by power-oriented political scientists. His definition is (p. 17):

> A public sector with both centralized and decentralized levels of decision-making in which choices made at each level concerning the provision of public services are determined largely by the demands for these services of the residents of (and perhaps others who carry on activities in) the respective jurisdiction.

The disciplinary differences are evident when Oates's empirical analysis of economic federalism includes fifty-eight nations. Contrast this with the more restrictive criteria of political scientists in which four, ten, or perhaps sixteen nations are acknowledged as federal systems.

These disciplinary contrasts are offered as descriptive and contextual observations, not as critical and limiting conditions. On the contrary, Oates's broad, empirical, and analytical approaches are uniform assets rather than deterrents to the understanding of important intergovernmental fiscal questions. These questions include:

(1) Which level of government is best suited to provide a particular public service?
(2) What are the justifications and optimal arrangements for using intergovernmental grants?
(3) What general and theoretical principles serve as guides in the design of a system of taxation under a federal arrangement?
(4) How are economic federal systems actually organized and what cross-national similarities and differences exist in intergovernmental fiscal relations?
(5) What are the dynamics of fiscal federalism, i.e.,

> what trends are present in the degree of fiscal centralization or decentralization over time?

Enough has been said to highlight the substantive questions and empirical focus of this volume. It has the additional and unusual feature of separating the sometimes involved and technical aspects of empirical analysis from the body of the text. This is accomplished by using appendices for each chapter and allowing the author less encumbered substantive exposition.

This book is more thoughtful than thought-provoking. It relies more heavily on clear, incisive analysis than on new or original insights. Its normative, position, which comes out favorable to fiscal federalism, is tempered and formal rather than burning and ideological. The book seems well suited for advanced undergraduate and graduate courses in federalism, public finance, and perhaps public policy. It does not map new avenues for the analysis or directional development of federal systems. But it does provide a much better understanding of the routes such systems have followed to date and some signposts at important forks in the roads.

DEIL S. WRIGHT
University of North Carolina at Chapel Hill

Reforming School Finance. By Robert D. Reischauer and Robert W. Hartman, with the assistance of Daniel J. Sullivan. (Washington, D.C.: Brookings Institution, 1973. Pp. xiv, 185. $6.95.)

This book is an attempt by three policy-minded economists to crystallize some issues in the national debate about school finance that bubbled up in the period from 1970 to 1972.

The introduction describes three reasons why national attention suddenly turned to the problem of financing elementary and secondary schools. First was a "taxpayers' revolt": for example, the percentage of school bond issues approved around the country began to decline after 1965 and fell sharply in 1969. Second was a series of court decisions that began with the California Supreme Court's ruling in *Serrano v. Priest*, in August, 1971. This and a string of subsequent rulings in other state and federal courts held that the present system of financing public schools through local taxes violated the federal Constitution and some state constitutions as well. Third, the decline in nonpublic school enrollment after 1965 threatened to put further financial pressure on the public schools.

A chapter full of numbers describing disparities in the present system of school finance in the U.S. is followed by a major chapter on "The General Fiscal Dilemma." Here the authors seek "to resolve whether a genuine crisis is developing or whether the problem is of a different sort: while we all want better schools, we are not certain that more money will improve the schools, nor do we like the taxes we now pay to support education" (p. 17). The authors apparently believe there is no real emergency. Total outlays for public schools more than tripled between 1957–58 and 1971–72, in current dollars. About one-fourth of this increase was due to growth in enrollment, which has now petered out as the last babies of the postwar boom are finishing high school. Approximately forty per cent of the increase in expenditures resulted from rising teachers' salaries, but the teacher shortage of the 1960s has now become a surplus. Property tax rates have risen, but other state and local taxes have grown faster than the property tax. And although the property tax is often poorly administered and inequitable, the authors, in their lucid and scholarly discussion of this complex question,conclude that the property tax is probably less regressive than it is reputed to be. Furthermore, what makes differences in the amount of taxable property between localities unfair is that low-income people are prevented from moving to certain wealthy localities—which suggests to this reader that an end to restrictive zoning would be a stronger remedy for injustice in local school finance than would general reduction of property taxes.

Although the pressures on public school finance will subside in the 1970s—and although reports of "crisis" were probably exaggerated in the first place—the authors do consider various proposals for reform. One strategy would aim at reducing inequities in the local property tax. The authors summarize a number of proposals, and include a novel suggestion of their own for deferred payment of property taxes, which would allow elderly homeowners with low incomes to keep their homes without any public subsidy (p. 40). Another strategy would replace some local property tax revenues with other local taxes. Finally, a third strategy would increase the federal and state shares (presently about seven per cent and forty per cent, respectively) of the cost of public schools.

This third strategy for reform is the subject of an entire chapter, which raises a set of

> basic questions that will have to be answered in designing a more equitable school finance system: How high should the basic state educational program be set? Will the differing educational needs and costs of various districts be taken into account? Should local districts be allowed to supplement the basic state program, and if so by how much? If local districts are allowed to supplement the state program, will the state make adjustments for the varying abilities of

school districts to raise the supplemental revenues? Finally, what kinds of taxes should states use for their increased share, and how should districts' ability to pay be measured? (p. 93).

The authors describe important trade-offs that arise in dealing with these questions. For example, if a given amount of state aid to local school districts produces more equalization of expenditures per pupil, then it will provide less local tax relief, assuming that low-spending districts are to be "leveled up." If, instead, high-spending districts were "leveled down," then wealthier residents might have to resort to private schools to obtain the quality of education they want for their children.

While one desideratum for state and federal aid to public schools is to avoid driving high-income groups into private schools, a frequently stated objective of aid to private schools is to avoid forcing their students into public schools. But the authors conclude that public subsidies of realistic magnitude would not have much effect on stemming the decline of enrollment in parochial schools. The only argument for aid to parochial schools in which they see much merit is that expanding private schools in general might provide more choice among educational styles and philosophies. This is really an argument for a full-fledged voucher system, which would let parents use tax revenues to pay for their children's education in any approved school, public or private. A voucher plan might also be the most effective way to equalize resources available for schooling among families of varying means, because any other scheme to redistribute resources could be undermined by high-income families sending their children to private schools. It is doubtful, however, that a voucher plan would be constitutional if it included parochial schools.

Reforming School Finance does not make firm recommendations of its own. Instead, it describes school finance as a complex distributional game, showing who would win or lose from various proposals, and why the outcome is sometimes uncertain. The authors also choose not to deal with the prior question: Why is school finance really worth worrying about? Now that the U.S. Supreme Court has overturned *Rodriguez v. San Antonio*, declining school enrollments in the 1970s will reduce the pressure for reform of any kind. Unless state constitutions provide a basis for action through the courts, no significant equalization of resources for education is likely to come about until there is widespread agreement that school children *can* benefit from more resources, and that *all* children have a claim to these benefits. But this book does not try to articulate the constructive vision of education which might inspire a major reform of school finance.

DAVID STERN

Yale University

The New Deal and the Last Hurrah: Pittsburgh Machine Politics. By Bruce M. Stave. (Pittsburgh, Pa.: University of Pittsburgh Press, 1970. Pp. 262. $8.95.)

The main theme of this book is its contention that whereas traditional opinion has held that the New Deal "assisted in destroying America's urban political machines. . . . the origins of the Pittsburgh Democratic machine . . . were rooted deeply in the New Deal; rather than enfeebling machine politics in Pittsburgh, the New Deal invigorated a previously impotent Democratic organization" (p. ix).

Professor Stave uses three methods to investigate the effect of the New Deal on Pittsburgh's machine politics: (1) interviewing 103 Democratic committeemen who served during the 1930s; (2) comparing names of committeemen listed in election return books with listings of the same individuals and their occupations in city directories; and (3) comparing the demographic findings obtained from the interviews of New Deal committeemen with similar data gathered from an investigation of Pittsburgh's precinct officials in 1954 (William J. Keefe and William C. Seyler, "Precinct Politicians in Pittsburgh," *Social Science*, 36 [January 1960], 26–32).

The interviews with 103 Democratic committeemen revealed that one-third were on the work-relief rolls at some point during the depression decade, and all but four of them served with the Works Progress Administration (WPA). A majority of those who served with the WPA were foremen or supervisors. Several of the committeemen used their WPA work as the "first rung on a ladder of public employment" (p. 166). From this and related data, Stave concludes that the interviews "emphasize the connection between WPA and grass-roots politics during the depression decade" (p. 182).

The comparison of the election-return books with the city directories showed that by 1962, 78 per cent of Pittsburgh's Democratic committeemen held a job with city, county, state or federal government. A study of twenty-two of Pittsburgh's thirty-two Democratic ward chairmen in 1960 indicated that twenty (91 per cent) earned their livelihood by means of a government position. By 1962, nearly 60 per cent of the Democratic committeemen who held public jobs were on the City payroll, as contrasted to the New Deal years when such jobs were more evenly distributed among the several levels of public ser-

vice. Professor Stave concludes that this comparison "highlights the shift from private to public employment by Pittsburgh Democratic Committeemen during the era and illuminates the organization's use of the payroll for the express purpose of building a machine" (p. 182).

A comparison of the findings relating to Democratic committeemen who served during the 1930s with those of a similar study undertaken in Pittsburgh during 1954 showed the following: (1) of the New Deal committeemen in the study, 57 per cent were under forty years of age as opposed to 20 per cent in 1954; 89 per cent were under fifty years of age during the New Deal's beginning in Pittsburgh, whereas 56 per cent were younger than fifty years of age two decades later; (2) whereas 40 per cent of the New Deal committeemen graduated from high school or went on to further schooling, 48 per cent of the 1954 committeemen did the same; (3) in both the 1930s and 1954, Roman Catholics predominated in the party; (4) in both the 1930s and in 1954, "the politically inclined Irish and the Germans [were] the two leading ethnic groups "involved in Democratic organization politics" (pp. 178–179); (5) there were two and a half times as many foreign-born committeemen in 1934 as there were in 1954. From the above and related data, Stave concludes that "the demographic data shows there was little significant change in the social backgrounds of party personnel who served before and after the age of Roosevelt—it implies continuity rather than change in the Democratic politics of pre-and-post New Deal Pittsburgh" (p. 182).

For this reviewer, the contribution of *The New Deal and the Last Hurrah* comes not so much from its efforts to persuade that "Pittsburgh Machine Politics" were somehow unique in being New-Deal-supported instead of New Deal-enfeebled; rather, the value of this book lies in its vignettes of the colorful characters who peopled the Pittsburgh political scene: the zany Mayor William N. McNair (keeping his campaign promise by putting his desk in the lobby of the City-County Building so that the public would have free access to their chief executive; promising a group of ministers that if they would recommend a candidate for director of public safety, a department abounding in patronage, he would give him favorable consideration; hosting a local vaudeville show for a week and appearing on radio as guest on Major Bowe's amateur show); the jubilant David L. Lawrence, wiring FDR on October 6, 1936—"Some more Roosevelt luck. Mayor McNair resigned and Cornelius D. Scully sworn in to fill vacancy by City Council"; the irrepressible Mrs. R. Templeton Smith, head of the local League of Women Voters (this reviewer attended a City of Pittsburgh Budget hearing in the late 1940's when Mrs. Smith said that when her curiosity was aroused by a very large sum being budgeted for the salaries of painters—and only a minimal amount being budgeted for the purchase of paint—she sought out the head of the division of painting who explained the apparent paradox—"Lady, paint don't vote!").

WILLIAM C. SEYLER
Temple University

The Supreme Court and Confessions of Guilt. By Otis H. Stephens, Jr. (Knoxville: The University of Tennessee Press, 1973. Pp. 236. $9.50.)

Supreme Court extensions of constitutional protections to pre-Court actors and processes have accelerated during the "Warren Court era." Issues such as search and seizure, self-incrimination, right to counsel, and exaction of confessions have been defined as within the judicial compass of the Fourth, Fifth, and Sixth Amendments. Each amendment has been relied upon, in varying degrees, to serve as a center point from which the compass arc has swung to protect confessions of guilt in federal courts, or in state courts via the Fourteenth Amendment. To draw police activity within judicial constraints has a logic which, despite cries of "handcuffing the police," the Burger Court may have difficulty disavowing.

In *The Supreme Court and Confessions of Guilt*, Otis Stephens has examined the search for standards to explain that logic in the particular area of confessions. His examination, given the variability of treatment accorded confessions, doubts neither the desirability nor the logic of constitutional protection. Yet the development of a judicial explanation, the interlocking of the various constitutional provisions, and the study of the impact of Supreme Court decisions on police in the area of confessions *could* provide the Court with potential avenues for retreat. Impact studies, as Stephens shows, will not give much support for retrenchment. Thus, any Burger Court modifications in the area of confessions can be located within the Court's development and interlocking of the changing constitutional justifications which Stephens has carefully examined.

We learn that the treatment of confessions—first from fragile Common Law roots dependent on judgments of "trustworthiness" and then grafted onto stronger due process stock, often with the intentional hybridization of search and seizure, self-incrimination, and right to counsel arguments—has been a slow process. Unwillingness by way of "incorporation" to extend applicability to local actions, perceived threats to "law and order" and to the independent professionalism of police, fears of "floodgates" (first of

criminals back to the streets, and then of litigious habitual criminals on frivolous grounds up through the courts), and self-delusions that civil remedies would still correct and reverse police wrongs—all had served to delay early Supreme Court intervention.

Now, the Common Law "right" to be free from self-incrimination was no right at all. (In Hohfeld's terminology, it would be an "immunity," since the state disabled one from confuting God's judgment through trial by ordeal. That evidence and verdict necessarily became the law and the truth. To call it a "right" would be to misstate the purpose behind enforced non-self-confession.) It is hardly surprising that Stephens finds a similarly conservative Common Law cast given confessions of guilt prior to the turn of the century. He cites the *Bram* case (1897) as the only early foray from Common Law justification into more broadly based and constitutionally prescribed protections against self-incrimination.

Retreat from this early, if inchoate and muddled, reliance on broader constitutional language for protection against wrongful extractions of guilt was assured by 1908 when, in *Twining*, self-incrimination arguments were held beyond Fourteenth Amendment reach. Retreat was made to case-by-case evaluations, depending on the trustworthiness of the litigant.

When next the Court went searching for a broader and constitutionally grounded rule, it came to rely on "fair trial reasoning. No longer right or wrong depending on the "cut of his jib," but right or wrong depending on what shocks a common man's sense of decency, the confession was judged admissible or not. Stephens reminds the reader that the atrocities toward Negroes and exposes such as those contained in the Wickersham Commission findings of the 1930s demanded a firmer judicialization of confessions.

Judicial "fair trial" standards for confessions, based on due process, weakened, both for want of precision and because of increasing agitation from the more conservative wing of the 1940s Court. Instead of a return to "trustworthiness" standards, Stephens suggests the *McNabb-Mallory* rule accomplished judicial supervision of confessions in federal courts. Thus administrative, rather than either Common Law or constitutional due process, grounds became preferred. Fears of such administrative scrutiny of grass roots policing raised immediate hackles in Congress. These outcries, plus cold war fears and the losses of liberal Justices Murphy and Rutledge, tolled further judicial activism in the area of confessions. Not until the advent of Justices Warren and Brennan, and the ensuing 1957 *Mallory* reassertion of supervisory solicitude, did the Court again deal actively with confessions. Once more, Stephens observes, the congressional hackles against Court coddling of criminals rose in an anguished chorus, now led by Senator Ervin. (The "law and order" leitmotif of that chorus did indeed prove seductive in the 1968 and 1972 presidential campaigns.)

By the time *Mapp* (1961) had pushed the Fourth Amendment guarantees to the states, *Escobedo* (1964) had expanded admissibility from a matter of case-by-case judgment to one of an absolute constitutional right, and *Miranda* (1966) had replaced circumstantial variables with broad constitutional commands, all these variegated and interlocking protections extended judicial theory into policing practice.

Stephens devotes considerable attention to the impact of these holdings upon law enforcement. All the fears of "judicial handcuffing" of the police find little verification, as Stephens's review of this literature shows. Yet, despite the good tidings that the Warren Court did not immobilize policing functions, Stephens has prepared us for the realization that these protective integuments are again being pulled away. The beginning of the retrenchment has already been signaled by a triad of Burger Court cases in 1970, by the Court's greater willingness to overlook negotiated pleas of guilt as a variant of inadmissibly extracted confessions, and by the *Harris* decision (1971) (which upheld use of information to impeach *testimony* though stricter Miranda rules had proscribed *any* use—against a person *or* his testimony). Stephens has defined the protective petals surrounding litigants in the area of confessions though perhaps greater emphasis might have been given plea bargaining. Do such bargains "build in" their own due process, albeit at a pre-judicial level? Or is bargaining a derogation to some degree of the very protections against inadmissible confessions of guilt Stephens has so thoroughly and admirably examined?

DAVID CHADWICK-BROWN
Alameda County Calif.

The Contexts of Social Mobility: Ideology and Theory. By Anselm L. Strauss. (Chicago: Aldine Publishing Co., 1971. Pp. xiv, 263. $8.50.)

The book is organized into three parts. Part I develops a very brief, highly selective critique of research and theory on American mobility. Part II directs attention to traditional and contemporary ideologies (or images) about American mobility, drawing principally on the writings of foreign commentators on the American scene, such as the ever popular de Tocqueville, historians, social commentators and novelists; and Part III proposes a partial "theory" of mobility that focuses on important, but, in Strauss's judgment,

generally neglected aspects of mobility. As a social psychologist and social historian, Professor Strauss has come to believe that contemporary, especially quantitative approaches to the study of mobility have slighted the psychological and historical contexts of mobility and have been especially insensitive to their own ideological preconceptions. In the hope of "transcending" these alleged deficiencies, Strauss first examines the "continuing public conversation about opportunities, equality, inequality, success, failure, mobility . . . along with the manifold cultural and institutional contexts within which the conversation has occurred" (pp. xii–xiii) on the grounds that it is not merely conversation but "precipitates individual, collective, and corporate action" (p. xiii). Borrowing from his general theory of "status passage" previously developed with Barney G. Glaser (*Status Passage* [Chicago: Aldine Publishing Co., 1971]), he then examines "not only . . . the direction of mobility (up-down) and . . . distances of rise and fall, but also . . . other important issues such as: the rate and timing of mobility, the degree of commitment to mobility goals, the variety of tactics employed in mobility actions, the contests for control among interested parties involved, the personal meanings of mobility, and the articulation of mobility concerns with other concerns" (p. xiii). To accomplish these ambitious objectives, he employs a "special methodology of comparative analysis" and "theoretical sampling" (as opposed to statistical sampling) in the development of a "grounded theory" of mobility (see Barney Glaser and Anselm Strauss, *The Discovery of Grounded Theory* [Chicago: Aldine Publishing Co., 1967]).

How successful is Strauss in achieving these various objectives? Strauss has generally been regarded as having made substantial contributions to the sociological enterprise, both in enriching our substantive understanding of such fascinating and important phenomena as the sociology of dying and also in broadening the range of methodological tactics in the development of theory. I was, however, very disappointed with this book which simply failed, in my judgment, to live up to its author's reputation.

While counting myself a member of the "quantitative camp" Strauss stigmatizes in the first part, I share somewhat his sense of the excessive limitations this group of researchers has imposed on questions about mobility in America. I was shocked, however, by the highly selective, excessively brief (a mere fourteen pages constitutes Part I), and dated portrayal of their work. There is surely something wrong with a critique and virtual dismissal of a tradition of research and theory that fails even to *mention* the classic statement of its position (Peter M. Blau and Otis Dudley Duncan, *The American Occupational Structure* [New York: John Wiley and Sons, 1967]), let alone detail its inadequacies systematically. The reader uninitiated in the contemporary developments in the field of mobility studies and the status attainment process (one of the most active and robust in contemporary sociology) would come away ill-informed and with a highly distorted notion of what is going on. The initiated reader would simply be left gasping.

We are informed that the analytic scheme of Part II is built around several sociological variables which Strauss admits sound more like labels for complex historical events or processes but which he hastens to reassure us "function as variables" (p. 15). This reassurance is, unfortunately, not convincingly demonstrated by his subsequent discussion. Two sets of variables (processual and ideational) and their interrelations are linked to the conditions for, and the behavioral and institutional consequences of, perspectives on mobility. The processual set of variables includes five major variables: the settling of the *frontier*, the peopling and building of the *countryside*, and the processes of *urbanization*, *industrialization*, and *immigration*. The ideational set of variables consists of three persistent American themes or imageries: the celebration of America as an *agricultural nation*, as the *land of destiny*, and as the *bearer and creator of civilization*. A chapter is devoted to each topic. The general format of each chapter is simply to summarize briefly the comments of selected observers on the "variable" in question, being careful to juxtapose conflicting views. Anyone reasonably familiar with American social history and historical and contemporary social commentators and novelists will encounter few surprises or new insights in this long and remarkably unprovocative middle section of the book. The compilation of authors and points of view is reasonably comprehensive, but the reader is left with the task of teasing out for himself most of the implications of the discussion.

Part III, by far the most successful section of the book, contains a number of provocative discussions about specific aspects of the mobility process sometimes neglected in contemporary research. To label this discussion a "theory of mobility" as Strauss does is to misuse the term, since it is not much more than an interesting shopping list of things one should probably consider in describing aspects of the mobility process for individuals and groups. Strauss assumes too much familiarity on the reader's part with his previous methodological and theoretical discussions and, as a result, does not adequately develop key definitions, assumptions, and rationales.

In sum, I certainly could not recommend this book as an adequate critical introduction to

sociological work on mobility in America nor as an especially fresh or novel perspective on the topic more generally. Its value may lie in bringing into one place a synopsis of a variety of more qualitatively oriented discussions of mobility by historians and social observers, past and present. Drawing out the political implications of American ideologies about mobility and inequality is not an expressed goal of the book; but Strauss does provide relevant materials and observations for those interested in such questions.

EDWARD O. LAUMANN
University of Chicago

The Prevention of Racial Discrimination in Britain. Edited by Simon Abbott. (London: Oxford University Press for the Institute of Race Relations, 1971. Pp. xviii, 502. £ 5.00.)

The Politics of the Powerless: A Study of the Campaign Against Discrimination. By Benjamin W. Heineman, Jr. (London: Oxford University Press for the Institute of Race Relations, 1972. Pp. xvi, 244. £ 4.30.)

Race and Law in Great Britain. By Anthony Lester and Geoffrey Bindman. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 491. $10.00.)

The surge of publications in England about British race relations has begun to match the occasional but intense attention immigration and race have received in British politics since the mid-1960s. Together with Rose (*Colour and Citizenship*), Rex and Moore (*Race, Community, and Conflict*), and Lambert, (*Crime, Police, and Race Relations*) these volumes constitute a shelf sufficient to acquaint those concerned with the starkly similar yet markedly different nature of the race "problem" in Britain and the United States.

The least substantial of the volumes, although in many ways the most comprehensive is Abbott, ed., *The Prevention of Racial Discrimination in Britain.* The study was commissioned by the United Nations Institute for Training and Research at a time when research about race relations as an issue of public policy in Britain was just beginning to emerge from the reportorial stage. The essays fall into three sections: "The Immigrants and the Receiving Society," which includes historical background as well as recent demographic data; "The Process of Racial Discrimination" in employment, housing, and education; and, "The Prevention of Racial Discrimination," which describes British anti-discrimination legislation and its administrative apparatus. Complaints under the two legislative enactments, the mild Race Relations Act of 1965 and the more comprehensive Race Relations Act of 1968, are administered by a Race Relations Board, whose activities are described, and a Community Relations Commission, whose vague charge is to coordinate and give advice on community relations in the interest of community harmony. For want of data—the British did not then and still do not gather census information on the basis of race—and a predominance of description rather then analysis, the essays vary widely in value. Simon Abbott's concluding essay is analytically provocative; the essays by Brian Cohen, Nicholas Deakin, Louis Kushnick, and Bob Hepple stand out among the others. Overall it is a useful survey if one allows for the fact that the essays were written when the state of the research art in this field was just beginning to mature and that events were rapidly outdistancing the data and many of the conclusions.

The Heineman volume is a model of its kind. It is a case study of the rise and fall of a civil rights pressure group, The Campaign Against Racial Discrimination. Between 1964 and early 1967 C.A.R.D. was the only organization in Britain dedicated to the formation of an alliance between immigrant organizations and liberal activists. Heineman combines sensitivity to the complexity of his subject with knowledge of interest group analysis and its limited applicability to the politics of the powerless and the tensions which tore C.A.R.D. apart. A fragile consensus, based initially on civil rights goals reminiscent of the United States in the mid-1960s, was shattered when preferences for establishmentarian solutions came in conflict with a pseudo-Marxist-Leninism. The ideology shifted unpredictably, depending on personal preferences and the needs of the moment, from revolutionary regeneration of British society and the Third World to black separatism. C.A.R.D. could not resolve these differences, nor could it resolve the tension between the ambitious plans for a grassroots movement in Britain and the need to influence governmental policy in London. The leadership core, numbering hardly more than fifty, was split on personal, ideological, and racial grounds from the outset. The membership ambitiously estimated at 3000, was as divided as the leadership. The liberals in C.A.R.D. had access to the government, particularly to the Home Secretary and prime mover of the 1968 legislation, Roy Jenkins. Their more militant counterparts, as Heineman points out, represented only minuscule segments of the immigrant "community" which itself defied clear definition. Geographical dispersion was intensified by class differences, differing generational goals, varying degrees of political consciousness and Britishness, and, not the least, the mutual suspicions within and among Indian, West Indian, and Pakistani groups. Heineman concludes that

the failure to aggregate such a diverse assemblage, split as it was by irreconcilable leadership styles and ambitions, rendered C.A.R.D. as powerless as its constituency. C.A.R.D.'s only legacy, beyond the legacy of failure, was experience in the tribulations of interracial organization and, possibly, an increment of awareness in Whitehall and Parliament about the need for more extensive antidiscrimination legislation. The liberal survivors turned to lobby for that legislation.

Race and Law in Great Britain is co-authored by one of the founders of C.A.R.D., Anthony Lester. His skill, together with Geoffrey Bindman's knowledge of the field, have produced a study of imposing scholarly quality. Lester is one of a handful of race relations professionals within the British bar. His expertise is enlightened by a Harvard Law School education and by on-the-scene observations of civil rights conflict in the United States in 1964. Bindman, a solicitor, is a legal advisor to the Race Relations Board. Both were active in forming the legislation they discuss. Their official involvement in no way diminishes their commentary, although evaluations of the performance of some persons involved in the attenuated network of the British race relations enterprise tends to be muted.

The study is, in part, a legal treatise which tracks the content and the reach of the Race Relations Act of 1968. Fortunately, it is also more. It probes what British readers in particular need to probe, namely, whether legislation against racial discrimination has any significant role to play. The debates in both parties about the desirability of the 1968 Act were filigreed with much the same rhetoric as when legislation was being contemplated in the United States during the Eisenhower years. The myth that law can have no effect, or should have no effect, is much alive in Great Britain. This fact accounts for the authors' careful rejoinder to a widely publicized lecture by an eminent jurist, Lord Radcliffe, some months after the passage of the Race Relations Act. He expressed doubt that racial discrimination was sufficiently condemned in British society (a premise about which British polls provide no real answer) to warrant prohibition by law. He reasoned that prejudice and discrimination in themselves do not necessarily carry associations of "moral ill-doing" and because law should extend only to matters in which the moral issue is viewed as beyond debate, legal intervention is questionable. Radcliffe's attitude is symbolic of a mood that affected the passage of the Act and that has had an impact on its administration. Where the issue has reached the courts the attitude has manifested itself in judicial literalism. The most recent decision by the Law Lords in a private club case (*Charter v. Race Relations Board* [1973]) may at first seem a distant echo of the Supreme Court's *Moose Lodge v. Irvis* [1972]. But the differences are more significant than the similarities; while *Moose Lodge* searches for the outer perimeters of what law may reach when state action under the Fourteenth Amendment is in question, *Charter* and other British cases search for what the statute read literally might in its own words seek not to encompass. As Lester and Bindman suggest, the Radcliffe view forgets the broader purposes of law and public policy. The judicial decisions, it can be argued, tend to mask a preference for traditional social patterns and common-law values and a narrow view of the judicial function as well as the function of law.

It is, then, all the more remarkable that the legislation has both substance and bite or, in fact, that it was passed at all. A relevant commitment to legislation was stirred by the accumulated though hesitant promises by the Labor Party that justice required concrete action by Parliament. More significant, however, was the realization that discrimination which immigrant parents might tolerate would not be passively accepted by the new generation of their native-born children soon to emerge from British schools. In the final weeks before passage, moreover, the Labor Party found it difficult not to match the severe restrictiveness of its own Commonwealth Immigrants Act of 1968 with some symbolic assurance that those in residence in Britain should be free of racial discrimination. Lester and Bindman set out lucidly the legislative history of the Act. Just as effectively they analyze the scope of the legislation and its administration by the Race Relations Board in housing, goods and services, and employment. Significantly, the latter was to be dealt with initially by industrial machinery after the government yielded to the combined pressure of the TUC and CBI. Some of the exemptions sound familiar: exceptions for those employing small numbers of persons, and a Mrs. Murphy clause, for example. Some do not: there is no prohibition against religious discrimination, racial "balance" in industry can be applied against those not principally educated in Britain, and discrimination in assigning berths to passengers or employees is allowed on British ships. By the time the next edition of this volume is published, Parliament may have repaired serious deficiencies in the enforcement procedures including grants of power to the Race Relations Board to initiate investigations without depending on specific complaints and the use of restraining orders and compulsory processes to obtain documentation. Such a turn from voluntarism is unlikely in the foreseeable future, however. These defects aside, and given the British setting, it is easy to agree with the authors' conclusion that the legislation is at once a sig-

nificant step forward in the law, and at last, recognition by the political system that a problem is at hand.

I find two deficiencies in this excellent study. First, although there is every reason to suppose that the authors are aware of it, they fail to emphasize the insufficient experience of race relations practitioners in Britain and the lack of purposeful recruitment of race relations professionals into government, the social services, the bench, and the bar. The deficiency is owing in part to the character of legal education in England and, in other sectors, the cult of the generalist. It is also a product of the social worker mentality. Civil rights professionals are rarities and the deficiency merits discussion. Second, I take exception to their conclusion that Section 6 of the otherwise innocuous Race Relations Act of 1965 prohibiting incitement to racial hatred (now by implication an integral part of the 1968 Act) serves any useful purpose. The authors appear to admit this and, in fact, seem to conclude that its impact has been negative. Yet they proceed to argue, unpersuasively, that the section should be extended, not repealed.

Race and Law in Great Britain will not be replaced as the standard volume interpreting legislation in this field. It will only need updating. It should be noted that this study, like the others, explicitly recognizes that there must be a change in the political atmosphere in Britain which encourages professionals, governmental officials in London and elsewhere, and established interests in the private realm, to become more profoundly alert to the needs of the new minorities in the allocation of privileges and benefits. Noblesse oblige or government by technicians will move matters only inconclusively. Minority power must develop. The burden of the Heineman volume is that interest must be harnessed to power. For the one-time immigrant, now citizen, this can take a variety of forms, or none at all.

RICHARD P. LONGAKER
University of California, Los Angeles

South Africa: Sociological Perspectives. Edited by Heribert Adam. (London and New York: Oxford University Press, 1971. Pp. vii, 340. $11.25.)

Dr. Adam begins his introduction by stating: "There is no paucity of publications on South Africa. Most of them, however, amount to little more than a polemical denouncement of the Apartheid system. Instead of socio-economic analyses, confessions are offered as to how unprejudiced the authors are" (p. vii).

Not true of this perceptive book. All fifteen contributing authors are critical of racial discrimination in South Africa. By and large, however, they do not substitute rhetoric for reason. To understand this volume's importance in the initiation of a new trend in social science analysis of racial tension in South Africa, it must be viewed within the context of a convergence of political thought by the three white political parties in South Africa. At the same time, there is also a convergence of political views of the whites and the largely disenfranchised majority.

A decade ago the Progressive party, favoring the franchise for all citizens who had a primary education, labored long to produce a federal plan for South Africa. The United Party has moved to a federal concept with some powers held by a multiracial body superior to the present all-white Parliament. At its 1973 Transvaal Congress, the "Young Turks," led by Harry Schwartz, for the first time invited black leaders to address them, and then applauded the concept of shared power. Although the National Party is not officially committed to federation, prominent intellectuals and key politicians now talk of the inevitability of a confederation or commonwealth.

On the other hand, the Bantustans, ridiculed by the white opposition parties and initially opposed by almost all African leaders, have become a dynamic force in politics. Alan Paton now welcomes the platform which Bantustans have given to the new generation of African leaders, such as Buthelezi, Mangope, Ntsanwisi, and Phatudi. The Progressives changed their concepts of federation to embrace the political realities of Bantustans. Criticism from Kaiser Matanzima forced the United Party to abandon the palliative notion that eight black M.P.s in the present Parliament could mean justice.

Although urban Africans dislike Bantustans, they do view various federal plans with some favor. Buthelezi's personal popularity in Soweto (black Johannesburg) stems mainly from his championing of the rights of all black men and women in South Africa.

When the Bantustan leaders talk of consolidating all the "Homelands," as Matanzima and Buthelezi have done, or of joining his Bophutha-Tswana with independent Botswana, as Mangope has done, they have introduced another political variable. The African labor strikes in Natal, the pressures on American companies to pay equal wages to Africans, and the campaign in Great Britain to shame British companies into better labor practices in South Africa have been followed by sharp increases in African wages.

Dr. Adam, formerly of the Frankfurt Institute of Social Research and currently at Simon Fraser University in Vancouver, has, therefore, produced this book at a propitious time. It not only perceives changes, but it possibly contributes to new ones. In his own essay, he does not envision

sweeping changes in the power structure, but he does examine the prospects for change within the white oligarchy, specifically the Afrikaner. In a series of valuable tables on white attitudes, Adam paints a picture of paternalism toward Africans and a wide variety of racist attitudes. Although he does not hold out great hope for a smooth transition to a multiracial society, he concludes that "the flexibility of the South African power elite to adapt its system of dominance to changing conditions, to strengthen it economically, and to streamline it politically by concessions to deracialization, should not be underestimated" (p. 101).

Jordan Ngubane, former Vice President of the Liberal Party, points to the syncretic culture of black South Africans which has evolved from the meeting of the Hottentots, Xhosa, and Sotho-speaking people with the "whites." Countering this fusion are the efforts of the authorities to "retribalize" such Africans. Ngubane is only occasionally optimistic in his opinion of the future, but he calls for a new approach to a negotiated settlement: "Black and white in South Africa need to be pushed towards an arrangement which will enable them to cool their tempers and in that way pave the road to a purposeful and constructive dialogue . . . " (p. 21).

Gwendolen Carter's 1965 lecture on the distinctiveness of African nationalism in South Africa turns out to have been most prescient. She is concerned with the narrowness of white exclusivity because it would produce a black exclusivism. She predicts, albeit reluctantly, the current surge of black power, especially in student organizations.

Fatima Meer, whose home is in Natal, looks at factors inhibiting African nationalism: Why, she asks on p. 121, if a group includes 80 percent of the people and 70 percent of the workers, doesn't it bring about better conditions for itself? Dr. Meer sees the 1950s as the heyday of the African National Congress. She makes the shrewd point that "passive resistance is doomed to failure when the stakes are equally crucial to both sides, and the governing party believes that to yield is to lose all" (p. 146). She tells us that in the 1960s, organized efforts at violence failed, in part because "saboteurs approached their task with the temperament of martyrs, rather than as terrorists . . . " (p. 148). However, Dr. Meer had previously observed "In the Transkei, the impossible seems to have happened and freedom appears to be offered on a platter" (p. 123). She cites the seductive case of Verulam, where "for the first time in their history Indians run the affairs of a whole town . . . [and] see this as real power" (pp. 122–23).

Philip Mayer presents an excellent study of religion and social control in the African part of East London, in which he analyzes the "Bhenguist" and the "Red Migrant" from African rural areas. In a situation of change, doubt, and insecurity, Mayer posits a choice between control by a group or control by individuals. Rigid, exclusive, total control by some group, Mayer feels, counters the anomie of the individual, but personal choice is lost. He favors leaving decisions to individuals.

The development of "non-white" universities is traced by Kogila Adam. She describes the "repressive conditions" in them, but speculates that "education could become a disintegrative agent for the present South African system, displaying as it does such caste-like features, and lacking an integrative principle in the community of shared societal values" (p. 211).

Editor Adam has included nine additional authors: Richard Ford compares the urban trek in South Africa with that in America; A. S. Mathews examines security laws and social change; H. F. Dickie-Clark is concerned with educational dilemmas in a plural society; J. W. Mann and Ellen Hellmann write in turn on attitudes towards ethnic groups and social change among urban Africans; Pierre van den Berghe describes degrees and kinds of racial segregation; Stanislav Andreski makes a rather superficial comparison of the social order in South Africa with Latin America, the United States, and ancient Sparta; G. V. Doxey uses his extensive knowledge to describe racial stratification in employment; and the volume concludes with the examination by psychologist Kurt Danziger of the modernization and the legitimation of social power.

Dr. Adam has achieved his goal of making a genuine contribution to understanding the political and economic forces emerging in South Africa. The various contributions develop the theme of a rigid political and social order undergoing great pressure for flexibility. The outcome is difficult to foresee.

EDWIN S. MUNGER
California Institute of Technology

The Country Party in New South Wales: A Study of Organisation and Survival. By Don Aitkin. (Canberra: Australian National University Press, 1972. Pp. xiv, 343. $11.70.)

The Australian Country party is not, to say the least of it, everybody's meat. Yet it is an interesting organization and is now at an important, and perhaps critical, stage. Though it has had something in common with farmers' political organizations in North America, the only close parallel is with the Agrarian parties of Scandinavia, especially of Sweden. All these parties were formed at very much the same time, about sixty years ago,

for much the same reasons; that is, a general grievance about neglect of rural interests, brought to a head by the disruption of international marketing by the First World War. All have been threatened by the subsequent decline of the rural vote. The Scandinavian parties met this problem in the 1950s by changing their names to Center parties and seeking support wherever it could be found. The Swedish party has since become by far the largest nonsocialist party, in terms of both votes and seats won.

The Australian Country party has for several years been agonizing about risking a similar gamble. Its leaders cannot agree or even make up their minds about whether to cling to a certain, though shrinking, following as an unashamedly rural party or to risk the loss of its traditional supporters in the hope of gaining many new ones. As Aitkin's sub-title implies, any worthwhile study of the party must largely be based upon the problems which it faced and still faces merely in surviving. And this is a very worthwhile study indeed—on the whole the best book to be written about any Australian political party. This despite the very major limitation that it is concerned with the Country party in only one of the four states in which it operates, although New South Wales is the largest of these states.

For one thing, the book is backed by a much greater body of evidence, from a wider variety of sources, than any other such book. Party officials at all levels have been remarkably helpful in making records of meetings available. The author has himself collected data and sought enlightenment from many local country party organizations over most of the state. He has made some use of data from national sample surveys of voters in 1967 and 1969, of which he was a principal organizer. In addition he has located and used a variety of private papers and other more familiar sources.

From all of this emerges a picture of a party whose strengths and weaknesses are almost equally obvious. The Country party has been able to draw on feelings of country separatism and distrust of the great cities which are both summarized and explained in an excellent chapter, "Rural Society." Yet, in themselves, explanations of this kind would explain altogether too much, since the Country party's support is far from evenly spread. It does not exist in two of the six states of Australia, and even in New South Wales it is not important throughout the countryside. In fact, in the South Coast area, which in some respects has the strongest traditions of geographical isolation, it has never gained any support.

Even within those areas where it can usually win elections, it has its strengths and weaknesses. As Aitkin establishes, its support is more vulnerable in those areas where dairy farming and wheat growing have predominated than in those devoted to sheep grazing. It is, indeed, only the grazing electorates in the north of the state that are really resolute in their support for the party. Moreover, the rural areas are studded with towns, some now declining but others with populations that increasingly tend to outnumber those directly involved in primary industry. And it remains the farmers and graziers themselves, proportionately a diminishing group within regions of diminishing importance, who still control the Country party and make up three-quarters of its membership.

With this rather limited base of truly committed support, the party has to repel the attacks of its rivals, who from time to time seek to carve off its weaker supporters. The Labor party seeks to add the less prosperous farmers and townspeople to the support it can expect from the rural trade unionists. The Liberal party, which has always hoped to eliminate the Country party and to become the one non-Labor party, would be an even more serious threat were it not inhibited by self-interest; its leaders appreciate that any all-out attack on the Country party would greatly benefit the common enemy, Labor. Therefore, the two non-Labor parties generally respect each other's established territory, and quarrel only over the disputed fringes.

Perhaps because of its domination by a traditionally stable and unexcitable rural elite, the Country party has an admirable record of internal tranquility and loyalty to its leaders, which contrasts with the record of both of its larger rivals and makes it, in a necessarily negative phrase, by far the least unpopular party in Australia. But it may be that this can be true only as long as it retains its traditional character; and in that case demographic changes seem almost inevitably to cause its decline. After many years in which, because of its cohesion, it has had much more than its share of influence over Australian government, it could quite suddenly find itself with almost no influence at all. Its leaders are making somewhat hesitant and uncertain responses, which are still very far from removing the threat.

All of this, and much else, emerges from Aitkin's account. At a more general level, this is a good book for those interested in party organisation and operation; in elites, especially rural elites; in the tenuous and shifting nature of party alliances; and even in such apparently marginal questions as the relations between rural pressure groups and governments. It would be of interest whatever the present circumstances facing the Country party. The fact, however, that the party is at present facing the choice between, on the one hand, a hazardous attempt at expansion and, on the other, the prospect of temporary safety but

ultimate decline undoubtedly makes the book the more apposite.

D. W. RAWSON
Australian National University

Aufstand der Jugend? Neue Aspekte der Jugendsoziologie. By Klaus R. Allerbeck and Leopold Rosenmayr. (Munich: Juventa Verlag, 1971. Pp. 268. DM 14.)

Lebensformen Deutscher Jugend, vom Wandervogel zur Popgeneration. By Werner Klose. (Munich: Olzog Verlag, 1970. Pp. 154. DM 14.80.)

Jugendliche und Parteien. By Peter Menacher. (Munich: Olzog Verlag, 1971. Pp. 168. DM 16.)

There is an increasing convergence now of studies of political socialization in various European countries and the United States. Western Germany, too, is producing socialization research in awareness of and frequently following American models so that meaningful comparison becomes possible.

Peter Menacher's book is a rewritten dissertation on political socialization in the Federal Republic. It critically reviews some of the German literature and is based on interviewing a nonrandom sample of 538 Bavarian ninth-graders (*Hauptschule*), with particular emphasis on their attitudes toward the political parties. The author comments especially on the psychological and sociological literature on West German civics education and on the change from the "skeptical generation" of postwar youth to the increasingly politicized youth of the mid-1960s. His own research is prefaced with a survey of other German and American studies at various age levels. Menacher's questionnaire ascertained the level of political knowledge of his respondents in particular about the parties, their functions and organization, their strengths and coalitions, and the youths' willingness to participate in partisan politics.

His young respondents turned in 60–90 per cent more or less factually correct responses, and in doing so surpassed by far the respondents of earlier West German surveys. Only in the politics of their own state did their level of information drop significantly, and of local party politics. While they recognize the parties well enough from what appear to be well-learned civics lessons, the ninth-graders were almost completely adrift among typical samples of party program statements, an indication of the relative shallowness of their understanding of issues that accords with Greenstein's findings of the "late arrival of issue orientation." The author's cross-tabulations of political knowledge with other personal indicators showed up only sex and metropolitan residence as significant factors related to the level of political information. Menacher concludes that his respondents of 1970, represent "the politically most literate youth in German history."

The quantitative methodology underlying this study is quite sophisticated and shows a mastery in technique and presentation that is still rare in Germany. Menacher ends with a discussion of how political education can be most effective given the multiple factors of political socialization revealed in his study and their interrelations. It is to be hoped that someone will soon undertake to compare his findings with similar surveys in France, the United States and elsewhere in Europe.

Werner Klose's *Lebensformen Deutscher Jugend* is a popularly written survey of the evolution of German youth cultures from the early days of the Youth Movement. The author, a *Gymnasium* teacher, has written knowledgeably about the Hitler Youth (HJ) and also about German student life.

He describes the parental setting and the schools during World War One and later with telling vignettes. The rise of the politically radicalized youth of right and left under the Weimar Republic is chronicled and linked to the rising Nazi dictatorship and its youth movement. After World War Two, the "skeptical" postwar youth scene in East and West Germany is surveyed and contrasted with the gross materialism of the 1950s among German youth.

The last half of the book goes into current youth phenomena in considerable detail, surveying family life, education, jobs and training and the politics of youth; but his inclination toward popularization and his desire to help bridge the generation gap clearly get the better of him on most counts. His explanations are too broad to shed much light upon complex and subtle differences that exist between urban and rural families and among different urban youth settings. His account of the educational atmosphere shows little awareness of the ongoing debate over West Germany's "educational crisis" and various reform plans, nor of the literature on student revolt of recent years.

The Allerbeck-Rosenmayr book is an uneven collection of essays of varying sophistication on the student movements of recent years. The two editors, in their introduction, see at the core of the university upheaval a sudden renewal by newly awakened "leftist forces" of the attack on long-slumbering, archaic structures in parliaments, universities, and schools. The era of postwar reconstruction, the Cold War, and outdated taboos of authority and sexuality are blamed for what nearly all the contributors to this book seek to explain, namely the contrast between the "quiet 'fifties" and the turbulent 'sixties at the universities. This was the beginning of a historical

movement, according to the editors, which now is proceeding to the next step, the infiltration and "taking root" of "radical and revolutionary forces at German universities and in the mass media." The editors realize, of course, that these new cadres of the technical intelligentsia are still very far from having achieved a following among the trade unions or the working masses.

The editors project their own strong opinion about their contributors in their introductory chapter. There is, for example, G. N. Wolkow, the director of "a large Soviet youth research and organizational center in Moscow," whose "interesting work" is said to "document how one can adjust reality to fit the Marxist theory." Indeed, Wolkow is predictably unkind to the student activists of the New Left: "Left-wing radicalism is characterized by the urge to destroy everything, an illusionary attempt to transcend the limits of one's hated existence . . . illusionary because they have no clear idea which way to go." He prefers to see in the student revolt "a typical phenomenon of capitalist systems," with strong elements of rightwing radicalism and attitudes favorable to capitalism.

The editors also give the cold shoulder to "a theoretical framework for the comparative analysis of student revolt" by Alessandro Cavalli and Alberto Martinelli, which is one of the better essays in the book, because such an undertaking "can be done only at the price of a superficial internationality and . . . represents no progress over the ethnocentric self-limitation to one's own country or continent." The editors apologize for not having included "any empirically valid work" on the Third World whose role in the student revolts of the 'sixties they evidently consider crucial. The absence of Japan in this context is even harder to understand. "Ethnocentric self-limitation" may indeed be the way to describe this myopic approach. As for Cavalli and Martinelli, their theoretical framework does in fact account for and differentiate non-Western student unrest from that of Europe and the United States. It is based on the conceptualization of structural changes in the international world (Cold War), domestic economics and social mobility, domestic politics, and mass higher education.

The "theoretical approaches" in the book are followed by four sound empirical, if somewhat differing, reports on the American (Keniston), French (Converse and Pierce), Italian (Statera), and West German (Kaase) student upheavals presented to the Seventh World Sociological Congress. Curiously again, Great Britain and the smaller European countries are completely omitted from comparison, not to mention the authoritarian systems of the Iberian peninsula and Greece. The empiricists tend to leave most of the historical and cultural contexts as a residual category, but at least they bring out clearly what makes the United States and France each so different in this respect.

A further empirical chapter, by Allerbeck, tests and rejects a series of common interpretations: student revolt as "the revolt of the privileged;" as a result of generational conflict; of permissive education; of dissatisfaction with the learning experience at universities; and as a reaction to the scarcity of jobs for graduates in the humanities and social sciences—all this against a background of careful structural and role analysis. Allerbeck's contribution is in certain respects the most illuminating in that he blends a sophisticated handling of critical social science method with the systematic exploration of the student role in contemporary society. This gives his analysis a depth that by comparison makes most of the other contributions seem onesided or superficial. Among other things, he relates the radical students' identification with their student role to their exaggerated perception of the hostility of the public toward student demonstrations and arrives at a striking correlation: The more radical the more identified with the student role and the more paranoid the more radical. This will strike a resonant chord of recognition in many of us here who were in close contact with students during the years of the upheaval. There is only a minor word of criticism to add about Allerbeck's assumption of the "totality" of the student role. The trend of the future, at least in the United States, appears to point away from such a "total role" toward the continuing education and retraining of adults, in other words, toward an increasing integration of the student role with other social and economic roles on a more age-heterogeneous scale. Seen from this perspective, perhaps the years of revolt have been the last gasps of the exclusiveness of the student role in the midst of societies that are similarly doing away with other total roles he mentions, "pure soldiers," monks, perhaps even the totality of the wife-mother role. If this should turn out to be true, the socially more integrated student of the 'fifties may actually have been more advanced than the militants of the 'sixties, an age when affluence permitted many European bourgeois parents to restore ways of life that were long believed dead.

PETER H. MERKL
University of California, Santa Barbara

Brazil: The People and the Power. By Miguel Arraes. Translated by Lancelot Sheppard with an introduction by Yves Goussault. (Middlesex, England: Penguin Books, 1972. Pp. 232. $1.25.)

Born in the backlands of Northeast Brazil, Miguel Arraes became a member of the Pernambucan state assembly, later served as mayor of Recife, and finally in 1962 was elected governor of

the state. The first socialist to hold this office, he had his mandate interrupted after 14 months by the repressive military coup of April 1964. In the aftermath of the coup he found exile in Algiers. This period allowed him to examine in retrospect the events of the past and to offer an in-depth analysis of the contemporary situation. The result is the present book, an easily understandable and readable discussion which nicely complements other recently available literature on Brazil by such North American political scientists as Riordan Roett, Ronald Schneider, Philippe Schmitter, and Alfred Stepan.

On a general level this book is concerned with elites and masses. Arraes was born in and associated with the elites of Brazil, but his concern and commitment lie with the people. Thus, appropriately his critique of past and present Brazilian society focuses on the role of the ruling classes, their interests and actions. In turning to solutions for the masses of Brazilians, he first directs attention to popular movements. He identifies these movements through the periods of colony, empire, republic, Vargas dictatorship, and up to 1964. He argues that the urban and rural masses both bear the weight of heavy exploitation. Since the dominating forces will isolate these masses from participation in national decisions, "the struggle for liberation of the country is also the struggle of the social classes for their very just demands since these can only be met satisfactorily with the winning of economic and political emancipation" (p. 211). The next phase of this struggle must rely on the support of various popular movements. While mistakes were made in the past and struggle lacked sufficient theoretical background, some victories were won, he goes on. Ultimately, "it is necessary to unite all the sectors opposed to imperialism to fight against it (p. 207).

In reaching this position, Arraes makes two substantial contributions to our understanding of Brazilian political economy. First, there is his analysis of contemporary developments, based on a number of propositions, and, second, some interesting theoretical perspectives. Arraes suggests several propositions in his analysis of the 1964 coup and its aftermath. First, the coup resolved the struggle between two factions of the Brazilian bourgeoisie. The outcome favored (p. 157) "the groups of financiers in league with foreign capital in association with groups of leading industrialists" who worked closely with imperialism and believed that the Brazilian economy should be integrated with that of the United States. Defeated was the national bourgeoisie, made up of groups of industrialists who desired economic independence for the nation. The ideology and actions of this latter faction had been institutionalized through the Higher Institute of Brazilian Studies (ISEB), whose ranks had included such well-known Brazilian political scientists as Helio Jaguaribe, Cândido Mendes de Almeida, and Alberto Guerrero Ramos. Second, the coup signified the removal "from political life of those connected with the popular movement who were occupying positions of responsibility" (p. 161). The 1964 coup represented a culmination of a series of incidents dating to the deposition of Vargas in 1945. Third, the coup brought to power the most reactionary military factions. Their aim being the integration with the United States, Brazil thus was subordinated to North American interests (Arraes carefully spells this out in economic, political, social, cultural, and psychological terms).

Arraes combines his analysis with a theoretical approach that is (1) both historical and contemporary; (2) economic as well as political; and (3) focused on class structure and conflict. Nearly a third of his book, for example, is concerned with the Brazilian economy, which he traces through commercial, industrial, and financial periods. His broadly sweeping discussion examines the significance of agriculture and private capital in the mercantile capital system of the colonial period, this leading to "large estates based on the cultivation of a single crop and directed towards external trade." With this foundation, economic history passed through "a monotonous succession of cycles." He clearly demonstrates how England came to replace Portugal as "the external center of attraction for the Brazilian economy" during the 19th-century empire period. With the coffee boom, capital shifted from the Northeast to the Center-South, resulting in the collapse of industrial projects and general economic imbalance. While these developments have been portrayed in greater detail in the writings of Celso Furtado and Caio Prado Júnior, Arraes offers a succinct analysis which explicitly ties economic to changing class structure. The interests of coffee planters, large estate owners, industrialists, and international financiers are examined from historical period to period.

Arraes also analyzes politics in terms of class structure. That the economy discriminated against the masses, he feels, is related to dominant interests inside and outside the country which decisively influence politics at all levels. This has been so "despite political independence snatched from Portugal in 1822 and despite the changes that have occurred in the nature of the parties in power in the central government. Those who held power were only able to do so through dependence on external factors which affected their conduct of the internal affairs of the country" (p. 93). Arraes does not let his discussion rest on this level of polemic, however. He goes on to offer an assessment of the Brazilian ruling classes. This is not a

definitive assessment of the ruling classes; indeed there is no such study of his country. But it is provocative and stimulating, interesting and imaginative, synthetic and analytical. In short, Arraes has offered both the beginning and advanced student a useful interpretation of Brazil.

RONALD H. CHILCOTE
University of California, Riverside

Racial Minorities. By Michael Banton. (London: Fontana/Collins, 1972. Pp. 192. $1.95, paper.)

Racial Tensions and National Identity. Proceedings of the Second Annual Vanderbilt Sociology Conference, November 4–6, 1970. Edited by Ernest Q. Campbell. (Nashville, Tenn.: Vanderbilt University Press, 1972. Pp. x, 262. $10.00.)

These two books, though differing in origin, purpose, and organization, offer a common approach (theoretically informed case studies) to a common topic (race relations). Campbell asserts that there has been "far too much searching for comprehensive explanatory schemes and far too little careful attention to the structure and dynamics of particular circumstances . . . " in the social sciences (p. 4). Banton, whose book is aimed at introductory sociology courses, gives pedagogical reasons for the same method. Even if one disagrees with their reasons, the approach has obvious merit. Facts can suggest as well as test hypotheses, and both books show this.

Banton begins with an account of one historical event: the collapse of the Campaign against Racial Discrimination (CARD) in Britain in 1967–68. He asks why CARD collapsed and offers four possible explanations. What distinguishes *Racial Minorities* from most scholarly work is that the author does not choose and defend one explanation, but argues successively and successfully for all four. He similarly offers four points of view, labeled Outlooks A, B, C(i), and C(ii), for the sake of neutrality, on the question of whether racial integration is possible. These empirico-normative ideal types serve as the book's leitmotif, as Banton discusses their implications for explaining prejudice and discrimination, for the possibilities of intervention to change racial behavior, and for policy preferences.

Banton's analysis is directed mainly at elucidating the effects of environmental variables on individual behavior. These variables include types of racial categorization systems, relationships between racial and economic cleavages, patterns of residential race distribution, the urban vs. rural location of racial contact, the prevailing norms of racial behavior, and the cultural differences between the societies from which minority immigrants stem and the majority society.

In a sense, however, the book is a case study within a case study. CARD is used to talk about the study of race relations in general, and the study of race relations serves as a sample of the philosophical and methodological problems of social science as a whole. The book is studded with sensible remarks about definitions, cutting points in scales, causal interpretations, and so forth, applied to the literature being discussed. Banton also considers ethical problems: What effect will it have if I publish a finding that there is high social support for discrimination? What racial terminology should I use in analysis when participants are fighting about terminology?

The possible suggestiveness of case descriptions is even clearer in *Racial Tensions and National Identity*, for here we are dealing with a collection of articles, whose contrasts help make the point. Generally, those articles which present facts and secondarily ask what these facts suggest constitute the most coherent, forceful, convincing, and memorable chapters. Those which begin with a theory and claim to explain a case with it, on the other hand, most often fail to link the two, if at all, well enough to make it clear what is explaining what and how. Examples of largely descriptive studies that are still theoretically suggestive are Milton J. Esman's on Malaysia, John Saunders's on Brazil, Ira Katznelson's on England (which, unlike Banton's book, concentrates on the *politics* of race), and Martin Kilson's on the U.S. The articles that may raise the reader's explanational expectations too high include those by Harmannus Hoetink on the Caribbean, Austin Turk on South Africa, and Immanuel Wallerstein (who might, incidentally, benefit from Banton's discussion of definitions) on Black Africa. These all, however, contain analytical perspectives and hypotheses well worth a more extended data confrontation than these authors have naturally been able to manage in their essays.

Without attempting to do justice to all twenty authors, discussants, and editor, I shall mention just two of the questions on which more than one of the authors has something to say. One question is: Who gets discriminated against the most? Banton cites a previous study to argue not only that there is more discrimination in Britain against immigrants than the immigrants themselves believe there to be, but also that apparently "the more able and better qualified immigrants experience more job discrimination than their less skilled fellow-immigrants" (p. 91, 93). Saunders, whose study in Campbell's book is mainly designed to show that color prejudice (of a pervasive though unhostile kind) is alive and extreme in Brazil, also observes that this prejudice is more likely to be exercised against upwardly mobile colored persons, since they "leave a highly heterochromatic lower class and enter a much more uni-

formly white middle class" (p. 150). And with increasing prejudice goes increasing awareness thereof (p. 161). Werner Baer, however, comments that "racial antagonism seems much stronger among the lower classes than among the elites" in the U.S., a difference requiring explanation (pp. 167–68). But is this a difference? Prejudice is not the same thing as antagonism, as Saunders points out, and even prejudice, Banton notes (p. 103), can be operationalized in various ways. In this context, it would be useful for Banton to ask why minority members seeking accommodations suffered more discrimination when appearing in person than when telephoning in Britain (p. 93), since LaPiere and others have found personal contact resulting in far less discrimination than letter-writing in the U.S. Is it really the case that "It is impossible to know whether the person at the other end of the telephone is black or white" in Britain (p. 69)? At any rate, the findings of such field experiments mean little unless accompanied by precise details of the procedure.

Another recurring question is about the inherent salience of race. Banton, of course, straddles the issue by assigning to "Outlook A" the belief that race will necessarily always define conflict groups. But several contributors to the Campbell volume support the view that race is a natural, normal, or inevitable political barrier. Katznelson argues that race in England was bound to be politicized under any but a "legitimately color-blind policy" (p. 83), and that, failing such a policy, considerable effort had to be expended by politicians to keep racial demands out of the political arena. Stanlake Samkange sees no prospect for the success of interracialism in Rhodesia, and Turk is pessimistic about its chances in South Africa. Esman says that communal cleavages consistently prove stronger than economic ones in Malaysia and that throughout the world they "have an unexpected persistence and may even surface into political prominence after they have been long dormant . . . " (p. 241). Stanley H. Smith agrees "that race consistently takes precedence over class in most social situations." (p. 94). Wallerstein, arguing that for analytical purposes we can consider races, religions, languages, tribes, etc. all as status groups, which people manipulate in order to change their class, likewise asserts the primacy of status over class in ideology and popular consciousness, in almost all situations of social change (p. 219). I would hope that at least some of the frequent arguments in this direction could be subsumed under the generalization, "Scholars tend to inevitabilize the current." Kilson, alone, suggests that ethnic escalation (paradoxically) hastens ethnic assimilation, but the evidence is weak.

On the technical front, both books are to be congratulated. Campbell's volume is unusual for a collection of conference papers in that it does indeed include the (uneven but useful) comments offered by discussants and does indeed have a (fairly well done) index. (Maybe next year we'll even get abstracts.) And the (unsewn) Fontana paperback does *not* lose its pages after opening.

JONATHAN POOL

SUNY, Stony Brook

Australia in the Nuclear Age: National Defence and National Development. By Ian Bellany. (Sydney: Sydney University Press, 1972. Pp. 144. $7.80.)

Dr. Bellany has written the first systematic treatment of the technical and economic issues involved in any Australian decision to develop nuclear weapons. The work is in two parts. The first, of some 45 pages, discusses the general technology of nuclear power and weapons and their relevance to national development. The second analyzes Australia's nuclear environment, the kinds of weapons which Australia might want to acquire and their possible cost as well as the chief categories of strategic concern for Australian governments. The author manages to squeeze some discussion of Australian attitudes to the Nuclear Non-proliferation Treaty into his 113 pages of text but does not try to deal with the domestic politics of any of the issues involved.

In one or two places Dr. Bellany, formerly of the Australian National University and currently at the University of Lancaster, in England, has been overtaken by events. Australia has ratified the NPT, as he recommended it should do. The US and the USSR have concluded the SALT I and II agreements and are engaged in a further phase of SALT talks. Strategic circumstances have changed, not least in the matter of anti-ballistic missile systems, the deployment of multiple warheads, and the availability of more accurate methods of delivery.

Moreover Bellany, like everyone else who has considered the subject so far, is unable to specify who Australia's nuclear opponent might be, what kinds of pressure he might exercise in the pursuance of what precise aims, and what diplomatic arrangements either side might have at that time with third parties. He is also, like the rest of us, unable to make close predictions as to the ways in which weapons technology will evolve. These gaps create at least three kinds of difficulty. First, he provides no criterion for choosing between the view that sophisticated arms plus allies help to prevent war and are therefore in Australia's interests, and the view that they attract hostility and are therefore bad. Second, he does not say

what position Australia might aim for in a world of more greatly differentiated nuclear capabilities, where US systems such as MIRV, Trident and surveillance satellites bear no relationship to the sorts of systems which an India or South Africa might acquire for quite different and local purposes. Third, he does not suggest a force structure in relation to a specific set of missions. This would lead to a systems analysis exercise involving problems of redesign in the light of changing hostile systems, problems of fuels, reaction times, and penetration capabilities which the book does not attempt. Partly for these reasons, and partly because of the constantly changing economics of nuclear technology, the figures he suggests for possible alternative Australian force structures also seem somewhat speculative.

Nor does Bellany discuss the dangers which the spread of nuclear technology poses for the monopoly of nuclear control by the governments of nation states. For these are territorially defined and have governments which can be called to account. But what if such materials come to be used, or their use is threatened, by liberation or terrorist groups of various kinds? These dangers as well as possible measures of defense against them, have been inadequately examined in the literature.

Within Australia, Bellany has come in for much criticism on the grounds that he has not merely ignored the domestic politics of these issues but has also failed to condemn those groups which have argued in favor of maintaining a nuclear option. For me however, the avoidance of *a priori* political and moral strictures is one of the strengths of this calm and sensible, if limited, book. Bellany points out instead that for Australia support of antinuclear weapon and antiproliferation measures is for the time being quite compatible with the acquisition of nuclear technology and the refining of nuclear options. His recommendation that Australia should ratify the NPT, written well before the new Labour Government did so in early 1973, is also on grounds which some of that government's supporters might find objectionable:

> . . . Australia is signing away very little. It may expect to get in return a more open-handed attitude on the part of the nuclear exporting countries, which will make the growth of an Australian nuclear industry easier, which will in turn make a threat, real or implicit, in the future to ignore the provisions of the NPT more credible and make a decision to acquire a nuclear force a rational policy alternative sooner. And yet an Australian ratification . . . if it contributes to the success of the treaty, will decrease the chances that Australia should ever have to become a nuclear power to protect itself from the effects of a competitive proliferation of nuclear weapons among its neighbours. . . . And the beauty of the NPT from Australia's viewpoint is that it does not preclude it or any non-nuclear signatory from taking steps, within limits, to insure itself against the day when the benefits of the treaty are seen to be outweighed by its disadvantages (p. 113).

Plainly, therefore, ratification does not of itself resolve the dispute between those who wish to "ban the bomb" and those who wish to refine Australia's technical capabilities. It is not the only respect in which Bellany's analyses and recommendations seem perceptive in retrospect. He points out that the US alliance is likely to become less important for Australia, that Australia is becoming readier to play a part in regional politics and that the machinery of the Australian federal government is, or was, quite deficient in its ability to understand and analyse the nuclear dimension of world politics. There have recently been some signs that Bellany's point has begun to be appreciated in Canberra. It is, indeed, time.

HARRY G. GELBER

Monash University

The Politics of Iran: Groups, Classes and Modernization. By James Alban Bill. (Columbus, Ohio: Charles E. Merrill Publishing Co., 1972. Pp. ix, 174. $3.95, paper.)

Although conditions in Iran are hardly conducive to free inquiry by social scientists, this has not dissuaded determined and enthusiastic scholars from attempting to unravel the intricacies of its rapidly changing political processes. Indeed, in the last decade, literature on the Iranian political system has appeared with regular frequency. The relative impunity of Western, particularly American scholars, to restrictive governmental measures has partially compensated for the difficulties encountered by native and often highly trained western scholars to probe the present political system critically and scientifically.

After several years of fieldwork Professor James Bill has produced one of the most perceptive and intriguing accounts of contemporary Iranian political processes. His emphasis is on the appearance of a new professional middle class and its relationship to other classes. The process of social change and political modernization is analyzed in terms of its impact on group and class relations.

The rise of the new class is traced to the constitutional movement of the early 1900s, continuing through Reza Shah's rule in the interwar years, and becoming an explosive and critical new force by the mid-1940s and early 1950s. This "professional bureaucratic intelligentsia" rejects the traditional power relationships, possesses or is

acquiring modern higher education, bases its power on its skills and talents, has been exposed to outside philosophical ideas, and is free from rigid religious dogmatism. It is, nonetheless, a heterogeneous class.

Intraclass differences have generated four distinctive groups that Bill presents as ideal types—the Uprooters, the Technocrats, the Maneuverers, and the Followers. The first group refuses to be bound by traditional patterns of relationships and is largely alienated from the present system. The technocrats are primarily concerned with carrying out the particular tasks for which they have been trained and wish to modify and repair the existing system in order to do so. The maneuverers are the defenders of the traditional power network who favor the ongoing relationship and use their modern skill and education to supplement their access to power positions. The followers are those who float in the safest and smoothest direction and gravitate toward the strongest personalities, groups and classes.

Analyzing the so-called White Revolution reforms, the author maintains that this new attempt as reform from above in the end will preserve the traditional power pattern. The Shah has concentrated the aristocracy in the city by severing its connections with the countryside and then allying himself with the peasantry against the professional middle class (p. 144). The first step buttressed his position in the system by weakening the upper-class challengers. The second was aimed at holding in check the challenge from the new middle class. This twofold goal has been plagued by the scarcity of technical and managerial manpower and the lack of commitment of large groups of the professional middle class. What economic progress has been achieved in the last decade has enabled the political elite to use its newly acquired wealth to attract the alienated intelligentsia. The use of economic incentives has created disaffection and rivalry, which in turn has cast doubts on the success of the reform program's basic objective of system preservation.

This work represents a happy medium between excessive preoccupation with methodological refinement and overdescriptive compilation of empirical data. Bill's conceptual and analytical framework is sound and indicative of the breadth of his knowledge in comparative political analysis. His empirical data reflect his profound familiarity with the Iranian political processes. One might question his categorization of various elements of the new middle class. The inevitable impression prompted by this type of analysis in which groups and subgroups fall in neatly designed categories, is that somehow politics in Iran is a highly rational enterprise offering its participants free and logical choice. In point of fact, in the mixed Iranian political culture, rationality is often conspiciously absent.

Positing the rise of a new class rather than the reallocation of traditional sources of wealth and influence among the existing class is also controversial. More significantly, the question of whether the numerical increase of the educated and literate segment of the population has dramatically changed class relationships remains basically unexplored. Thus a more comprehensive discussion of the transformation of the power bases of the landed aristocracy would be in order. Has the land reform contributed to a redistribution of wealth or merely its transformation from land ownership to commercial and industrial capital? Has the ratio of control of the national wealth by the privileged class begun to show a significant change?

A further area requiring exploration is the role of the military. Although Bill occasionally refers to it, a discussion of the military as the main prop of the present regime would have significantly enhanced our understanding of the Iranian system. Where does it belong in the scheme of the new class configuration? What have been some of the consequences of the recent changes in its recruitment and composition?

These remarks should be taken primarily as suggestions for areas of research rather than as a criticism of the present work, which is a welcome and valuable reference book for both the specialist and the student of comparative politics in general.

SEPEHR ZABIH
St. Mary's College of California
University of California, Berkeley

Industry and Government in Britain: The Federation of British Industries in Politics, 1945–65. By Stephen Blank. (Farnborough, England and Lexington, Mass.: D. C. Heath and Co., 1973. Pp. xiii, 256. $13.50.)

This is the long-awaited essence of Professor Blank's massive Harvard dissertation, one of the many studies of British interest groups stimulated by Professor Samuel Beer, who here contributes a preface. As a lengthy study of the Federation of British Industries (F.B.I.) derived substantially from original sources, the work is unique and will be indispensable. Professor Blank "charmed" Sir Norman Kipping, director general of the F.B.I. (who uses the term in the Foreword) into letting him loose in the archives. After the F.B.I. died in 1965 to rise again in the C.B.I. (Confederation of British Industry), Blank even won over Kipping's virtual successor.

The book has eight chapters. After a brief setting of the general scene, Professor Blank plunges into a history of the organization from 1916

(when it was founded) to 1945. Chapter 3 is more analytical: authority, leadership, behavior, including a brief account of the interaction with government, Parliament, etc. The chapter heading says "in 1945," but the material is actually garnered from later time periods as well, right up to the 'sixties. For that reason a little awkwardly, we are then turned back in time to the F.B.I.'s relations with the Labour Governments of 1945–51. The succeeding Conservative Governments are then summoned to the scene, after which the author examines the "revolution in industrial attitudes, 1961–65," meaning essentially business acceptance of something like indicative planning. With chapter 7 the focus is more on the F.B.I. as such, while the epilogue sees the F.B.I. joining hands with the National Association of British Manufacturers (N.A.B.M.) and the British Employers' Confederation (B.E.C.) to form the C.B.I. The F.B.I. and C.B.I. are then compared along a few dimensions and the C.B.I.'s relations with Labour Governments from 1964 onward are swiftly characterized.

What emerges from this narrative and analysis is not the power but the weakness of the F.B.I. as an interest group. Two classes of variables are adduced in explanation. First, cohesiveness. The F.B.I. was so structured that it had to operate without the City of London, which enjoyed its own privileged relationship with government; without many of the smaller industrialists, who had formed their own organization (known in advanced old age as the N.A.B.M.); without the B.E.C., responsible for industrial relations; and without the Association of British Chambers of Commerce (A.B.C.C.). Even within the F.B.I., divisions went deep—over industrial relations, international trade generally and tariffs in particular, economic growth, and so on. For most of the F.B.I.'s lifetime, these cumulative weaknesses tended to prevent the conversion of potential into actual political power. So, secondly, did the values of British society in terms of industry and commerce and as entertained even by the actors themselves. Industrialists and traders "enjoyed" relatively low status; conscious of that assessment, they lacked confidence in their approaches toward the seats of the mighty in Whitehall. "British industrialists did not walk easily of (*sic*) frequently in the corridors of power" (p. 208). (The number of "typos" in this book is excessive.)

In judging this thesis, one first notes that it covers only high policy. Toward the end of the book, against the grain of the argument, one learns that on the implementation of policy, the influence of the F.B.I. apparently was substantial ("could be," the author writes, p. 213). But ambiguity surrounds even high policy. To say that an association has fallen short in converting potential into actual power is not to say that the power it did manage to exert was not considerable compared with other groups. On this score we needed some sort of input-outcome assessment based on the minutes and correspondence, but the unique opportunity was not seized. In this context but also for students of public administration, the failure to discuss the precise nature of the F.B.I.'s relations with that semi-autonomous public body, the Import Duties Advisory Committee of the 1930s, is unfortunate. That de facto Tariff Commission, allegedly a focus of F.B.I. influence, is mentioned only in passing (p. 30).

This suggests that the book has some of the defects of its virtues. Anyone who has written official history, whether of a voluntary body or for a government series, knows that the main danger is not out-and-out censorship but the gradual acceptance of an official frame of reference. In terms of access, this work is not unlike official history, which may account for the uncritical tone, the apparent skirting of some sensitive areas, and some incompleteness in the record. Thus, the account of the de facto conversion of the F.B.I. to indicative planning in November, 1960, omits the important kite-flying speech the previous February by Mr. (now Sir) Reay Geddes, then managing director of Dunlop's, which was made against the advice and over the head of a high F.B.I. official. Still, the initial judgment remains unimpaired: the work is unique and will be indispensable for all students of British government and politics and of the comparative study of interest groups.

GRAHAM WOOTTON
Tufts University

Popular Movements and Secret Societies in China, 1840–1950. Edited by Jean Chesneaux. (Stanford, Calif.: Stanford University Press, 1972. Pp. 328. $11.95.)

Professor Chesneaux's book is a collection of fifteen articles describing the political and social role of Chinese popular voluntary associations during the period of transition to modernity. There is a long history in China of political and religious dissent based in groups variously called "secret societies" or "heretical sects." Early republican revolutionaries were quick to capitalize on this history, because in these societies of the people one can find indigenous antecedents to modern nationalism, class struggle, and communal utopianism. The central concern of *Popular Movements* is this: for all of their archaic patterns of organization and belief, in what ways did these associations provide support for emerging revolutionary tendencies? Were middle-class republicans influenced by popular forms, or did the "secret societies" adapt themselves to new

goals? Would the 1911 revolution have succeeded without them? What was their role after its success? In short, in this very important area, how much does the new China owe to the old?

What we need on this topic is a good, overall history, written by a scholar sensitive both to political development and the *gestalt* of popular culture. Such a book does not yet exist, but much of the path to follow is indicated by *Popular Movements.*

Jean Chesneaux has already written a book entitled, *Secret Societies in China in the Nineteenth and Twentieth Centuries* (University of Michigan Press, 1971; first published in French in 1965). *Popular Movements* is based on contributions made to conferences and seminars held between 1964 and 1967, most of them having been prepared for the "Seventeenth Congress of Chinese Studies" held at Leeds in 1965. (Is this the "International Conference on Chinese Studies"?) The original French edition of the book, entitled *Mouvements populaires et sociétés secrètes en Chine aux XIXe et XXe siècles* (Paris: François Maspero, 1970), included an additional nine papers not translated for the English edition. Both versions include a bibliography and glossaries listing secret society names, leaders, and terms.

While mentioning the frequent crises of modern Chinese history, the editor says, "Our object in this book is to examine the role of secret societies in these crises . . . [to consider them] as a subset of the general history of China during the nineteenth century" (p. 3).

This object is substantially achieved, though as Professor Chesneaux indicates, much work remains to be done. The book is at its best in studies of particular groups and individuals within limited settings, weaker in essays which attempt to delineate persistent patterns over time. What one misses most is long-range historical perspective and more precise typology. Though popular organizations with a potential for rebellion have existed in China from at least the latter Han dynasty (25–220 A.D.), the present work contains very little discussion of such groups before the nineteenth century. A more determined effort could have been made to set up the historical context. Typology is important because we are dealing with different kinds of groups whose traditions have a certain integrity of their own. Some were religious sects with histories going back to the Sung and Yüan dynasties (960–1368 A.D.), while others were generally secret societies in a tradition originating in the mid-seventeenth century. The role of messianic ideology, for example, was much more important in the sects than in the societies; their organization and rituals were also quite different. To be sure, during the nineteenth century there was some intermingling of these two types, but they deserve more precise analysis than our essayists supply.

The need for a clear grasp of structures and types is also revealed by the book's failure to define its own title. There were both "popular movements" and "secret societies" in traditional China, as well as a variety of bandit gangs and peasant riots. What were the salient differences between these forms? How did Triad leaders relate to mass movements? On this we find brief comments here and there, but nothing sustained.

In addition, the book omits without even mentioning it a whole category of Chinese voluntary religious associations which was not politically oriented at all, such as the Wu-wei chiao (Lo-chiao), the Chen-k'ung chiao, the T'ung-shan she, the Red Swastika society, and the Taiwan Tz'u-hui t'ang, to mention just a few. It is important to discuss popular phenomena from the point of view of political development, but surely this can be done without neglecting large chunks of relevant data.

Nonetheless, there is a wealth of interesting detail in *Popular Movements:* Frederic Wakeman discusses "secret society" invasions 50,000 strong accompanied by mass initiations in broad daylight. Ella Laffey contributes a rare biography of a rebel leader who started as a charcoal burner and ended as a Ch'ing general fighting the Japanese on Formosa in 1895. In another study, Lucien Bianco concludes that the real purpose of the Red Spears was self-defense against an oppressive warlord; the Red Spears were both too limited in vision and too much a part of the local power structure to be properly seen as a revolutionary vanguard. (In other situations, however, such groups could be quite aggressive indeed, so self-defense cannot be the whole story.) Mark Mancall and Georges Jidkoff give an interesting discussion of a proto-Communist republic in Mongolia during the last years of the nineteenth century. John Lust sums up the central concerns of the book in a good treatment of "Secret Societies, Popular Movements and the 1911 Revolution" (pp. 165–200).

One should perhaps also mention a few more specific problems with this collection, such as Boris Novikov's overemphasis on ideological factors in the Triads. The Triads had many different functions, depending on the needs of their local membership. Since there were popular rebellions in every dynasty, whatever its origin, risings against the Manchus did not necessarily involve nationalism. Some minor quibbles: the book needs brief résumés of its contributors, the bibliography is incomplete (as Professor Chesneaux recognizes), and some terms are not properly translated (the San-chieh chiao [p. 295] was

Buddhist, not Manichaean [cf. p. 26]; *P'u-tu* means "universal salvation" not "universal crossing," [p. 295], etc.).

The book is weakest in its treatment of religion, which was important to most of the groups discussed. The prevailing attitude seems to be that only sociopolitical activities and statements are significant; the rest is so much "shared superstition" (p. 43), and "spiritual mumbo-jumbo" (p. 20), (see also Professor Dunstheimer's statement on p. 28). The two articles which *do* attempt to deal with cultic and mythological elements at greater length—those by Guillaume Dunstheimer and Lev Deliusin—are sketchy and are based largely on secondary or tendentious sources. (in Professor Deliusin's case, hostile reports from government-controlled newspapers during the 1950s).

The issue here is nothing more than giving due weight to all aspects of the phenomenon at hand, without unnecessarily suppressing or distorting any. Surely modern scholarship should be able to enter without prejudice even so strange a world as Chinese folk-religion and to learn much there about why the people acted as they did.

Nevertheless, the book provides important and interesting information not previously available in such convenient form. It should be read by all who wish to understand more deeply the role of popular culture in the rise of modern China.

DANIEL L. OVERMYER
The University of British Columbia

Fifteenth Century England, 1399–1509: Studies in Politics and Society. Edited by S. B. Chrimes, C. D. Ross, and R. A. Griffiths. (Manchester, England: Manchester University Press, 1972. Pp. ix, 192. $10.00.)

Historians who review books with an eye out for the interests of political scientists often cannot place a work into one of the subfields of the discipline. In the present instance the difficulty is compounded, because the work in question is not an integral account of anything but is instead a collection of essays. Moreover, the authors do not adhere to a single theme, at least on the surface.

Four have elected to focus on aspects of the reign of a monarch. A. L. Brown examines the establishment of the Lancastrian regime by Henry IV. B. P. Wolffe studies the rule of Henry VI in his majority. C. D. Ross looks anew—and skeptically—at the rising reputation of Edward IV among modern scholars. And Professor Chrimes summarizes the main points of his recent and definitive study of *Henry VII* (University of California Press, 1972).

The other contributors examine classes and regions. T. B. Pugh puts under the microscope the English magnates, knights, and gentry, in order to show how the magnates became a true peerage, the exclusively "noble" class, thereby ushering in a new epoch of English social history. R. L. Storey ranges over the northern terrain he knows so well, seeking to grasp why the aristocracy there exercised an influence disproportionate to its numbers and wealth; he finds the answer in its character as a genuine feudal warrior class, securing its power in exchange for protecting England against the real threat from Scotland. R. A. Griffiths provides a useful counterpoint by showing how the Welsh marcher lords built their power out of the much less substantial danger from Wales.

Brown's essay strikes the common chord—power and its exercise—when he demonstrates that the main problem facing Henry IV (and every other fifteenth-century king of England) was the widespread lack of trust in the monarchy among the landed classes. This lack of trust arose in part because English kings from 1399 to 1509 lacked both the wealth and machinery of government needed to assert real sovereignty and so make good Edward I's ringing boast: to see justice done for every Englishman. The other authors in turn supply the harmonics, in the form of evidence that the remedies close to hand and obvious as cures for the ills of royalty proved worse than the original malady. Kings had to resort to "generosity," or rather often heedless extravagance, as they tried to shape regimes able to take the high ground of good government, only to find themselves lost in the swamps and thickets of debt and resistance to taxation. Wolffe does not prove his case, that Henry VI truly exercised his power personally, but he does trace the ruin of Lancaster in its poverty and the resultant bargains made for political support. Ross will not allow the recently held view, that Edward IV succeeded where the Lancastrians had failed. And Chrimes denies even to Henry VII the laud usually thought due him: high praise for restoring law and order and putting finance on a sound basis. Pugh, Storey, and Griffiths, in quite different ways, reveal that while the crown was the only force with the potential to impose order on the factious society of the period, it fell far short of doing it.

Indeed, what comes through so clearly by inference is that sovereignty, which was a thing much written about in the fifteenth century, was an accomplishment of that sixteenth century revolution in politics and society we call the Reformation. Political scientists concerned with theory, comparative government, and the nature of politics, may study with profit the data pointing toward the conclusion I have stated and will now clarify. Sovereign power grew up in England only when great issues brought into fundamental re-

alignment the classes weighty in politics. In the attack on the greatest of all private corporations —The Church—the crown finally forged the weapons needed to reduce lesser franchised groups and to shape one commonwealth out of the society of "orders" left over from the Middle Ages. And it was able to do so because ecclesiastical wealth, once diverted to royal purposes, supplied the nerve and sinew of effective royal government. This book is a very useful contribution to our understanding of the political circumstances that had inhibited royal power in the preceding century.

ARTHUR JOSEPH SLAVIN
University of Louisville

Teachers' Unions and Interest Group Politics: A Study in the Behaviour of Organised Teachers in England and Wales. By R. D. Coates. (Cambridge: Cambridge University Press, 1972. Pp. xiii, 138. $4.50, paper.)

This short study has two principal aims: to depict the relationship between changes in British government policy making affecting education and the activities of teachers' interest groups, and to "specify the determinants of the behaviour of organised teachers in terms applicable to the behaviour of interest groups outside the education sector" (pp. vii–viii). In meeting the first objective the author provides a most useful examination of the shifting interface of group pressures and national government policy. His attempt to achieve the broader objective is less successful.

Schoolteachers in England and Wales have long been organized into associations for salary bargaining and related purposes. Three-quarters of them now belong to one or more of eight white collar unions, of which the National Union of Teachers accounts for about one-half the total membership. Coates's study emphasizes the internal aspects of union activities less than the development of strategies for enhancing their impact on governmental decisions. The focus is not on teachers, schools, communities, or local authorities, but on national structures of education and economic policy. Rather than asking if there may have been politically relevant shifts in the composition, situation or needs of the teachers themselves, Coates treats these as constants and portrays union activities largely as responses to changes in the outside political environment.

The main value of the study lies in its demonstration of how the unions attempted to come to terms with new power relationships in the British governmental machine. From about 1870 to the early 1950s their strategic targets were Parliament, the Ministry of Education, and (more sporadically) public opinion; and the concomitant tactics were traditional lobbying, bargaining and consulting within the education ministry, and discreet public relations campaigns. In the first half of the century, as Parliament's role in policy making receded, the unions concentrated their efforts almost exclusively on the single salient department, which Coates describes as comparatively autonomous in the policy and financial areas prized by the unions. This strategy produced predictable, if seldom exciting, results. The waning in the early 1960s of the "educational boom," and the decline in the proportion of public expenditure devoted to education, unhinged the teachers' commitment to traditional pressure tactics and stimulated experimentation with techniques more congruent with newer power relations in overall public policymaking. Easy access to the ministry no longer sufficed, as it now became necessary to strengthen the minister at the Cabinet level in the higher-level competition for funds, and (even more difficult) to gain steady access to decisional points where national economic policy was being hammered out (chiefly in the consultations between major departments and organised capital and labor). This troubled awareness led the normally sedate teachers' groups to turn to what for them was increasingly militant behaviour—to strikes, mass campaigns for larger public investment in education, and finally affiliation with the Trades Union Congress. Coates's analysis of these adjustments in strategy leaves the impression that however radical they were in terms of the unions' traditions and memberships, they were not strikingly effective. On bread-and-butter issues, British school teachers, despite their advanced organization, appear to have fared little better than their counterparts elsewhere. The upgrading of education in public spending priorities has not been re-established by these newer methods.

Perhaps this was bound to occur in a period when the central problems of the nation were viewed by the governing politicians in crisis economic terms. To cite just one example: the unions' decision to affiliate with the TUC failed to gain them access to the critical planning arenas, as they were systematically shut out of top-level consultations done under corporatist formulas, while at the same time their uncertain attitude toward the strike weapon deprived them of the clout enjoyed by miners, dockers, medical personnel or others.

These developments are, up to a point, instructive to students of interest group politics, and are well presented in this study. The author's attempt to hang on them a re-evaluation of the state of interest group theory comes off less successfully. He finds that literature marred by two central tendencies: to confuse influence with behavior, and to emphasize either political environmental factors to the exclusion of organizational characteristics, or vice-versa. His own study, he believes,

avoids the first pitfall by limiting its focus to strategy and tactics and not conjecturing beyond them into the question of impact. Happily for us (if not for his position) his case study demonstrates that this urge cannot be sustained even when it is pursued consciously; for he must, and does, introduce assessments of influence to explain shifts in strategy. But his reluctance to do so systematically leaves the issue unresolved. Coates's second heuristic point probably has more practical merit, but his method of advancing it—through devising a checklist of four sets of environmental and three internal group characteristics—is stated so generally that it is a doubtful guide for either designing or operationalizing future research in as diverse a realm as interest-group behavior. The more revealing question may still be how it is that some (but not all) kinds of groups not only adjust their behavior to changing patterns of government decision making, but also contribute to changes in those patterns.

JAMES B. CHRISTOPH
Indiana University

The Destruction of Brazilian Slavery, 1850-1888. By Robert Conrad. (Berkeley: University of California Press, 1972. Pp. 368. $14.50.)

This is a detailed study of the events leading up to the legal abolition of slavery in Brazil in 1888, based on archives, contemporary periodicals, parliamentary debates and census materials covering the 38 preceding years. After the end of transatlantic slave trade in the 1850s, there was a sharp increase in the movement of slaves from the old centers in the Northeast to the newly booming coffee-growing areas of the Center South, which were also experimenting with immigrant labor. Against this background, Conrad explores the very slow building of abolitionist sentiment and then the final rush of activity in the 1880s. He does not venture deeply into race relations or social history, and rather severely limits himself to the dates in the title, which has the consequence of limiting the background information available to the reader, and providing no information concerning the results of the abolition, its implementation or social consequences. The emphasis is on positions taken and maneuvers made by the key actors.

Perhaps because there is a large and growing number of scholars writing on Brazilian slavery and race relations, Conrad stresses his own contribution only with respect to a relatively narrow historical issue. The presence of representatives of the coffee-growing province of São Paulo in the final abolitionist coalition has often been interpreted as a sign of their progressivism. Conrad shows convincingly that they were strongly pro-slavery (given their need for labor) until the very last minute, when they jumped on the bandwagon to save what they could.

Broader and more interesting questions receive less satisfactory answers. Conrad suggests basically economic motives for the choices being made by key actors—particularly in the contrast he emphasizes between the Northeasterners who became abolitionist as their slaves were sold to the South and the coffee-growing elites who were pro-slavery to secure scarce labor. He does not, however, provide any reasonable economic analysis for the decline of slavery in the Northeast nor its retention in the Center South, even, as he says (p. xiv) after it was no longer necessary. He flirts with economic history, but does not carry it through.

The political scientist will find much useful material here for an understanding of political processes in 19th-century Brazil. But again, Conrad does not really exploit it. In his preface he asks why the abolitionist movement formed only in 1880 and why it was so successful so quickly. Somehow the question is badly put. As he says, the conditions were in the making for some time, and it is only the formal and noisy aspects of the movement which were late, and only in comparison with the United States experience. He emphasizes the role of international pressures—rather direct from Britain, and more general as a result of Brazil's growing isolation as a major slave nation. There are fascinating vignettes of prominent leaders taking radical stands or changing their position dramatically. But these are unexplained, attributed to unfettered choice of men. Conrad does not use the available information to explore the structures of power which shape conflict and consensus. In short, the book shows the results of a method which sets out to collect all information available explicitly related to slavery, and not one which proceeds on the basis of even a tentative explanatory framework. Aside from the hints at economic advantage, the actors in this particular drama seem strangely detached.

In sum, then, the book is a useful source of detailed materials about an important decision in an important country. In the end, however, it demonstrates the need for a well-grounded approach to make the immense amount of work which goes into a study such as this more theoretically productive.

DOUGLAS A. CHALMERS
Columbia University

Industrial Management in the Soviet Union. By William J. Conyngham. (Stanford, Calif.: Hoover Institution on War, Revolution and Peace, 1973. Pp. xxxvi, 378. $9.50.)

This book analyzes the role of the Communist party in managerial affairs during the Khrushchev period. Under Stalin, the Communist party was

wholly political in function. To be sure, it exercised power at all levels of industrial management, but the function was supervisory, and party personnel were nonspecialized generalists. After the death of Stalin, the flaws of the economic system were brought to light guardedly. Soviet leaders acknowledged that growth rates had diminished, technical innovation had been thwarted, and workers lacked identification with goals of technical modernization. The role of the party under Lenin and Stalin forms the first part of this book. Part Two begins with Khrushchev's ascendancy.

Professor Conyngham points out that Khrushchev inherited a Stalinist system of two giant bureaucracies side by side—the Party and the state. The Party dominated the state, and both had centralized and hierarchical bureaucracies. Party personnel paralleled state personnel at each level of hierarchy. The two hierarchies were separated by function. Party personnel dealt with policy, with verification of goal achievement, with propagandizing of personnel. State personnel dealt with operation, production, accounting. The very concepts of management differed between the two. When the state managed, the noun was *upravleniye*, "the Weberian concept of the explicit and rational exercise of impersonal power" (p. xxviii). When the Party managed, the noun was *rukovodstvo*, "a paternalistic, personalistic conception of authority based upon the intellectual and moral superiority of those holding power" (p. xxviii). The focus of this book is the imposition of party *rukovodstvo* on the state (economic) bureaucracy.

The problems of the Soviet economy, Conyngham tells us, lay with the bureaucratic structure of both Party and state. That structure achieved a high degree of centralized power but stifled the spontaneous creativity necessary for economic efficiency. In the post-Stalin period, the Party's focus changed to economics, and the Party itself directed the change.

The reform of the Soviet economic system might have been addressed either to the Party or to the state-managerial apparatus. Khrushchev chose party reform. The second part of this book re-creates the party attempt to acquire and direct economic functions. Party membership was expanded. The new members were younger, better educated (especially in technical skills), less ideological, and were often volunteers. The opposition that developed protested the policies of "economics over politics." It decried the absence of ideological work, the preference of "technocrats over humanists." The Party, in an effort to assert an entrepreneurial role at the level of production management, sought particular new strength at the grass roots level. Conyngham concludes that this policy failed, that centralization was not threatened meaningfully. At the top, a Party-State Control Committee, frankly a party organ, sought to cement state and party decision making. This, too, failed and the Party retreated to its former political role.

The third part of this book is a richly detailed study of the moves and strategies of party reform directed toward economic hegemony. The economic goals were simply stated: growth in the chemical industry, revitalization of agriculture, technical innovation. Yet the economic incentive structure was relatively unchanged. Party and state officials coordinated their activities, but in order to enhance local development at the expense of regional and national. Jurisdictional disputes were rife. Economic crime occurred in interlocking webs between party and state officials. Professor Conyngham concludes that the managerial practices institutionalized under Stalin remained essentially intact.

In an epilogue on the period to 1970, the author notes that current leadership has "replaced the ebullient, personal and agitational style with a conservative, bureaucratic, pragmatic approach" (p. 254). Of course, the present regime has chosen to reform the state bureaucracy and to return the party bureaucracy to its original focus. The Party-State Control Committee died.

The audience for this book probably is limited to students of the Communist party in contemporary Soviet Union, who will find it rich in carefully documented historical detail. Its subject would interest students of bureaucratic reform, but the text requires a comprehensive knowledge of Russian materials and the Soviet system. The glossary is inadequate and the author uses transliterated abbreviations from the Russian far too freely for the non-Soviet specialist. Its subject might also interest economists, but the intensive use of political science technical vocabulary probably rules out this audience as well.

ELIZABETH CLAYTON
University of Missouri, St. Louis

Development Policy II: The Pakistan Experience. Edited by Walter P. Falcon and Gustav F. Papanek. (Cambridge, Mass.: Harvard University Press, 1971. Pp. 267. $9.00.)

In 1953 Harvard University, under the aegis of the Ford Foundation, entered into an agreement with the Government of Pakistan for the provision of technical assistance to the newly established Pakistan Planning Commission. This was the beginning of an attenuated love-hate relationship between Harvard and Pakistan the consequences of which made Pakistan, for a while at least, the showcase of planned economic development.

While Pakistan's gain from the association was the more significant, students of development have gleaned a rich harvest from a series of publications emanating from the Development Advisory Service distilling the knowledge, and sometimes the wisdom, acquired in this undertaking. The eight essays of the present volume were completed in 1969. There is, therefore, no mention of Bangladesh, but the context strongly suggests that if the 1972 debacle was not inevitable it was clearly within the bounds of possibility.

An introductory perspective to Pakistan's development by Walter P. Falcon and Joseph J. Stern sets the stage. Economic development there certainly was, with an increase of GNP from Rs. 21.92 billion in 1954–55 to Rs. 72.06 billion in 1968–69. Social development there was not. In the latter year illiteracy was 85 per cent; three-fourths of the households disposed of monthly incomes of less than $42; the daily caloric intake was an unbalanced 2100; there was one doctor for each 7000 inhabitants; 6 per cent of developmental funds went for education, and 2 per cent for health. Stern follows with an essay targeted on disparities in developmental progress between the East and West wings. Gross regional product per capita in East Pakistan was Rs. 293 in 1950 and 325 in 1960—a gain of Rs. 32. In the West Wing the comparable figures are RS. 342 and 412—a gain of Rs. 70. Development, in short, seems to have exacerbated the inequities between East and West.

G. C. Hufbauer analyzes West Pakistan exports, and the promotional devices which government has employed. These include tax exemption, tax rebates, export performance licensing, "pay as you earn," and export bonus vouchers, all of which raise critical problems of sectoral discrimination and entail heavy political costs. Henry D. Jacoby develops a system-planning model for electric power in West Pakistan, designed to inform investment choices between independent and integrated market and technological structures. Falcon and Carl H. Gotsch look at West Punjab agriculture and find Punjabi peasants as sensitive as anyone else to price incentives, modern irrigation, and technology, and quite as able, as events have proved, to achieve spectacular expansion of production.

Two of the essays are concerned with what was then East Pakistan. Robert Repetto analyzes the economic aspects of irrigation design in the East Wing, where the problem is to reduce the capital intensity of irrigation projects and increase productivity per investment unit. John Woodward Thomas reviews the rural public works program initiated in 1962, which over a five-year period achieved highly salutary results in relieving rural underemployment, constructing and reconditioning roads, developing community water supplies, building embankments, digging drainage and irrigation canals, and providing schools, community centers, and health facilities. In 1967 Ayub's East Wing janissaries took over, the program degenerated into a political and utterly corrupt operation, and in due course it withered on the vine.

Papanek concludes the collection with a study of Pakistan's industrial entrepreneurs. He points out that the distinctive features of Pakistani industrialization were its rapidity, its structure (i.e., based on a llimited number of very large units), and its dependence upon imported technology. Commercial banks were almost unknown at the time of partition, and the principal—almost the only—sources of funds for industrial development were the merchant-capitalists. In short, those who had, got. Since the technology was imported, education and technical competence were not prerequisite. They sometimes came along as consequence. The industrial system is characterized by a high degree of structural concentration.

This and the other volumes from the Development Advisory Service constitute a worthy intellectual capstone to what was a distinguished experiment in development planning. They will be definitive in the literature for some time to come.

ROWLAND EGGER
Southern Methodist University

Revolution and Tradition in People's Poland: Education and Socialization. By Joseph R. Fiszman. (Princeton, N.J.: Princeton University Press, 1972. Pp. 382. $15.00 cloth, $7.50 paper.)

Joseph Fiszman belongs to a small group of scholars well qualified to write on contemporary Polish affairs. Born in Poland, he has fluent command of the Polish language, a circumstance that not only permitted access to primary sources, but also allowed him direct questioning of relevant informants in his research, conducted in Poland. The book offers a summary and conclusion of his efforts, the partial results of which he presented earlier in a number of articles and lectures.

The book has a clear topical pivot, indicated by the subtitle, around which the author wraps a number of interesting problems relevant to the main theme. It appears that Fiszman was able to get to the roots of the basic question and to provide an honest, serious, and highly readable presentation of his findings. The volume deserves the attention not only of students of Polish and Communist affairs, but of people interested in American educational problems as well. Naturally, his report—like every scholarly enterprise—is partly subjective, and though he is too good a scholar to allow his prejudices to ensnare him completely,

they show in a number of instances, especially when he refers to the past.

Fiszman's study focuses on Polish teachers' efficiency in transmitting Communist educational values to their pupils in Polish elementary and secondary schools. On the whole, he finds their performance less than overwhelming and points out several reasons for their ineffectiveness. First, they retain strong ties with the past and with "those same traditions that have prevented the larger society from embracing wholeheartedly the new socialist ideology and the radical cultural, social, and political changes that accompany it" (p. 317). Characteristically, the younger teachers trained after World War II demonstrate less degree of commitment to the values they are expected to promote than their older colleagues do. According to Fiszman this is primarily due to their attachment to the Church, representing "fundamental moral, political and philosophical opposition to the new regime" (p. 311). A second reason for the teachers' deficient performance is dissatisfaction with their status and material compensations. While enjoying relatively high esteem in the community, most teachers consider themselves deprived of proper professional recognition and fear further reduction in their status. The prestige problem is accentuated by the very low salaries of teachers, especially at the elementary level. In addition, Fiszman notes that teachers are unhappy with the attitudes of the regime toward educational needs, including their training to fulfill the demands the system imposes upon them. In Poland, as in many other countries, industrial development takes precedence over educational requirements. Consequently, many individuals are deprived of the major tool of social advancement, and the existing stratification system is perpetuated, putting a question mark on the Communist promise of a classless society.

Fiszman's findings lead him to make some predictions. Although their novelty is not very remarkable, they are worth stressing. Most significant is his view that in the future a *modus vivendi* will be reached between the Communist regime and the existing traditional institutions, including the Church, and that a blend of new and old cultural values will be achieved in time. When this happens, the rulers will stop viewing the teachers as agents of socialization and will demand from them only performance in support of an existing power structure. To this prognosis Fiszman adds a statement—somewhat contradicting what he said earlier about prewar teachers (p. 197)—that the Polish educational system is traditionally well-suited to perform this role.

Despite its value, the book does have its weaker points. One is a lack of "historical sensitivity." Throughout the book the author silently assumes that the traditional values and institutions, especially the Church, always acted as reactionary and retrogressive forces. This attitude appears to express more the ideological tendencies of Fiszman's sources than the complex realities of the past and the present. Another weakness is that when the author illuminates problems by contrasting them with prewar examples, some of them do not seem relevant to the main theme and present incomplete information. Take, for example, the case of Jewish students at Polish universities. While there was pressure to introduce *numerus clausus* because of the great number of Jews in the legal and medical professions (55 per cent of all attorneys in Warsaw and Lodz, 86 per cent in Cracow, 73 per cent in Lwow; 35.8 per cent of all doctors in Warsaw, 62.5 per cent in Lwow, 65 per cent in Cracow, 66.7 per cent in Lodz), restrictive practices of admission never became official educational policy as claimed by Fiszman on p. 57. In 1935–36 Jewish students constituted, 13.2 per cent of the entire student body, a proportion greater than the share of Jews in the total population of the country (9.7 per cent). More than one-third of them studied law (39.3 per cent), and close to 10 per cent medicine.

It would be wrong to infer from this evidence that Fiszman consciously manipulates the issue. One rather suspects that he was conditioned to accept unquestioningly information given him by interested advisors. While his predisposition might be excusable, it raises certain doubts in regard to his other sources. The perplexity becomes even greater when one realizes that Fiszman's findings are mostly based on a secondary analysis of material provided by research groups in Poland. Was he given access to all relevant sources? Why at no point can the reader examine the text of an entire research instrument such as questionnaires and interview schedules?

To raise these questions is not to minimize the effort of the author or to detract from the merit of the book. It is a valuable contribution, filling a gap in the literature of the subject and providing much new information—unfortunately some of it already outdated—in the area bordering on education, sociology, psychology, and political science. Once again it confirms the fact that the junction of various disciplines always presents a promising site of exploration.

Princeton University Press deserves credit for publishing such a useful study and doing it so effectively. But why allow errors in spelling Polish names (e.g., Sienkiewicz and Putrament)?

V. C. CHRYPINSKI
University of Windsor

The Southern Expansion of the Chinese People. By C. P. FitzGerald. (New York: Praeger Publishers, 1972. Pp. 230. $8.95.)

This impressive attempt to survey the whole

sweep of the southward movement of Chinese influence, culture, and power from the earliest times, and in its light to examine the situation today, should be studied by everyone concerned about the future of eastern Asia; and who is not? It is history not for its own sake but for guidance in the interpretation of a contemporary political problem bedevilled by guesswork. His aim, Professor FitzGerald tells us in his Introduction, is to provide the background facts of what he sees as "one of the most enduring features of all Chinese history" (p. xv). He starts with the Sinization of China itself, beginning with the flow of influence and culture from the original confederacy of states in the Yellow River basin into the Yangtze valley in the days of the Chou Dynasty (eleventh to third century B.C.). Then southward into what is now North Vietnam, with a big spurt under the T'ang (618–906), followed by the consolidation of the South under the Southern Sung; but without the Vietnamese, for with the T'ang collapse they gained their independence. The author contrasts the case of Vietnam, which absorbed Chinese culture but rejected incorporation in the empire, with that of Yunnan, a non-Chinese region, which after initial conquest by Kublai Khan was incorporated by the Ming (1368–1644). The two chapters on Yunnan, besides offering much that will be new to many students of Chinese history, outline a possible model, as Professor FitzGerald sees it, for further Chinese expansion, should it become feasible. His explanation of the Ming abandonment of the pursuit of sea power after the voyages of Ch'eng Ho into the Southern Ocean and beyond is the most adequate one has yet seen. Henceforward, he points out, China could intervene in South East Asia only by land, whereas most of the migrant Chinese, who flocked to South East Asia in such large numbers in the nineteenth and twentieth centuries, went by sea, settling in lands to which sea power was the key. Thus "the most impressive expansion of the Chinese to the south came at a period when the home government took no interest at all in any such development" (p. 216), and was too weak to play any part in it.

In his last two chapters Professor FitzGerald sums up the situation today and its prospects. He adduces ample reasons to show how supremely difficult a policy of intervention in the Nanyang (Lands of the Southern Ocean) would be for China, and how far from being a fifth column for Mao Tse-tung the Overseas Chinese communities have become. He points out that the policy of disembarrassing herself of the Nanyang Chinese enunciated at the time of the Bandung Conference of 1955 was not a trick, as the Western press insisted. And notwithstanding the excesses of the Cultural Revolution of 1966 nonintervention is still the guideline. But the hostility of the postwar governments of South East Asia toward their resident Chinese communities has to be faced, and the problems arising from the massive Chinese settlement in South East Asia must, sooner or later, become China's concern.

Regrettably in a book which one has read with enjoyment and profit there are too many errors in its references to South East Asian history. The Burmans entered Burma in the ninth, not in the twelfth and thirteenth centuries (p. 3). Ko-lo-feng's conquest of the upper Irrawaddy valley in the eighth century did not result in "the more or less elimination of the Mon peoples" (p. 55). They were strong enough to halt the great Nanchao invasion, which destroyed Halingyi, the Pyu capital, in 832, and went on in the succeeding centuries to play a powerful role in the political and cultural life of Burma. Wu San-kuei, who drove the last Ming claimant into Burma, did not threaten Mandalay in 1661. That city was not founded until two centuries later. The Burmese chronicles say that he advanced right up to the walls of Ava, the then capital, and compelled the king to hand over the fugitive. Marshall Sogatu did not cause the Angkor kingdom to offer tribute to Kublai Khan: the force he sent into Cambodia for that purpose never returned. It would be well to check up on the book's references to South East Asian history when a new edition is planned.

D. G. E. HALL

Herts, England

Preconditions of Revolution in Early Modern Europe. The Johns Hopkins Symposia in History. Edited by Robert Forster and Jack P. Greene. (Baltimore: The Johns Hopkins University Press, 1970. Pp. 214. $8.95.)

Comparison of revolutions entails comparison of old regimes. And vice-versa, for a revolution may serve the student of an old regime much as a glass-bottomed boat serves the naturalist examining marine life. Old regimes are the primary topic of the five historical essays in this book. In their introduction, however, the editors classify not the old regimes but the outbreaks whose "preconditions" are analyzed:

- Two great national revolutions (the Dutch and the English, discussed by J. W. Smit and Lawrence Stone, respectively).
- Two national revolts which might have become revolutions (the revolt in Catalonia in 1640 and the Fronde in France, discussed by J. H. Elliott and Roland Mousnier, respectively).
- A large-scale regional rebellion with a limited potentiality of becoming a revolution (the uprising led by Pugachev in the middle Volga region in 1773, discussed by Marc Raeff).
- A secessionist *coup d'état* (through which Portugal withdrew in 1640 from the Spanish monarchy, discussed by Elliott).

• Two urban *jacqueries* (in Palermo and in Naples, in 1647 and 1648, discussed by Elliott).

The outbreaks with revolutionary potentialities (the first three categories) were all preceded by much the same kinds of conditions. The editors summarize in a list: "social tensions born of conflicts over status and authority more than of material stress, weaknesses in political structure, state ambitions, discontent with existing regimes and government policies, elite participation, an alternative ideology capable of uniting dissident elements, an institutional vehicle for opposition activity, and some prospects for success" (p. 17). A list is not an explanation, and this is not put forward as one, but it does designate some important dimensions on which comparison might be (and might have been) made.

For explanations, one turns to the essays. Each is by a well-known expert on the historical milieu in which one or more outbreaks occurred, and on its own terms each could scarcely be bettered. But, guided only by the distinction between "preconditions" and "precipitants," these historians vary greatly in the explicitness of their theoretical perspectives, in the emphasis accorded to description of structure or to narrative of process, and in the focus on durable and general preconditions vs. temporary and specific ones. Perhaps the comparability and differential explanations would have been better if the writers had begun with some such list as the editors eventually drew from the essays.

Still, what they offer is illuminating and suggests comparisons that range outside the historical boundaries of Europe between 1555 and 1774. For instance, it is evident that the Spanish imperial system was overextended for its technological and fiscal base, and in obsolescence began to fall apart; and it would be instructive to examine the contrast with the British imperial system after American independence.

Dutch independence was pivotal in Spanish history. The revolt of the Netherlands bears comparison, from another angle, with the English revolution of the 1640s. (Stone is not persuaded of this as much as Smit.) Considering the complexity of the events, Smit's 35 pages and Stone's 54 pages are masterly expositions. They therefore stimulate further questions which, putting aside geographic circumstances, would perhaps make clearer the character of the political forces at work. What might have been the fate of Philip II if in 1576 he had been in Ghent when the Spanish garrison there was forced to capitulate? What might have been the position of the parliamentary forces if in 1642 Spain or France had been able and willing to land men and supplies in Bristol for Charles I? Historians do not take happily to counterfactual analysis of political developments. But perhaps it would sharpen the discussion of causality and of comparability.

The historians in this book agree that class conflict was not primary in the events they are concerned to explain. They place different degrees of emphasis on this negative point and react divergently to the idea of a "bourgeois revolution." They might have explored more systematically the major alternative line of thought, which would center on conflict among local or provincial communities and on conflict between each of these and a central authority. Certainly this idea is not ignored: it is especially prominent in the essays by Elliott, Mousnier, and Raeff; and regional disparity between Holland and the other Netherlandish provinces appears to Smit a key fact, while Stone of course mentions county government and the rise of a "country" ideology opposed to "the court." But I would argue for promoting the community/outsider type of conflict to the status of an organizing theme. If we could account satisfactorily for the differing conduct of local units under the stress of Europe's modernization, we would be able to make better sense of the wide variety of revolts in this period.

PHILIP DAWSON
Brooklyn College, CUNY

Chinese Education and Society: A Bibliographic Guide. By Stewart E. Fraser and Kuang-liang Hsu. (New York: International Arts and Sciences Press, Inc., 1972. p. 204. $15.00.)

This bibliography mainly covers English language publications and translations. A wide range of sources is presented from bibliographies to year books. The references are primarily to secondary rather than primary sources. Emphasis is on materials from the United States, Western Europe, and Taiwan. There is a superficial survey of Chinese Communist sources.

Beginning with a spotty, but general survey of materials on the Cultural Revolution, the bibliography focuses on a review of educational materials from the primary to the university level. In addition there is a listing of materials on teacher's training, rural education, and the impact of international relations on education. The latter topic is not too clearly defined.

The period covered in the bibliography, from 1966 through 1971, was a time when China's educational system was in transition from what Mao Tse-tung called a "bourgeois, revisionist" educational system to a "revolutionary" one. The decisions made on education were determined by political events and political debates. Though the bibliography concludes with a listing of more general political writings, the authors, Professors Fraser and Hsu do not give sufficient attention to the debates on education between the various political groups. For example, the views on education of Mao's opposition are not presented. As a result, the reader can learn of the sharp conflicts that occurred over China's educa-

tional direction only through secondary sources

In summary, this bibliography is primarily a wide-ranging survey of China's educational system in a period of transition. The reader who wishes to learn why the educational system was in such upheaval during the Cultural Revolution needs a listing in greater depth of the political debates on education. These debates, more than any other materials, reflect the controversies that have accompanied China's educational revolution.

MERLE GOLDMAN

Boston University

Agrarian Revolt in a Mexican Village. By Paul Friedrich. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1970. Pp. 149. $7.95, cloth; $3.95, paper.)

Introduced as the "first detailed and systematic anthropological study of the origin, development, eruption and conclusion of an agrarian revolt in one village," this brief volume encompasses the cultural background of a village in 1885, economic and social changes from 1885–1920, the life of an indigenous revolutionary, the years 1920–26, the causality of revolution, *ejido* statistics, diet, and the Tarascan language. What bears discussion, however, is not so much the book's marquee, for which the audience can recalibrate, but its architectonic. Had the book been substantially longer, Friedrich's thoroughness undoubtedly would have explored in more detail some of his fundamental premises concerning the agrarian revolt syndrome, premises with which a number of authors might disagree.

To others who also have tried to document the Mexican Agrarian revolution as "history" or as ethnographic reconstruction it may come as a surprise to read: "Beneath the political slogans and social confusion of the Mexican Revolution (1910–1920) there surged the peasants' urgent and implacable demand: '*la tierra*'." (p. 4) On page 91, however, both Professor Friedrich and his indigenous revolutionary Primo Tapia seem to be mystified by the actual behavior of the peasants: "Another immediate problem confronting Primo was the paradoxical refusal of most Narajeños to participate actively in any land claims. 'The *ejido* was for their own benefit but they didn't want it'." And on page 92 we learn that the signatures petitioning for the land were obtained by a ruse: petitioners for land had actually given their signatures for requesting a priest. Despite this evidence the demand for equity in land usufruct is postulated as one of "The Seven Causes" of local agrarian revolt. The remaining six are: ideology of agrarian reform, local agrarian organization, physical violence, gifted leadership, pre-existing patterns of local social structure, and "the encapsulating politico-governmental organization" (p. 136).

There are other problems with the above causes. Just as the demand for *la tierra* is not sufficiently demonstrated as a cause, the gifted leader of the revolt arrives in his village *after* the revolt to find the village deeply enmeshed in agrarian politics. Also, it is merely asserted, never demonstrated, that physical violence is a cause. As for local social structure, the author himself admits village events were always transformed by what went on in Mexico City.

On the positive side, however, the role of the cultural mediator or broker of social change is superbly instanced in the biography of Primo Tapia, the revolutionary. Primo, a longtime *bracero* in the States gets involved in Los Angeles with the Flores Magón brothers and their devotion to a Spanish anarcho-syndicalized version of Bakunin and Kropotkin and the IWW. Primo's original intellectual curiosity, and the modes for its expression he found amongst the few alternatives available to him, his attempt to feed it into the system as native son returned, and his tragic Promethean end constitute a fascinating and insightful account.

This reviewer's attitude toward the book is more positive than objections to the epistemology of the author might indicate (though they are real, and important). The author tells us, at the end, that "like Thucydides . . . the first scientific historian" he has "made fully intelligible the causes of (one) agrarian revolt" (pp. ix, 135). But Thucydides was "there" for his history of strictly contemporary events, in which he was both participant and historian, and the history of the Peloponnesian Wars spans eight books. Therefore, if read as a Companion Summa under the Bo tree for total enlightenment of (even one) revolt, some will find this book wanting. But if read as a collection of some of the results of those aspects of anthropological and linguistic inquiry in which Friedrich's scholarliness excels, it has great merit. *Agrarian Revolt in a Mexican Village* certainly belongs in the library of any teacher or student of Mexico of that period. It belongs, not as a piece of "Thucydidean Modern," but as a chest of drawers with some questionable improvisations in overall design, but with several genuine and elegant parts.

LOLA ROMANUCCI-ROSS

University of California, San Diego

Jewish Nationality and Soviet Politics. By Z. Y. Gitelmann. (Princeton, N.J.: Princeton University Press, 1972. Pp. 573. $20.00.)

This book is not only first-rate but also, in all likelihood, original. It is an impressive tribute to the marriage of social science and history. Professor Gitelmann has used a framework taken from the political sociology of developing and modernizing states and used it to elucidate the history of the Jewish Sections of the Russian

Communist Party. These were units within the Bolshevik Party charged with the task of integrating the Jews into the new state. The area is defined as "the interaction between Bolshevik schemes of modernization and political development, on the one hand, and developments within the Jewish community, on the other." Gitelmann begins with a fifty-page description of the politics of the Jewish question in Tsarist Russia, a description which is itself a masterly survey of the proliferation of political parties and groupings within Russian Jewry in the two decades or so before the Revolution. It may be that the Zionists and the Jewish Kadets are somewhat overlooked, but the overwhelming bulk of Russian-Jewish political interest is, without a doubt, fairly represented. At times Professor Gitelmann also gives a psychological dimension to his analysis, as, for example in the portrayal of the "marginal man," the "non-Jewish Jew," and the general phenomenon of "Jewish self-hatred." He makes the useful point that amongst nationally conscious Jews who did not become "marginal men" the Bolsheviks had almost no followers. From a certain point of view Professor Gitelmann is undoubtedly correct in formulating the attraction of Bolshevism for the non-nationally conscious Jew in these terms: "Blocked by the majority from assimilation, refusing integration into the existent minority, they created another minority-culture, not defined in ethnic terms, which they were able to transform into a majority culture by political means" (pp. 108–110).

The Jewish sections of the Communist party were established in 1918 to work in concert with the Jewish Commissariat. Their main thrust, Professor Gitelmann tells us, was directed against the "bourgeois-clerical-Zionist camp," with the general aim of reconstructing Jewish national life on "a proletarian-socialist basis," giving to the Jewish masses the complete right to control all existing Jewish social institutions (e.g., schools). The Commissariat would also enable Jews to venture into agriculture, procure government relief for the needy and shelter for the homeless, and fight anti-Semitism, pogroms, and so on. What this meant in practice was the re-formation of the communal councils by way of administrative fiat combined with coercion or the threat of force. The Hebrew language, the Zionist movement, and the Jewish religion all had to be discarded or crushed.

The fight against Hebrew at least was successful; but some idea of the inner turmoil undergone, by Jewish Communists can be gained by these remarks, quoted by Gitelmann, from Shlomoh Yaakov Niepomniaschi. A former student at a rabbinical seminary, he was in 1917 secretary of the Poltava Zionist Committee. But two years later he joined the Bolsheviks and was swept up in the storm of the pogroms in the Ukraine. He served in the Cheka and in the Red Army during the Russo-Polish War. In a letter of the early 1920s he wrote: "We who have gone through so much terror are convinced that only the Soviet Government saves us from a blood bath. . . . The question is posed in a cardinal way: either with the Horthys and Mussolinis or with Lenin and Trotsky" (p. 282). So far so good, but what of the Communists' Jewish policy? Here Niepomniaschi wrote: "I will say, in Gordon's words [the reference is to J. L. Gordon, the famous poet of the nineteenth-century Russian Jewish Enlightenment reviewer] 'I am a slave to Hebrew forever' . . . I gave my best years to these old writings. . . . I cruelly choke these sentiments in me so that they shall have no influence on our political line. But how long can one be a tyrant over oneself? Ach, it is indeed bitter " (p. 283).

Professor Gitelmann is very illuminating indeed when it comes to describing precisely what the Jewish Sections offer by way of education. It could be Jewish in language only (Yiddish); instruction in culture was inappropriate because of its religious and Hebraic roots. Furthermore, such teaching of Jewish history as there was, concentrated on the conflicts between Jewish workers and their employers; even with respect to Yiddish literature, predominantly those writings were selected which made the ghetto community a target for satire, the better to point the contrast with the achievements of Soviet Society. It is not surprising, therefore, that Professor Gitelmann can talk of Soviet Yiddish culture as "a synthetic sub-culture created and supported largely by an intelligentsia-dominated minority and with a limited following among the Soviet Jewish population as a whole" (p. 500).

In his conclusion, Gitelmann seems to argue that, whatever the failings of the Jewish Sections in relation to the Jews, at least they were successful agencies on behalf of the Soviet government. In other words, in terms of assimilating the Jews as a society in process of modernisation, the Sections were at the least able to prepare the Jews for entry into the mainstream of Soviet Society (though this process was only completed by the forced industrialisation of the five year plans).

Although Gitelmann ends with the closure of the Jewish Sections in 1930 he is well aware that Soviet Jewish policy has ramifications and affiliations that extend, say, to the treatment of the problem of ethnic identity in the postwar communist régimes (e.g., Yugoslavia, Czechoslovakia) the vitality (or lack of it) of a secular Jewish culture, and similar experiments in Jewish life. This book is a model piece of historical writ-

ing and a contribution not only to Jewish history but also to that of the Soviet Union, no less than a guide to certain problems of the lesser communist states.

LIONEL KOCHAN
University of Warwick, England

Democracy in Crisis: New Challenges to Constitutional Democracy in the Atlantic Area. Edited by E. A. Goerner. (Notre Dame, Indiana: University of Notre Dame Press, 1971. Pp. 199. $9.95.)

Democarcy in Crisis is a collection of ten short essays on a variety of critical problems confronting democratic governments in Western Europe, the United States, and Canada. The editor, E. A. Goerner, is a professor of government at the University of Notre Dame, and the contributors, all of whom have published extensively in their fields, have backgrounds in either teaching, government, or journalism in the countries on which they have written. Because the book has a thought-provoking theme and the writing throughout is clear and concise, it is likely to appeal to the general public as well as to a specialized audience interested in comparative politics.

Some of the essays are provocative. Giovanni Bognetti, in "The Crisis of Parliamentary Government in Italy," explains political instability and governmental paralysis in the Italian parliamentary system during the Christian Democratic leadership of the 1950s and early 1960s as a serious challenge to constitutional democracy in Italy. He argues persuasively that a realistic alternative to the dangerous *immobilité* in Rome is an invitation to the antisystem opposition to participate with the center-left in government, namely an "opening to the Left," for the Italian Communist Party (PCI) in the Cabinet. Robert H. Evans neatly complements Bognetti in a separate essay, "The Changing Role of the Communist Party in Italy," by suggesting how the PCI has gradually been readying itself for this move and why the party's growing electoral constituency, especially in the South, soon will mandate a decision-making role for it.

In "Judicial Power and Constitutional Democracy in Italy and West Germany," Donald P. Kommers presents a comparative appraisal of judicial review, a controversial yet often neglected aspect of parliamentary democracy in these two former authoritarian states. Kommers finds that judicial review, in spite of deep-seated suspicions of its compatibility with state sovereignty, has become a well-established aspect of democratic government in West Germany, more so than in Italy, where, Kommers suggests, politics in general and the democratic processes in particular enjoy little popular understanding or support. In contrast with widespread compliance in West Germany, public officials in Italy appear to ignore constitutional decisions, and regional governments, parliament, and the Vatican harshly attack the High Court.

Bastiaan Van der Esch in his essay, "The European Community and Constitutional Democracy," raises interesting questions about the strength of democratic political processes on the supranational level. He finds the European Parliament unable to monitor decision making of the EEC Council but does not believe that the legislature should be endowed with more authority until its representative character has been strengthened, perhaps by direct election of its members. He is also concerned with the ways in which the powers of the community conflict with the socioeconomic sovereignty of members and warns against community identification with any one ideology. Van der Esch believes the community's abandonment of political neutrality could jeopardize the future of European unification.

Other essays, though thoughtful, are pedestrian. Jean Blondel writes on challenges to constitutional democracy in Britain and France and focuses almost entirely on student extremism. He finds that French students are more active and anti-establishment than British students, though he offers no convincing explanation for this difference. He apparently sees no danger to constitutional democracy either in the overbearing and compulsive executive power and the weakened and servile parliament of the Fifth Republic or in the potential for Cabinet autocracy, especially under the Tories, in Britain. Gerhart Niemeyer examines in "The New Left" the revolutionary and cultural aspects of student extremism in the United States and West Germany and argues that this new antiestablishment counterculture can and will do much material damage to democracy because of the prevalence of intellectual and moral anarchy in today's free societies. For the conservative Niemeyer, it would appear, democracies dare not be libertarian in their attitude toward activist critics, lest they risk self-destruction. After all, he argues, look what happened to the Weimar Republic.

The book as a whole suffers because of the failure of editor Goerner to provide direction and coordination. The individual essays vary greatly in quality and in content. Several are based upon substantial research with rigorous documentation, while others appear to be little more than aggregations of casual and highly subjective reflections or reminiscences. Also, there is no apparent logic or editorial plan behind the sequence of essays. Some deal with political and constitutional challenges and others with economic, cul-

tural, and foreign policy problems. The notion of crisis is never defined and seems to vary in meaning from essay to essay. There is no explanation of why some of the contributors have adopted a cross-national approach while others chose to focus on a particular country.

Finally and frustratingly, editor Goerner provides no answers to central questions in the essays. To what extent are political and constitutional crises more or less serious challenges to democracy than economic, cultural, and foreign policy problems? How and why have some democracies responded more or less effectively to the challenges confronting them? What are the implications of today's crises for the future well-being of liberal and democratic government in the Atlantic area?

MINTON F. GOLDMAN
Northeastern University

The Military Indoctrination of Soviet Youth. By Leon Goure. (New York: National Strategy Information Center, 1973. Pp. 75. $1.00, paper.)

In 1967 the Soviets shortened the length of obligatory military service by one year and instituted an obligatory preconscription military training program in schools. This monograph reviews the latter "military-patriotic" educational campaign since then. Frank R. Barnett, President of the National Strategy Information Center which sponsored the study, observes in the preface that a "whole generation of youth is being raised in an atmosphere of fear and hatred for the Western democratic countries, and imbued with the need for constant preparedness for an apparently inevitable conflict" (p. v). Or, as the author puts it: "Current Soviet efforts to instill in youth a sense of nationalism, militarism, and readiness for war are on a scale unprecedented in peacetime . . . and appear to be leading to the militarization of the Soviet education system" (p. 1). In other words, a warning is sounded to all those who support detente between the USSR and the West that the Soviets pursue policies and actions that run counter to this spirit.

The author proceeds admirably in his task. The study is well documented and the style of writing is factual. But somehow he never convinces the reader that a "new Soviet man" is really in the making. Toward the end of the study, moreover, he himself seems to have run out of steam: "It remains to be seen how much of this nationalistic and militaristic indoctrination will 'take' " (p. 73).

The trouble with Soviet reporting on patriotic, military and ideological themes is that they become so larded with inspirational optimism that things no longer seem what they are. The author could be faulted for accepting some of that reportage at face value. The preconscription training at school may include both military-ideological training and sports, with the former getting more publicity than the latter although the students' interest is in reverse. Or to quote a Soviet Lieutenant General from the monograph—"military-patriotic work . . . is considerably enlivened before festivals and jubilees, and dies down when there are no jubilees" (p. 61).

All of which is not to deny that preconscription training in Soviet schools should not be watched for possible danger signals. The Soviet educational establishment, however, was strong enough to torpedo the Krushchev school reform. It can easily dispense with any encroachments by the Soviet military unless, of course, the military takes over in the Soviet Union.

JAAN PENNAR
Richmond College, CUNY

Germany in Our Time: A Political History of the Postwar Years. By Alfred Grosser. (New York: Praeger Publishers, 1971. Pp. 378. $12.50, cloth; $4.95, paper.)

For a reviewer to characterize a book as only the latest edition of an oft-written work would, in most cases, ensure an invitation to pistols at dawn. Yet there seems no more appropriate evaluation of Alfred Grosser's *Germany in Our Time* than to note its relation to a distinguished lineage. Quite simply, it is the greatly expanded, most mature product of what truly has been the work of a quarter century: Grosser's careful, complex, compassionate, probing of the German enigma. His purpose first and foremost has been the enlightenment of his fellow Frenchmen, but his contribution to the understanding of Anglo-Saxons and of Germans themselves has been scarcely less important.

All the virtues of the three earlier volumes are displayed in *Germany in Our Time* in even greater profusion and intensity. There are Grosser's easy, flowing style unmarred by jargon or extended treatment of abstractions; the wealth of historical, statistical, and biographical detail; and the extensive, most useful bibliographic notes covering the scholarly literature in three languages. The book's explicit target audience, the general reader with $12.50 to spend, will find almost too rich a feast—as in Grosser's ambitious but relatively successful treatment of major economic social and intellectual forces in the postwar evolution of the Federal Republic (carried off in less than one hundred pages). The expert, should he look, will find many a nugget of half-forgotten domestic political data or of linkage relationships between events, a masterful portrait of the Nuremberg-Occupation period, and myriad unique

comparisons of contemporary French and German social/political development.

To repeat only what others have long observed: Grosser's approach, his perspective, are unduplicated among most students of German politics. He not only blends the skills of journalist and scholar, he has also in essence fused the role of objective observer, impartially allocating praise and blame, with that of the committed partisan of the Franco-German tradition, to which he is both heir and benefactor.

His book, nonetheless, contains many occasions for critical slings and arrows; indeed the expanded nature of this volume has made some earlier warts all the more visible. It is, for example, not at all clear whether a general audience will be able easily to thread its way through all of the dates, place names, scandals, and curriculum vitae, however exhaustive and interesting the collection may be for its own sake. Grosser's analysis also ends far short of the purported 1970 cut-off points; he gives embarassingly short shrift to the significance and the achievements of the first SPD-led coalition in postwar history. His treatment of German foreign policy in general and *Ostpolitik* in particular verges on the banal. And however laudable his all-German intentions, the chapter on East Germany, with its superficial and outmoded substantive discussion and the clear disinterested flavor stemming from Grosser's total detachment from this Germany, would have been better omitted.

On balance, however, the evaluation of *Germany in Our Time* must be positive. As a handbook introduction, a ready politico-historical reference, as a context-setting reading for undergraduate classes, it will be of considerable benefit to multiple audiences. And it may well serve as another stepping-stone to still another updated and expanded examination of the centuries-old question, "Who are the Germans and what is their fatherland?"

Even so, this reviewer longs for a different Grosser book, faintly evident here in the occasional deft aside or sweeping generalization. How and when will Grosser (now entered in the highest of French intellectual orders) be persuaded to write on the process and the limits of postwar Franco-German *rapprochement*? To be sure, it will not be a subject to be approached chronologically or an analysis which will smell as sweetly of undisputed or continuous success as the story of postwar German rehabilitation. But we will all be the poorer in this era of détente and transition if Grosser continues to deny us the analytic fruits of his sympathetic understanding of two long-warring cultures, of his unflinching objectivity and critical judgment in the face of questions on which European political history turns, and of his unique insights as a skilled participant-observer. The wait will not be made easier by further tantalizing in an expectable *Germany in Our time, 1975*.

CATHERINE MCARDLE KELLEHER
Inter-University Consortium for Political Research, Ann Arbor

The Dominican Republic: Rebellion and Repression. By Carlos María Gutiérrez. (New York: Monthly Review Press, 1972. Pp. 172. $6.95.)

Carlos María Gutiérrez is an Uruguayan journalist and "man of the Left" who spent approximately two weeks in the Dominican Republic in late April/early May, 1971. Although the book begins in the first paragraph with a number of provocative statements about Dominican politics generally, by the author's own disclaimer in the second paragraph no effort is made to develop these themes in the body of the work. Rather what we have is a collection of feature stories and vignettes written for a news service during the author's brief stay in Santo Domingo, reflecting both the pressure to turn out a feature-length piece every day and the brevity of the author's acquaintance with the Dominican Republic.

The book almost, but not quite, falls into the *APSR's* category of a work which is of too poor quality or not significantly relevant to the interests of political scientists to merit review. This is not because Gutiérrez is a man of the Left (in fact, as we shall see, that is what eventually makes the book worth looking at), but because the book is exceedingly superficial and full of errors. That, unhappily, is characteristic of virtually all reporting on Latin America by junketeering journalists, both North American and Latin American. Under pressure of deadlines and in the absence of any more solid acquaintance with the country to which they are briefly sent to cover "the crisis of the minute," the almost inevitable result is interpretations based on surface observation and preconceived notions that in the Latin American context are usually misapplied or simply wrong. This is unfortunately as true of the Marxist interpretations which Gutiérrez brings to bear as it is of the liberal-democratic prejudices with which most North American reporters seek to interpret Latin American events. Suffice it to say that the Gutiérrez book is frequently flawed by such factual errors and misinterpretations that its usefulness even in terms of its own limited goals is seriously diminished.

What ultimately makes the book worthwhile is the series of interviews Gutiérrez records with prominent Dominican Leftists. Included are interviews with Dominican Revolutionary Party (PRD) chieftain Juan Bosch (who has since resigned that party to form his own group); Marxist

intellectual Juan Isidro Jiménes-Grullón; a spokesman for the "Marxist-Leninist" Dominican Popular Movement (MPD); Communist Party leader Narciso Isa Conde; and a representative of the Camilo Torres Revolutionary Committees. These interviews are provocative, for they not only show how different are the perspectives and strategies of the various Leftist groups, but also how deep are the personalistic and ideological divisions among the Left. They demonstrate the Left's weakness and fragmentation, its ineffectiveness in organizing trade-union and peasant support, and its frustration as a result of the political strategies pursued by President Joaquín Balaguer. The Left remains largely ineffective not only because of the terror and oppression inflicted on it by the Dominican military and right-wing terrorist groups (themselves made up of many ex-MPD members), but also because Balaguer has proved an adept politician and manipulator who has consistently outmaneuvered his opponents, co-opting or subverting the opposition leadership, undercutting their arguments by stimulating much economic growth and considerable social progress, and commanding not only the Right and Center of the political spectrum but, through patronage and his control of the governmental machinery, building his own strong backing among those groups, workers and peasants, to whom the Left looks for support. Particularly in the interview with Bosch, one senses the hopelessness of the revolutionary position, the frustration of a whole series of leftist strategies over the last decade, and the utter dependence on outside circumstances (Bosch concludes that the Dominican "oligarchic front" is unlikely to be broken by indigenous forces but will crumble only when the U.S. economy, on whom he says the oligarchy is dependent, itself collapses).

Having shown us where power and influence do *not* lie in the Dominican Republic, Gutiérrez's book is woefully weak on telling us where they do lie and how the increasingly complex Dominican system actually functions. Only three or four paragraphs in the book begin to get at the root causes of Dominican underdevelopment; the legacy of the Trujillo era; the competition of rival civil-military factions for the immense spoils that go with capture of the state machinery and economic enterprises; the fleeting, unstable, often nonideological nature of political loyalties, and hence the absence of virtually *any* kind of institutional structure, be it on the Left, Right, or Center. That is the kind of analysis the Dominican Republic calls for; sadly this book does not come to grips with these issues at all.

HOWARD J. WIARDA
University of Massachusetts

The Long March to Power: A History of the Chinese Communist Party, 1921–72. By James Pinckney Harrison. (New York: Praeger Publishers, 1972. Pp. 647. $6.95.)

Professor Harrison's book is synthetic history at its best. Harrison has acquired the craft under the steady hand of Professor C. Martin Wilbur, who is perhaps the most assiduous and persistent Western student of twentieth-century China. With solid grounding in Chinese language, Harrison has done the patient work of sifting through documents and checking through all relevant Western studies. He gathers the relevant information from a wide variety of diverse sources, and with an eye on the main thread, weaves a lucid tale. It is the best available text for an introduction to the history of the Chinese Communist movement before 1949.

Although Harrison began the work for this volume many years ago, he completed it at an opportune time. He draws heavily on a busy decade of monographic research. Furthermore, before going to press, he had the benefit of Chang Kuo-t'ao's massive recollections of the movement up to 1938, with rich personal detail never before available (and now happily rendered into excellent English by Robert A. Burton and published in two large volumes by the University of Kansas Press). He further benefited from the best of the Kuomintang studies of the Communist movement, a recently published three-volume history and collection of documents, *An Analytic History of the Communist Movement* (Taipei) by Warren Kuo. This work provides rich new documentation, albeit selective and not without bias. And the new work by Jacques Guillermaz (the first volume, translated into English and published by Stanford covers the movement before Communist takeover, and the second not yet translated from the French covers the movement after 1949) provides especially rich detail on the military side.

Harrison would have been well advised to stop the book with the Communist rise to power in 1949. The 100-odd pages on the Communist state from 1949–1972 does not compare with the richness of the 500-odd pages on the rise to power from 1921–1949. It is not that Harrison does not make a serious effort to give an overview, but state power brings new complications in all directions, and Harrison's own work has been concentrated in the 1921–1949 period. The part after 1949 is like a hasty appendage to a masterful study.

What then do we have from a first class American synthetic history of the Chinese Communist movement? We have a rich and judicious account of the role of Soviet advisors, of internal squab-

bles, of the changing policy lines, of the escape from the Kuomintang, the Long March, Yenan, World War II, and the Civil War. Some political scientists may complain that the work is lacking in theory, but we China specialists who have seen misleading and distorted pictures at the hands of "theorists" can only be delighted at seeing the basic story told correctly.

One frustration for any scholar of the period is that on many tangled issues, different documents tell conflicting stories; and there is always the nagging question of whether some partisans have invented or rewritten the documents. Harrison is a craftsman in checking the sources and judicious in giving the reader a sense of the nature of the documentation, but still, many statements have to be prefaced by "If these sources are correct, . . ." or "It may therefore be presumed that. . . ."

Another frustration is that the younger generation of American China scholars, in government and academe, lack a feel for Chinese politics. This deficiency is due not to their inclinations but to the nature of Chinese-American relations. Harrison, like most other Americans, has relied overwhelmingly on documents. He has gone through more documents than Guillermaz, who provides the only comparable integrative effort; but Guillermaz spent many years in China and knew many of the leaders personally, and through his sense of Chinese political life, he can often zero in on what was going on at the deeper level, when the good American historian is filling in with lots of detail which does not always separate the vital from the peripheral. At times, in reading Harrison, one gets the urge to rush back to Chang Kuo-t'ao with its richer, subtler personal picture even when Chang's memory cannot compare in accuracy with Harrison's documentation.

We have also not yet advanced our study to the point where the history of the Chinese Communist movement can be woven in with a rich overview of political events and of the broader social context. The documents of the movement do provide a view of the Communist leaders. But it will be some time before the general sociopolitical history of Republican China is sufficiently pulled together to provide the background that a historian of the Chinese Communist movement can relate to his own studies. In the meantime we can be grateful to Harrison for providing a reliable overview, a text to give students, and a guide for the more detailed studies and source materials on this history of the Communist movement.

EZRA F. VOGEL

Harvard University

Soviet and American Society: A Comparison. By Paul Hollander. (New York: Oxford University Press, 1973. Pp. 476. $12.50.)

Comparative studies of Soviet and American societies were much too long dominated by the convergence debate. Since for each two objects one can find a level of abstraction high enough for these objects to look exactly alike (or, conversely, sufficiently low to expose their distinctiveness), the ill-famed debate has been admittedly abortive from the start and reflected much more on the participants' political sympathies than on the substance of the matter allegedly under discussion. Paul Hollander—both by theoretical design and for the method of research and presentation he had chosen—swept aside the traditional idiom and offered an entirely new approach to the task of comparison. It is just as well that he did, since on the eve of Pax Kissingeriana it is only natural to expect many a political scientist to jump headlong onto the convergence bandwagon, thereby following the trail blazed by some of his predecessors inspired by the war-time alliance and, later, by the double luck of the death of Stalin and the dying out of McCarthyism. These old specters of convergence hung too high, in the rarefied air of grand theory (Sorokin's universal civilization cycles; Galbraith's industrial society type; Parsons's "democratize or perish" principle), to be effectively forced into a face-to-face confrontation with facts of a more mundane nature. Every future specter of the kind, if reborn, will have to face this legion of well checked, fact-like stubborn facts, which Hollander has so painstakingly and sedulously summoned and got straight.

To be sure, the relevance of Hollander's contribution as "a convergence debate to end all convergence debate" is somewhat diminished by his side-stepping rather than frontally attacking the main issues the preachers or adversaries of convergence tried to grapple with: whether the two societies *become* more similar or less similar to each other in the process of their respective evolutions. One of the few virtues one could concede to the convergence debate was that it judiciously underscored the necessity of the historical analysis. The outspoken neglect of this necessity is the paramount weakness of Hollander's analysis. To him, the two societies are firmly fixed types, rather than living and changing entities. He compares instantaneous snapshots rather than tendencies. He captures the two societies in the states which by historical chance happened to be contemporaneous, but he fails to monitor the paths which led to these states or the still unfolded prospects for the future. Having made this decision, he could only create a flattened, purely phenomenal picture bereft of temporal depth—

one which is not immediately translatable into the convergence idiom. If a convergence addict wishes strongly enough to do so, he can still, unfortunately, dismiss Hollander's erudition as a recording of a transient episode.

Worse still, the lack of diachronic dimension has not been made up for by a cohesive synchronic model which could assign relative importance to disparate facts collected in the book and recited in the same pitch. Thanks to the author's scrupulous homework and smooth, lucid narrative, the reading is rewardingly illuminating; but the question "so what?" will be inevitably asked by a diligent reader and, as far as I am concerned, will remain vexingly unanswered. The book's structure reminds one of a perceptive, but picaresque, traveller's diary: a series of sightseeings, each vividly and perspicaciously described, but subject to no other logic than the traveller's shifts of attention. Time and again, the reader is pleasantly struck by particularly insightful observations: when, for example, Hollander discusses the "projection effect" acting upon the perception of the other side (pp. 26, 153); when he picks the non perception of alternatives as a viable form of legitimation (p. 72); when he points out that all evaluations of the genuine role of coercion are inconclusive in the absence of free expression (p. 94); when he reveals the role of secret and secrecy as a factor of power (p. 104). The whole book is virtually ablaze with such scattered insights. Sometimes whole chapters rise to the level of delightful self-contained studies in their own right (like the one concerned with the phenomena of students' unrest and the "New Left"). But having finished the book and having enjoyed many intellectual delights, the reader cannot help asking whether Hollander has not gone too far in his obstinate intention to keep closely to the thin crust of phenomena and to stave off all theoretical commitment.

That Hollander's findings do not articulate into a comprehensive system is more than a minor irritant. Hollander seems to have little use for causal explanation, and consequently his occasional causal statements tend to be perfunctory, and sometimes even flippant. Thus, Hollander wishes his reader to believe that the character of political parties (in parliamentary democracy) is not determined by their exercise of power but, of all things, by their social base (p. 46); that the bureaucratization of the Soviet system was caused by the Leninist obsession with intra-party controls and centralization (p. 49); that the Soviet stratification system has been set up in a deliberate way (p. 214); Hollander assumes that there are "values of the society" (p. 123 ff), as if Shils or Stinchcombe never wrote a word; he thinks one can discuss the peculiarities of the Soviet social structure without paying any attention to the abolition of private ownership, i.e., of the non-political bases of social power, and the ensuing blend of the "public" and "private" power etc.

But even more important, theoretical models are particularly necessary in comparative studies —since they and only they can provide the guidance about what should be compared with what. Having located his task neatly and squarely on the phenomenal level, Hollander can rely only on the misleading similarity of institutional names and so perpetuates (though inadvertently) the thoughtless habit of taking the cognitive net produced by the dominant political science vocabulary for the "objective," immediately given, empirical and factual, reality. It did not occur to Hollander that the same functions may be performed in each society by entirely different (in look and in name) institutions. Should one compare political parties, or ways and means of mobilization, elite training, eliciting support, interests integration etc.? Should one compare the Supreme Soviet with the American Senate, or rather the latter with the Central Committee of CPSU? Should one compare elections, or rather the means the two countries apply to live up, or to pretend that they live up, to the shared assumption of the sovereignty ultimately vested in the people? Should one compare distributions of income or rather specific ways in which control of some people over the situation of others is generated and sustained? There is no end to such questions, and no one of them may be as much as posited without reference to some kind of a theoretical model. The idea that in order to embrace reality more feasibly one has to slip off his theoretical armor is an old fallacy which Hollander's book has in no way disproved.

All this said, Hollander's study provides more detailed, tested, and intelligently conveyed information on the two societies than any other book on a related subject. For this reason it will remain, and justly so, a most comprehensive reference book for everyone who may contemplate comparative analysis of the two societies most jarringly dissimilar and most perplexingly gravitating toward each other.

ZYGMUNT BAUMAN
University of Leeds

The Revolutionary Potential of Peasants in Latin America. By Gerrit Huizer. (Lexington, Mass.: D. C. Heath & Co., 1972. Pp. 237. $12.50.)

This book has a basic premise: "there is not a characteristic and generalized peasant mentality which is basically different from the mentality of other people" (p. 19). The main purpose—and value—of the book is to show that rural community development programs not only have to be

designed *for* the peasant but also *against* the repressive structures and institutions that entrap and enslave peasants.

Huizer's argument runs on two tracks: the practical and the theoretical. Huizer, the seasoned activist, draws from his experience to criticize the use of compromise models of social change in the design and execution of community development programs. According to Huizer, the record shows that latifundio owners and rural elites are adamantly opposed to any effective program that is likely to undermine their economic power and political influence. And, by definition, the development of peasant communities cannot take place except at the expense of that power and that influence.

In the second—and best—chapter of his book Dr. Huizer narrates his involvement in community development projects in the villages of San Luis (El Salvador) and Partinico (Sicily), and in the Punitaqui Valley (Chile). In every case, peasant apathy and distrust evaporated (1) when the peasants realized that they would control the program, and (2) when the potential benefit of the program was perceived as greater than the cost of reprisals that it would elicit from the landed elites (p. 47). Huizer concludes this chapter by arguing that peasant apathy toward development programs stems from the realization that such programs are no match for the influence and control exercised by the landed elites.

Huizer's theoretical discussion is somewhat less satisfying. He criticizes a number of scholars for their inability to see the real causes and nature of the peasants' passivity and resistance to change. This critique includes Banfield's "amoral familism," Foster's "image of the limited good," the concept of the "patrón configuration" as formulated by Loomis, and the "encogido syndrome" of Erasmus. Huizer believes that these concepts fail to incorporate the nature of the social system in which the peasant is entrapped because

> Banfield, Foster, and Erasmus, although aware of some of the wider and political implications . . . limited themselves to the introduction of concepts . . . which only describe or qualify the behavior of peasants, but do not clearly indicate and explain the motivation behind it (p. 60).

Huizer stands on fairly solid ground in this evaluation, but he then turns around and introduces Holmberg's notion of a "culture of repression." The substitution violates the logic of Huizer's own argument, and introduces a great deal of circularity in the discussion leading him to claim that "only a dramatic change gives the peasants incentives to give up their distrust and resistance to change" (p. 65). If peasants are exposed to a "culture of repression," if they are part of that culture, how can one overcome their distrust in a short period of time? On the other hand, this substitution very much weakens Huizer's useful conclusion, namely, that the peasants' attitude is a healthy reaction of normal people to a pathological social climate (p. 65). In summary, Huizer dilutes his observation that the distrust of the peasant is a reflection of a power calculus with an unnecessary substitution of concepts.

In chapters four and five Huizer shows that peasants are capable of organizing reform movements responding to charismatic leadership, relative deprivation, and violent counter-reform moves from the landed elites. In these chapters he repeats several times the point that peasants utilize violence as a last recourse when they have already exhausted all other possibilities. He also emphasizes that most of the time, peasant movements have to "go it alone," and that these movements have been more successful when a regime has found it necessary to give them arms and a paramilitary organization.

In the vast expanse of subject matter that one may characterize as "the agrarian question," Huizer's contribution is limited to the specific point of how peasants can become "power contenders." But in this particular aspect the book is extremely useful. To the practitioner the book sounds less defensive than similar statements made by more prominent actors in peasant politics who find it necessary to justify some of their acts.

Huizer offers a nice blend of humanistic concern—short of the usual paternalism—and subdued radicalism. The title is deceptive; one finds no cries for immediate revolution, no sweeping generalizations about its inevitability. But this is a radical book: it correctly identifies the fallacy of nonradical rural development. Huizer's emphasis is on Coser and not on Marx, but he goes to the root of his subject: Rural development involves interest conflict and any program that ignores this is going to fall short of its objective.

ENRIQUE BALOYRA
University of North Carolina, Chapel Hill

Czechoslovakia and the Absolute Monopoly of Power: A Study of Political Power in a Communist System. By Barbara Wolfe Jancar. (New York: Praeger Publishers, 1971. Pp. 330. $17.50.)

With the generally recognized demise of the usefulness of the concept of totalitarianism in studying Communist regimes, an effort has been made to find more appropriate analytic tools. In this book Professor Barbara Wolfe Jancar ambitiously but not entirely successfully proposes the concept of absolute monopoly of power, in the case of the communist regimes held by the Com-

munist party, as a theoretical tool broader than totalitarianism yet more pertinent to Communist regimes than the basically democracy-oriented approaches of elite studies, group theory, and the like. In particular she advocates greater attention to the ideological factor essential to these regimes, yet does not herself devote very much space to this component, neglecting entirely those aspects of the Czechoslovak reformers' ideas devoted to Marxism-Leninism and a new approach to socialism. Within her model, many categories overlap and are insufficiently differentiated from the point of view of relative importance; moreover some of the basic assumptions are unclear (e.g., the idea that power is exclusive but not total is is probably true but not very helpful within the already posited framework of a one-party state). This lack of clarity may be due to a more basic flaw: the author states rather than proves that these societies are absolute monopolies of power and then asserts the obvious, that some of these monopolies were broken, while others were not. Some of Dr. Jancar's analyses of why the monopoly broke down are excellent (viz., the treatment of the Slovak Communists as a built-in faction) but in other areas (economic reform, the Party, the reviews of the trials of the 1950s) her analyses are confused and even contradictory.

To some degree the fault may lie in the failure clearly to distinguish between periods (pre-1968, 1968, post-invasion); for example, the conclusion on the erosion of the monopoly of power at the end of the chapter on the ruling group would seem to apply to the post-invasion period rather than the period discussed in the chapter. And the author somewhat surprisingly contends that the gap between the ruling group and the rank and file was identical in the Novotny era and throughout the Prague Spring. In one place she speaks of a "real revolution" (p. 278) while elsewhere she says that the erosion of the monopoly was "potential rather than actual" (p. 235) in the important area of law and state power; in yet another place that the monopoly eroded but the regime was "very definitely in command" (p. 279) and still elsewhere, without any indication of the period intended, "the ruling group found itself unable to exert exclusive authority over . . . the Communist Party" (p. 182). Paradoxically she claims that "Dubcek and his supporters were not absolute monopolists by conviction, for they shared with their people a democratic past" (p. 91) but later explains the failure of the Dubcek regime to abandon absolute monopoly thus: "The 1968 Communist leadership, *like its predecessors* (italics mine), was too narrowly trained in the principles of absolute monopoly." The paradox is not entirely the author's creation, but at the very least an explanation is called for.

One of the author's basic hypotheses is "that under pressure during the 1960s to de-Stalinize, the Czechoslovak leadership followed the common pattern of attempting de-Stalinization. To win an independent position vis-à-vis the Soviet Union, it has no other course than to mobilize popular support for liberalization" (p. 37). Such a thesis is astounding with regard to the 1960s until 1968 and inaccurate even for the Dubcek era. Pressures for de-Stalinization were met reluctantly and only partially by a recalcitrant Novotny (who, despite a brief altercation with Brezhnev, based his position on Moscow's support) leading finally to liberalization and democratization in the Prague Spring (for which popular support was absolutely vital). This drive for a type of socialism suited to Czeshoslovakia's needs and traditions stopped short of a demand for independence from the Soviet bloc, i.e., de-satellization, and, indeed, it was only the interference from the Soviet Union and its allies which prompted the eventual emphasis on sovereignty. Mrs. Jancar herself seems to realize this fact, for, despite the stated intention of proving her hypothesis, she refers to it almost nowhere in her study and even in her chapter on sovereignty presents a much more accurate image of a movement which only belatedly took on signs of national communism (desatellization) as distinct from what Brzezinski calls domesticism. Moreover, one of the strengths of her book is her grasp of those measures which alarmed the Soviets (the pluralism, signs of a breakdown of the leading role of the Party), with the possibility of breaking off from the Warsaw Pact or from the close alliance with Moscow remaining a potential rather than actual source of concern.

There are also a number of factual errors: on p. 187 it is inaccurately stated that the workers achieved their demand that Smrkovsky stay in office in December, 1968. The renegade Locomotive Workers Union did *not* achieve a *modus vivendi* with the railroad and the central trade union authorities (p. 185); in fact each side initiated sanctions and the workers eventually were completely defeated. The Slovak CP Congress during the invasion *did* (contrary to p. 178) approve the 14th "Vysocany" Congress, withdrawing this resolution only after Husak's intervention. Rudolf Barak, who challenged Novotny by the threat of exposing the latter's implication in the trials of the 1950s was not, as the author claims (p. 106) himself innocent of such a past. He became Interior Minister in 1953 (not 1964, p. 106) and was implicated, at the least, in the 1954 trials. Slovak Communist leader Clementis (sic) was *not* a Jew nor tried as such (p. 104); student leader Jiri Müller was expelled from the University and drafted in 1966, i.e., one year be-

fore the Strahov demonstrations, *not* in 1967 (p. 192). The workers (together with the "bourgeois intellectuals" p. 205) were not the major actors in the 1960s, for as the author herself pointed out elsewhere they were regrettably late in joining the reform movement.

The book is nonetheless eminently readable, and the author draws well on the post-invasion purges to prove her interpretation of pre-invasion events. She accurately if briefly pays tribute to the role of the mass media, and she shows a keen awareness both of the internal political problems faced by the Dubcek regime before and after the invasion and of the Soviets' concern over the direction of the Prague Spring. The analytic-theoretical approach is commendable if frequently weak and inconsistent. Its application to Czechoslovakia, with the intention of explaining the events of the 1960s is generally unsuccessful and often confusing.

GALIA GOLAN

The Hebrew University of Jerusalem

Inflation and Economic Development in Brazil, 1946–1963. By Raouf Kahil. (London and New York: Oxford University Press, 1973. Pp. xvi, 357. $32.25.)

In their search for explanatory precision, political scientists have been turning more and more frequently to economic determinants in the political policy arena. The power of economic factors to explain political outputs has led some writers to despair that political variables may make no difference at all. It is therefore interesting to find an economist, writing for other economists, who concludes a detailed study of Brazilian inflation, 1946–1963, by stating "There is reason to think that the factors ultimately responsible for the inflationary process were chiefly political, not economic" (p. 330).

Kahil's analysis is oriented by the structuralist/monetarist controversy of the early 1960s. He adopts the structuralist interpretation as a working hypothesis, though he is far from committed to that school. He identifies three principal characteristics of the structuralist argument. First, "the underlying causes of inflation in underdeveloped countries are to be found in their systems of production and/or trade organization" (p. 30). Second, in contrast to monetarists, who attribute inflation to "rising money wages, expanding bank credit and mounting budgetary deficits . . . structuralists maintain that [these phenomena] are not the causes, but the consequences of the rise in price" (p. 30). Third, given these conditions, "the attempt to apply stabilization policies in underdeveloped countries is not only futile, but positively harmful" (p. 31).

Kahil tests these propositions from both sectoral and longitudinal perspectives. In chapters II through V he provides a detailed analysis of four sectors considered crucial in the structuralist interpretation—agriculture, industrial development and urbanization, capital supply, and the external sector. In chapters VI through VIII he integrates the performance of these sectors into a discussion of the inflationary process which he divides into four phases: 1946–48, 1948–54, 1954–58, 1958–63. Contrary to the structuralist argument, he finds the economic conditions in each sector to have been favorable at critical points in time for the maintenance of economic development without inflation. Each phase was characterized by overall growth accompanied by a moderate inflation which responded well to briefly applied stabilization measures. Each was terminated by a rapid increase in prices which Kahil maintains was caused by the government's tendency to abandon stabilization measures just as they were becoming effective and to intervene in the economy with inconsistent, untimely, and generally counter-indicated policies. He can find no fundamental economic factors responsible for the inflationary spiral and no sound economic rationale for Brazilian governments' economic policies during the period. He concludes, therefore, that the galloping inflation which plagued the economy in the 1960s and which contributed heavily to the military coup of 1964 was the net result of politically motivated government policies, and not of structural weaknesses in the Brazilian economy.

The material in chapters VI to VIII is of particular interest to the political scientist because of the discussion of the rationale behind the selection of and the impact of government decisions. From a detailed reading of ministerial reports and special studies, the author culls indicators of the political considerations which confronted decision makers and which led to the policy choices. His interpretation of the political impact on economic policy making lends support to the expanding-sum game or grand coalition theory of Latin American politics advocated by Anderson, Kenworthy, Schmitter and others.

> So long as inflation remained relatively moderate and was accompanied by rapid growth . . . substantial resources could conceivably be diverted to one section of the population without any other section necessarily suffering, even temporarily, a significant reduction in welfare. . . . Thus Brazil's successive governments had little inducement to introduce measures of stabilization which, if successful, would have deprived them of formidable economic and political power afforded by inflation and which would have aroused the resentment of important pressure groups . . . (pp. 333–4).

The cumulative effect of the increasingly drastic distributive policies was both economic and political chaos, which led, ultimately, to the 1964 crisis and coup.

Kahil touches only briefly on these factors, being of the opinion that "psychological and political considerations lie outside the purview of economists" (p. 282). His study nevertheless provides a wealth of information for the scholar interested in the politics of Brazilian development. As a case study, couched in a framework of general development economics, the material is also suggestive of processes likely to be seen in other developing countries.

Given that much of Kahil's study is based on published material complete through the end of the decade of the 1960s, it is unfortunate that the author did not use it to carry his analysis beyond 1963. Effective control of Brazilian inflation was not achieved until 1969 or 1970, and we have few studies of recent Brazilian development, in contrast to the wealth of material available on the events which led to the 1964 military "revolution." The political scientists, suspecting at least a partial continuity of policy between pre- and post-revolutionary governments, would be especially interested in a comparison of the policies of stabilization and growth of post-1964 governments with those of growth and inflation of the earlier governments.

In conclusion, Kahil offers some very useful material, both data and interpretation, along with some very heavy reading for the political scientists interested in the politics of development. The study can be recommended to both Brazilianists and students of other Latin American countries. Unfortunately, given the outrageous price attached to the book, it is unlikely to reach the shelves of many scholars, economists, or political scientists.

MARGARET DALY HAYES
Indiana University

On the Road to Communism: Essays on Soviet Domestic and Foreign Politics. Edited by Roger E. Kanet and Ivan Volgyes. (Lawrence: The University of Kansas Press, 1972. Pp. 212. $8.50.)

Originally presented in 1967, this collection of essays concentrates mainly on the Khrushchev period. There is no common unifying theme. Part I, "Essays on Soviet Domestic Politics," is concerned with various aspects of political socialization. "Ideological Administration Under Khrushchev" by Erik P. Hoffman, "A Conceptual Framework for Mass Political Socialization" by Robert Sharlet, and "The October Revolution and Soviet Education" by Nicholas DeWitt, deal with the subject directly; "Flexibility and Dogmatism; The Ambiguous Legacy of Leninism" by Paul Roley, "Capital Punishment in Soviet Criminal Legislation, 1922–1965: A Code Content Analysis and Graphic Presentation" by Will Adams, and "Soviet Historians Review Their Own Past; The Rehabilitation of M. N. Pokrovskii" by George M. Enteen, have some relevance to it. The theme in Part II, "Essays on Soviet Foreign Policy," (covered by "Changing Soviet Attitudes toward the Developing Countries" by Roger E. Kanet, "The Arabs, Marxism and Moscow: A Historical Survey" by Jaan Pennar, and "The Soviet View of African Socialism" by Arthur Jan Klinghoffer), is the development of Soviet policy toward selected Third World countries. "Strategic Aspects of the Sino-Soviet Dispute" by Chae-Jin-Lee, examines China's perception of her power position between the United States and the USSR. Contributions vary from survey to analysis of empirical research.

Roley's short opening chapter analyzes the dichotomy in Lenin's thought between his inflexible emphasis on the "organizational principle" and his basic political pragmatism and tactical flexibility. Roley considers the former not "the essential characteristic of Leninism" (p. 7), but "a direct outgrowth of (Lenin's) political realism," considered by the author to be "the main characteristic" of the Leninist creed (p. 10). Regrettably the dichotomy is not traced in application to recent changes in Soviet ideological interpretations vis-à-vis arguments of revisionists and dissenters.

The framework for the socialization theme is provided by Sharlet's short contribution. It defines the term "mass political socialization," lists Soviet socialization agents, and briefly reviews the two main (according to the author) socialization periods in the USSR, with 1928 as the divider. No new ground is broken. One misses an attempt to single out the post-1956 period, and the socialization needs in an advancing industrialized society with its burgeoning pluralism.

It is the Hoffman chapter, dealing with post-1956 efforts to revitalize the ritual of the Party's ideological administration, that provides insights into the new needs of Soviet political socialization. Based on extensive empirical data this contribution analyzes the mechanics of the work of the Agitprop apparatus, its system of vertical and horizontal communications, and its "administrative efficiency." Hoffman finds that despite its formidable formal powers, Agitprop's ability to function effectively depends on its ability "to persuade provincial officials that what it wants of them is in their own best interests" (p. 40) and thus clearly links the socialization process to the pragmatic requirements of the Soviet state and its ruling elite. It would have been helpful to find an assessment of the reasons for the Khrushchev

decision to intensify and improve the work of the ideological apparatus as well as an assessment of the effectiveness of new methods in changing political behavior of the Soviet populace.

The pragmatic character of Soviet socialization is also illustrated in DeWitt's interesting analysis of the evolution of Soviet education. Its main thesis is that following an initial period of experimentation, the Soviet educational system has been geared to meet the practical economic needs of the society—"to implant . . . some kind of basic functional skill or specialty which would enable the state to integrate an individual into productive employment and which would help to retain him in that employment" (p. 74), and that the Soviet citizen has responded by turning educational opportunities thus provided to maximum personal advantage.

The Adams chapter offers a labor-consuming analysis of the Soviet legal attitude to capital punishment as seen through content analysis of the 1922–1965 criminal codes. As Professor Adams cautions, methodological limitations allow for the assessment of formal legal attitudes only.

The final paper in Part I examines in depth recent efforts of some Soviet historians to rehabilitate Pokrovskii's views. While unresolved so far, the debate offers fascinating insights into the current state of Soviet historiography, in which elements of historical objectivity and legitimate differences in interpretation intertwine with the need to support past formulations. A short synopsis of Pokrovskii's views and the main thrust of Stalinist criticism of them would have been helpful for the uninitiated reader.

The Foreign Policy section is shorter and more cohesive than the section on domestic politics. The first three chapters basically cover the same ground, developing a general thesis that the evolution of the Soviet attitude toward Third World states followed closely the pragmatic needs of Soviet foreign policy. There are some redundancies, especially in reference to the interpretation of the political nature of the developing countries: from the Stalinist concept of "bourgeois-nationalist governments," through Khrushchev's "national democracies," to the current concept of "revolutionary democracies." Kanet concentrates on the evolution of Soviet policy towards the Third World in general; Pennar on the analysis of Soviet policy towards the Arab world; and Klinghoffer on the Soviet evaluation of African socialism. The three contributors empirically validate the thesis that *realpolitik* has governed adaptations of Soviet theory but demonstrate also the dangers of distortion in analysis of current events inherent in ideological bias. The case in point has been the Soviet persistence in applying class analysis to African politics (Klinghoffer, pp. 187–8). The three chapters all mention the existence of the Soviet Union's own model of development in Soviet Asian areas (pp. 151, 174, 186), but none develops the point. Domestic political implications (particularly in the theory and practice of the nationality policy) have been of considerable importance in the formulation of Soviet policy toward the Third World, particularly in its theoretical aspects. Minor discrepancies can be seen. Thus, Kanet (p. 148) quotes Professor Potekhin, the late Soviet Africanist, to the effect that "classes, as defined by Marxism-Leninism, did not exist in Africa"—a concession to the African leaders' own view. Klinghoffer, on the other hand, also citing Potekhin, asserts that African socialists' claim that Africa is a classless society is "disputed by Soviet political experts who aver that the African countries are now experiencing bitter class struggle . . . (p. 179). One may also question Kanet's statement (p. 143) that the *rapprochement* with non-Communist nationalist regimes was an entirely post-Stalin phenomenon.

The last chapter appears dated, after the Sino-American détente. In retrospect, however, Chae-Jin-Lee's analysis provides a good background for an understanding of Chinese motives behind the détente. A rapprochement with the United States seems a logical way out of China's strategic problem of power inferiority vis-à-vis both her "imperialist" enemy and her erstwhile ally. Where the author erred (along with most of us), is in his choice of which one was, to the Chinese, a "lesser evil." On p. 207 he says: "It is not entirely improbable that, as political realists, the Soviet and Chinese leaders, if faced with a vital common threat from the United States, might forge at least a temporary alliance." A substitution of "American" for "Soviet" and the "Soviet Union" for the "United States" in this sentence would have been warranted on the basis of the preceding analysis.

TERESA RAKOWSKA-HARMSTONE
Carleton University, Canada

The Politics of Economic Reform in the Soviet Union. By Abraham Katz. (New York: Praeger Publishers, 1972. Pp. vii, 231. $15.00.)

The title of this book is somewhat misleading, for the author focuses primarily on the economic aspects of various economic reforms in manufacturing, rather than on the politics in the broad sense of the term. Thus we learn a good deal about the nature of particular reforms and what some of the top political leaders were doing or saying at particular points in time, but little about politics within the party, particular conflicts and accommodations among different interest groups, or administrative dynamics. The book is based primarily on Soviet sources and covers the period

from 1920 through 1971 in chronological fashion, focusing most attention on the period following the death of Stalin.

Professor Katz's forte is his close attention to specific policies, e.g., the introduction of particular types of bonus schemes for plan managers or the types of mandatory plan goals to be given to them. He tries to link the discussion and implementation of such measures to major trends in the economy. His analysis of some major policy problems such as the proportionalities question (the relative importance of investment and production in particular sectors) is lucid and interesting.

The major drawback of the book is that it tries to cover too much in too short a space. By focusing primarily on the industrial sector, it virtually omits considering policies in other sectors. In trying to sketch some complicated issues in a few lines, Katz does not do justice to the complexities of the subject he does discuss, e.g., his parallels between organizational problems in the '20s and in the post-Stalin period.

The first fourth of the text, which brings the reader from War Communism to 1953, includes few materials that are not available in other English language sources. The next quarter of the book, covering the period from the death of Stalin to the preparations of the 1965 reforms, is also too compressed. The reader, for instance, gets relatively little feel for the economic advantages or disadvantages of the Sovnarkhoz arrangement or the administrative changes that subverted this experiment.

The third quarter of the book focuses on the preparation and the announcement of the 1965 economic reform and covers some of the various reform proposals and experiments. These events have also been covered in earlier English language sources, although Katz is able to place these events in greater perspective; his discussion is also more readable because he does not feel it necessary to examine in detail the various schemes proposed by Soviet mathematical economics for optimal planning or functioning of the economy. The last quarter of the book is devoted to a discussion of the implementation of the reform and the process of retrenchment from reform. The reader is left with a number of unanswered questions about why this retrenchment occurred, and whether economic or political or more narrowly administrative factors played the most important roles.

The audience for this book is relatively small. The book focuses on too narrow a range of issues to be very useful to the reader uninitiated into the intricacies of the Soviet economy; and yet it is not detailed or comprehensive enough for the specialist. It is, however, a handy volume for political scientists who are knowledgeable about the USSR and who wish to have a compressed clarification of the recent economic reform proposals and implementation.

FREDERIC L. PRYOR

Swarthmore College

Arab Contemporaries: The Role of Personalities in Politics. By Majid Khadduri. (Baltimore: The Johns Hopkins University Press, 1973. Pp. 255. $8.95.)

Political Trends in the Arab World: The Role of Ideas and Ideals in Politics. By Majid Khadduri. (Baltimore: The Johns Hopkins University Press, 1970. Pp. 298. $3.00, paper.)

These are the latest books to come from the pen of Professor Khadduri, one of the pioneering scholars in the field of Middle Eastern studies in this country and one of the most prolific. In *Political Trends*, the author surveys the history of modern Arab political thought from the pan-Islamist revival to the contemporary period. The political ideas of this era have attracted the attention of several scholars and are the most documented and studied in the field of Middle Eastern studies. By undertaking to write yet another book on the subject, Professor Khadduri aims at a synthesis which highlights the main themes that have preoccupied Arab thinkers and influenced Arab politics. The result is a comprehensive and cohesive account of the diverse philosophies and variations on a theme that constitute contemporary Arabic thought. Professor Khadduri's success may thus signal the need for a new approach to the study of ideology in the Middle East. The study of intellectuals, the social forces that shape their thinking, and their political role are subjects that have not yet received sufficient attention. Similarly, the impact of formal ideology on ordinary people deserves attention. Nationalist and socialist ideas are spreading fast among the peoples of the Middle East and North Africa, the majority of whom are still without formal education. It would thus be quite enlightening for someone to attempt an explanation of this mass phenomenon, especially because most of what is written about the subject is of general and schematic nature.

In *Arab Contemporaries*, Professor Khadduri tries to introduce to the English-speaking world major political figures such as Nasser, King Faysal, Bourguiba, and Nuri al Sa'id, who have occupied the center of the stage in the Arab World. The author includes less-known but important statesmen and political thinkers who had a profound impact on Arab events and thinking in modern times (e.g., as Michel 'Aflaq, Khalid Bikdash and Kamal Junblat). In all, the twelve

biographies of Arab leaders in Professor Khadduri's book constitute a welcome addition to the political biographies library. In contrast to political thought in the Arab World, biographical studies are still lagging, not only in English but also in the Arabic language in which the art of political biography has not yet come to its own. The reader will find Professor Khadduri's contribution to this field informative and useful but should not expect to find a study in depth. As the author states, his purpose is to introduce some leading figures whose ideas and activities have played an important role in Arab politics. However sketchy the various biographies may be, they are based on profound knowledge of Middle Eastern history and politics. Not only has Professor Khadduri been a careful observer of Arab affairs for a generation, but he has also personally interviewed many of the leaders about whom he has written. The result is sound, reliable, and informative.

ILIYA HARIK

Indiana University

The Changing Brahmans: Associations and Elites among the Kanya-Kubjas of North India. By R. S. Khare. (Chicago: University of Chicago Press, 1970. Pp. xxii, 251. $11.00.)

The Kanya-Kubjas are one of north India's most important and conservative Brahman castes. In describing and analyzing the formal organizations and elite behavior of the Kanya-Kubjas, this study makes a welcome, though limited, contribution to our understanding of the changing political structure of castes in contemporary India.

The first part of the study is a straightforward description of Kanya-Kubja (K-K) organizations. It is important because it shows how ineffectual and apolitical caste associations can be. Because political scientists have tended to direct their attention toward castes which have developed fairly effective organizations and assumed political prominence, their efforts tell us relatively little about what is typical of castes. Khare's case study also does not help us establish what is typical, but it does increase our knowledge about the range of caste behavior which must be taken into account by a general explanation of caste change. Khare is a social anthropologist who decided to study the Kanya-Kubjas because he happened to teach in one of the colleges associated with the caste. This simple and pragmatic method of selection leads into a world very different from that usually described by political scientists.

Although Khare is not writing to stress the unimportance of K-K organizations, there is little doubt that they are unimportant. For example, the all-India association has been unable to call a general meeting for the last thirty-two years; K-K educational institutions have been almost exclusively the work of individuals, not the caste; the monthly caste journal is a one-man effort which has only four hundred subscribers (probably much less than one half of one per cent of all K-K families, by my calculations). Khare's description of the K-K associations is not nearly as complete as one would like, but it is also apparent that caste association budgets are very small, that caste associations seldom if ever undertake to direct the caste in any matters, let alone political ones, and that any efforts at social reform which may have been made through caste associations in earlier years have been largely without effect.

The second part of this study examines how caste and class status systems operate to determine the prestige and behavior of members of the K-K elite. These chapters contain some good description: the routine and concerns of a traditional priest; the way government officers use parts of their residences as offices but still maintain a private portion for family and kin; and how government officers avoid responsibility by writing the most noncommittal remarks possible on government files. Much of what Khare reports is not specific to Kanya-Kubjas and will prove valuable to anyone wanting to better understand elite behavior in north India.

The problem with this part of the study is that the status of Khare's observations are unclear. We are told that intensive studies of more than eighty families support the analysis, but the evidence which Khare presents consists almost exclusively of selected illustrations. The range of data presumably at Khare's disposal is seldom explored.

More important, it is difficult to tell just exactly what group Khare is describing in these chapters. He often talks as if his observations were to apply to all Kanya-Kubjas, or at least all prominent (elite) Kanya-Kubjas, but his basis for doing so is not clear. We are not told how the eighty or so families were selected for study, but only that "no sample representation [of what?] is claimed" (p. xix). If, as it usually appears, Khare's studies are only of those families connected in some way with K-K caste organizations, we are dealing with observations that very well may not apply to most Kanya-Kubjas, even elite ones. In fact, one of the major shortcomings of the entire study is the complete failure to place those connected with caste organizations in the larger context of all Kanya-Kubjas. Obvious questions are never asked: Do members of caste organizations differ from other members of the caste? Why do the vast majority of Kanya-Kubjas apparently ignore their caste organizations?

In the final chapters Khare puts his descriptive material into theoretical and comparative perspective. Khare wants to call caste associations

interjacent organizations. Interjacent between what is not always clear, but Khare views the caste association and the behavior of those associated with it as lying somewhere between home and office, informal and formal, tradition and modernity. Khare's basic contention is that the caste association cannot be considered "modern" because it pursues parochial objectives and because it does not measure up to Weber's ideal type for bureaucracies.

There are two problems with this approach. First, it fails to distinguish between a group and the way a group is organized for collective action. Thus Khare cannot view the caste as an ascriptively based [traditional?] interest which may be organized for limited purposes through the structures of a modern voluntary association.

The second problem is related to and probably helps give rise to the first. Khare discusses the contemporary organization of the Kanya-Kubjas without any reference to their traditional political organization. Khare does note that the Kanya-Kubjas, like most Brahman castes, have never had the traditional hereditary *panchayats* (councils) associated with lower castes. But from this lack of traditional formal organization, he seems to infer there was no political structure at all traditionally. He certainly makes no effort to describe how local units of the caste managed, and still manage, to boycott those who violated caste rules. Thus Khare ignores the intriguing question of how Brahman castes operated without panchayats. It appears that it is this failure to consider "traditional" political practices which leads Khare to overlook the discontinuity with the past and to describe caste associations as interjacent organizations. Structurally, caste associations represent a sharp break with the past. They are "modern" in form and there seems to be little to be gained by describing them as "interjacent." Caste associations fall short of Weber's ideal type primarily because they are small and unimportant, not because they are infused with "tradition."

In brief, Khare provides a wealth of descriptive material which should provide an antidote to those studies which perhaps attach too much importance to the role which castes play in contemporary Indian politics. Khare does not always handle this material in a way which I think promotes a more general understanding of the changing political structure of castes, but his efforts deserve attention and they should stimulate others.

RODERICK CHURCH

Brock University, Canada

The Rift in Israel: Religious Authority and Secular Democracy. By S. Clement Leslie. (New York: Schocken Books, Inc., 1971. Pp. 185. $7.50.)

During the past several years, the study of politics in Israel has, as it were, come of age. After an extended period in which most students sought to produce general texts—examinations of politics at the macroscopic level—a growing number of scholars, both Israeli and foreign, have devoted their attention to detailed examinations of specific aspects of Israel's political system. The articles and books which result are refreshing, and fill important gaps in our understanding. And the enterprise is sufficiently new so that we have thus far been spared the tediously detailed examinations of utterly trivial matters which have come to characterize much of our scholarship in other settings.

Against this background, this volume by S. Clement Leslie, a revered British academician and civil servant, at first seems something of an anachronism. It takes as its subject the theoretical tensions which underlie Israel's political and social life, ignoring almost completely, almost proudly, the specific knowledge of the system's inner workings now available. For this reason, it seems at first no more than a restatement of general themes long since familiar to students of Israel's politics, this at a time when the least we have the right to expect of macro-studies is that they build on the careful work that has now been done. But the surprising and happy fact is that Mr. Leslie has managed, after all, to say something new, and to say it with a sensitivity and grace that is quite compelling. It is rather more ambitious to seek wisdom, rather than knowledge, but Mr. Leslie's gamble pays off handsomely.

Specifically, Leslie seeks to interpret the major issues which confront Israel these days—the proper role of religion in the State, the viability of kibbutz utopianism, the place of the Israeli Arab —within a historical metaphysic that is commonly recognized but infrequently employed as an analytic tool. He sees Jewish history as an acting out of the tension between the particular and the universal, and he finds this tension at the heart of much that happens in Israel today. Which is to say that he describes an Israeli present, correctly in my view, that differs rather dramatically from the episodic events which crowd the newspapers. In this view, Israel is the latest and most dramatic example of a historic Jewish effort to deal directly with a problem all men, and nations, face, but none, save the Jews, has taken as its special task. In its current form, the problem is whether nationalism can be benign—or, in Leslie's terms, whether the universal moral code of individual conduct which the Jews have given the civilized world can now be extended to the corporate life of a nation, embodied concretely in the institutions and behaviors of a nation.

Leslie's effort to show how that problem is expressed in the most mundane affairs of state is, in the main, successful, and therein lies the strength

of his approach. The book is not without flaws, among which the most upsetting is Leslie's sometime failure to drive a brilliant point all the way home, instead merely tempting the reader to imagine its full ramifications, a kind of hint-and-miss approach which can be quite irritating. Nonetheless, whether measured against other ambitious efforts or against Leslie's own goals, Leslie has created something new and most valuable, has provided us a vocabulary which responsibly and sensitively expands the boundaries of our consciousness. If Leslie is often more cryptic than one might wish, that is because he dares more than most, and is perfectly prepared to share his own confusions and gropings with his readers.

It is not likely that Leslie's book will often be footnoted; it does not contain the kind of "information" which lends itself to citation. But it is still less likely that it will fail to have an important effect on the conceptual framework of those who have read it, and that, of course, is the most important citation of all.

LEONARD J. FEIN

Brandeis University

The Shadow: Latin America Faces the Seventies. By Sven Lindqvist. Translated by Keith Bradfield. (Baltimore: Pelican Books, 1972. Pp. 291. $2.45, paper.)

Recent scholarship depicts at minimum three alternative futures for Latin America: (1) imminent revolution, (2) evolutionary reform, and (3) no change. Fidel Castro and Che Guevara inspired the first, the Alliance for Progress the second, and several prominent scholars the third, among others Claudio Veliz and Seymour Martin Lipset. Of the three scenarios, Lindqvist argues the third, which hardly makes his book "a classic" as described by Richard Gott in his foreword to the volume. Nevertheless, the book is probing, albeit extremely pessimistic. It is based upon fourteen months in Latin America, recorded conversations, personal impressions, and Lindqvist's prior extensive experience of travel in and writing about third world problems. The conclusions bode ill for the approximately 80 per cent of the Latin American population that is barely subsisting.

Lindqvist's prognosis rests upon basic assumptions. First, Latin America's industrial growth is controlled by economic dependence on foreign firms, principally those of the United States. The key sectors of the economy are still in the hands of foreigners, as they were in the nineteenth century, resulting not in goods produced for a future mass market based upon mass needs, but in production of those industrial commodities compatible with the purchasing power of the ruling class with whom foreigners do business. This situation reinforces those conditions favorable to both foreign investor and the aristocratic class structure—low wages, liberal regulations, cheap state credits, tax relief for the companies, and continued low income, poor living conditions, and little purchasing power for the lower classes.

Lindqvist documents cases of undeclared and untaxed profits (coffee in Brazil), undervaluation of exports to pay lower taxes (bananas in Guatemala), and extremely high profits per year for the foreign concerns (oil in Venezuela). He argues that Latin American countries face rising debts through heavy borrowing and the cost of debt-servicing, at times when the capacity to tax the upper classes lags, alongside of mass unemployment, corruption, and little real economic diversification.

Second, Lindqvist is skeptical of reform groups in which other writers place faith. This includes the middle sectors, for "they are as parasitic as the old upper class" (p. 62), the church because of "the submissiveness of the great majority of priests" (p. 137), and the military owing to their entrenched politically conservative positions. Nor does revolution seem possible, given political fragmentation, guerrilla weakness, and military power. These projections are persuasive, lucid, and informative to new students of Latin American politics for whom the book is intended.

Yet one can question Lindqvist's extreme pessimism. While dramatic change in the 1970s may not occur, some cause for hope exists. Many Latin American governments now perceive massive low living standards as "problems" to be solved, rather than as "situations." This problem-solving orientation is matched by the gradual development of planning institutions to cope with poor health, housing, education, and undiversified industrialization, e.g., Cordiplan in Venezuela, and one finds a new interest in rational public administration among some leaders. Moreover, President Juan Velasco Alvarado and Latin America's Andean Pact should not be written off completely as attempts to break the dependence syndrome.

Lindqvist's stress on foreign economic dominance as the single cause for Latin American underdevelopment obscures other underlying forces. These include the authoritarian Iberian culture, patronage, organizational deficiencies, elite factionalism, and concern with the world of ideas rather than pragmatic action. This is not to conclude that development is impossible, given traditional values and the situation of foreign economic control, but that its conditions of change occur in unique settings not amenable to single-factor analysis.

W. RAYMOND DUNCAN

SUNY, Brockport

The Nigerian Military: A Sociological Analysis of Authority and Revolt, 1960–67. By Robin Luckham. (Cambridge: Cambridge University Press, 1971. Pp. xiv, 376. $18.50.)

Dr. Luckham's impressively comprehensive and detailed inquiry into the Nigerian military is also a "book about human tragedy of terrible proportions" (p. xi). The military has been the commanding institutional presence in Nigerian politics since January 15, 1966. On that date, a small group of army officers staged a military coup against the constitutional regime. The officers did not succeed in taking control, but the civilian government collapsed, handing power over to the "legitimate" armed forces. The army General Officer Commanding, Major General J. T. U. Aguiyi-Ironsi, became head of a Federal Military Government, with a military governor in each of the four regions. On July 29, 1966, a countercoup within the army displaced Aguiyi-Ironsi, and Lieutenant Colonel (later Major General) Yakubu Gowon emerged as Supreme Commander and Chief of State. On May 30, 1967, the Military Governor of the Eastern Region declared its secession as the Republic of Biafra. Federal troops invaded the East on July 6, 1967. Such is the bare outline of events marking the Nigerian tragedy.

The author's systematic and objective analysis not only instructively describes and explains the events in which the military was involved in the critical years of 1966 and 1967 but also critically tests current theories respecting civil-military relations in new nations and the transfer of institutions from one sociopolitical context to another. The study begins with a detailed account of the coups of January and July, 1966. The second part is a synchronic analysis of the organizational weaknesses and strains in the Nigerian Army. Part Three, returning to a sequential treatment, examines the course of interaction between the military and its political and social environment. The treatment throughout is informed by impressive research, although documentary materials and observational data—the latter obtained in 1967 in circumstances of considerable difficulty—are necessarily supplemented by "a certain amount of interpretative understanding of events" (p. 13). Dr. Luckham's interpretations are persuasive.

The painstakingly and judiciously assembled chronicle of events, though not played for dramatic effect, would be as fascinating, were it not so distressing, as it is rewarding. The major value of both the sequential and the synchronic treatments, however, lies in the methodical search for explanations of events.

In the climactic years of constitutional rule, 1964–1966, when the integrity of the civilian regime was steadily declining and national cohesion weakening, the Nigerian Army stood out as an acknowledged symbol of national unity, thought to be largely free of the "tribalism" which infected so much of Nigerian society and all the more respected for its political neutrality. After the coup in January, 1966, the army, which was now also the government, began to fragment and weaken internally, and the countercoup of July, 1966, marked the sundering of the military organization apparently by hostility between ethnic and regional groups in it.

The author's analysis "boils itself down in the main to the interplay of environment and the organizational cohesion of the military" (p. 7). Largely because of the pace of institutional transfer, notably in the form of rapid Nigerianization, the Nigerian Army did not develop the internal strengths of its British model. It was thus plagued by and vulnerable to a variety of structural strains and cleavages. Internal cleavages often coincided with and were aggravated by primordial—ethnic or regional—loyalties. Furthermore, ethnic and regional identities served as the main link with the political and social environment, and the heightened exposure to that environment which followed the events of January, 1966, accentuated in the military the very primordial conflicts which so severely strained Nigerian society as a whole. Thus this element was more explicit and important in the July coup than it had been in January. " 'Tribalism' was the symbolic master key which unlocked pent-up organizational tensions, communal rivalries and political conflict all at the same time" (p. 197). Although the army "intervened to put an end to tribal and regional disputes in the name of national unity, it ended up itself divided on those very issues it purported to resolve" (p. 204).

Luckham does not argue that tribalism made inevitable the fragmentation of either the army or the nation. Indeed, he stresses the multiplicity of causal factors throughout. Without the influence of primordial suspicions and hostilities, however, it is clear that the history of both army and nation would have been different. "The fragmentation of the army . . . not only reproduced the nation's cleavages, but brought them to their logical conclusion, by making secession a realistic possibility for a region which had the force of arms at its disposal" (p. 205).

GRADY H. NUNN
University of Alabama, Birmingham

Money and Capital in Economic Development. By Ronald I. McKinnon. (Washington, D.C.: The Brookings Institution, 1973. Pp. 184. $7.50.)

This original and valuable study of economic development takes its start from the remarkable economic progress of South Korea since 1964. In

1964–65, there was a sharp devaluation and unification of exchange rates; an increase in tax collections and user charges on outputs from government-owned industries; and a banking reform which raised the official ceiling on interest rates from about 15 to 30 per cent. The decline in the rate of inflation which followed (from 19.5 per cent annually in 1960–64 to 8 per cent during 1965–69) raised the real rate of return on holding one-year time deposits from an average of −3.8 per cent in 1961–64 to +18.9 per cent in 1966–69, a figure which declined somewhat as expectations of relative price stability took hold. In the period of reform (1965–69) exports increased about tenfold, real output by 90 per cent. In this context of policy and progress, there was a sevenfold increase in the money stock, corrected for price increases, and the proportion of the money stock to GNP rose from 9 per cent to 33 per cent.

This astonishing performance by a nation which appeared a poor development risk, indeed, in the early 1960s led Professor McKinnon to examine other examples of progress and frustration among the less developed countries and to reflect on monetary theory as it relates to development policy.

He begins with a stylized portrait of the dilemma from which South Korea escaped in the second half of the 1960s. He describes how the inherent fragmentation of a less developed country leaves most economic units dependent for investment on their own savings, incapable of mobilizing the resources necessary for a radical, discontinuous shift to technologies of higher productivity. He then considers the array of conventional governmental measures undertaken to substitute for a large, interacting, and efficient capital market, concluding that such measures distort the allocation of investment resources and income distribution, by failing to provide an equilibrium rate of interest: it is too low for the favored few, too high for the many dependent on rural moneylenders, etc. The endemic inflation to which such interventions contribute yields further distortions.

Professor McKinnon draws back at this point to isolate the particular assumptions of neoclassical monetary theory which do not fit the typical state of affairs in a less developed country and to demonstrate (mainly with Latin American examples) why the standard International Monetary Fund remedies for inflation have led to recession and frustrating stop-and-go policies rather than to sustained growth along optimum sectoral paths.

Turning to remedy, Professor McKinnon argues the generality of the Korean case, buttressed by references to postwar Germany and Japan, as well as Taiwan since the 1950s and post-1965 Indonesia.

The heart of his argument is that a disciplined fiscal policy can end highly inflationary expectations; in that setting, high nominal rates of interest will yield an increased willingness to hold assets in the form of bank deposits; the real size and competitiveness of the banking system will increase; and the distorting environment of both negative interest rates and credit shortage will give way to a more or less true (and rather high) equilibrium rate of interest. On this base, the rise in the money stock will exceed the rise in GNP; and the economy can move forward rationally, its fragmentation overcome, with steady growth itself yielding a rise in the propensity to save beyond that achieved by the initial Korean-type reforms. (The latter proposition is formally elaborated in Chapter 9.)

Professor McKinnon then extends his general theory to the foreign balance, advocating tax policies consistent with trade liberalization and a highly selective acceptance of foreign capital. He underlines the limits and frustrations of policies of import substitution; and with respect to foreign investors, his dictum is (p. 176): "Poor countries have been cavalier in their use of foreign capital, although a puritanical approach would have served them better."

There is a good deal to be said in favor of this somewhat libertarian analysis and boot-strap prescription. Professor McKinnon is correct in isolating the unrealistic assumptions of neoclassical monetary theory and the policies that flow from it; in describing the distorting effects of inflation, including both negative interest rates and capital shortage; in underlining the capacity of even poor nations to expand domestic savings under a regime of high real rates of interest and relative price stability; in asserting (p. 149) that "the main restraints on foreign trade originate within LDCs [less developed countries] themselves" and that, with proper exchange rates (and other policies), the world trading framework permits large increases in diversified exports; in concluding that a great many public interventions in the economy are counter-productive, and that some forms of public and private foreign investment in LDCs can be counter-productive.

On the other hand, his portrait of the Korean case is incomplete and, by implication, the conclusions he draws are too sweeping. Korea was nursed forward with large external assistance for a decade after the end of the war to a point where the reforms of 1964–65 became practical. (In retrospect, one of the greatest contributions of foreign aid has been to buy time for some societies to find their feet.) The Korean economy was further cushioned by high if declining levels of foreign aid during the critical early years of its take-off; the Koreans exhibited (like the Japanese in the post-

Meiji Restoration period) a passion for education, remarkable entrepreneurial gifts, and a sturdy sense of national purpose; their precocious absorption of complex new technologies has depended substantially on private capital imports; and their economic progress has been framed by sophisticated aggregate and sectoral national plans. Much the same could be said of the miracle on Taiwan which was launched about a decade earlier.

In short, a rapid increase in capital formation, in diversified manufactured exports, and in the absorption of sophisticated technologies depends on many more factors than Professor McKinnon examined. The quality of private (and public) entrepreneurship, in particular, is a critical variable that cannot always be assumed optimal in LDCs or, indeed, in more advanced economies. And, in effect, this is what Professor McKinnon does assume in his general prescriptions.

Nevertheless, within limits, his analysis is valid; and his crusading zeal is not inappropriate, given the state of affairs in important parts of Latin America, India, and a good many other LDCs.

W. W. Rostow
University of Texas, Austin

Pattern for Profit in Southern Africa. By Ian Mackler. (Lexington, Mass.: Lexington Books, 1972. Pp. 120. $8.50.)

This thin volume dealing with the involvement of the United States in southern Africa is offered as a study in foreign policy. It contains for the author's frame of reference sufficient data from resources that are commonly utilized in this subject area, but it would be an exaggeration to suggest that the value of this work derives from empiricism. With a limited set of facts, Mr. Mackler displays a high degree of craftsmanship. However, he has clearly not attempted to produce a methodological *tour de force*. The study conforms to neither the monographic models of policy science nor the conventional renditions of diplomatic history. It revives memories of J. A. Hobson, Leonard Woolf, and Leonard Barnes. In part it reads like a pamphlet; in part it is a well-argued dissertation, depending heavily on deductive and syllogistic analysis. What the author has in mind he states directly and lucidly, and he is emphatic in the pursuit of his contentions.

Mr. Mackler roughs out a much abbreviated account of the conduct of American delegates at the United Nations on the question of the status of Namibia (Southwest Africa as administered by the Union and later Republic of South Africa). The account is neither thorough nor up to date, and thus it seems that the author has simply employed this topic to get to a more central matter, namely, the presence and posture of the United States in southern Africa as manifested by the operations of the multinational corporations and American government. He returns to the Namibia matter repeatedly, but he really has cast an extended net to catch a giant specimen, unmistakably a specimen of imperialism.

With clarity Mr. Mackler lays it right on the line when he articulates the thesis that in southern Africa the U.S. dollar and flag have inseparably combined to serve an intimate partnership of economy and state. Unlike certain writers he wastes no time asking whether the dollar follows the flag, or the flag the dollar. According to the author, the corporations seek great rewards in the form of profits, and Washington seeks strategic raw materials for the defense establishment. Together, reinforcing one another, they find in South Africa a favorable climate for investment, trade, and procurement.

Profitability is the magnet for American capital, necessity moves the United States government. Mr. Mackler portrays in this instance what Professor Andreas Papandreou has described as "paternalistic capitalism" (*Paternalistic Capitalism* [Minneapolis, University of Minnesota Press, 1972]). American leaders in both private and public sectors appear to be singlemindedly responsive to the opportunities afforded them to attain their first-priority goals. South Africa with *apartheid* meets their expectations. Mr. Mackler ascribes to American leadership no exceptional inclination to involve itself in southern Africa mainly to attain political hegemony, support white supremacy, or demonstrate cultural superiority. The implication is that American policy is distinctly practical.

As presented by Mr. Mackler, this is a point of considerable importance for policy formulation. It may very well explain why efforts to reverse the United States commitments in southern Africa and the leanings of the American delegation at the United Nations generally failed. Other considerations appear to be no match for the profits-and-strategy pattern that sustains American policy. Thus, little has come out of the appeals to the corporations and to the United States government to cease and desist operations in South Africa and Namibia on grounds that *apartheid* is immoral, that Africans are deeply offended, and that the United Nations obligates its members to sever economic ties with South Africa. Apparently none of these arguments is of sufficient relevance in the prevailing system to penetrate deeply into the American decision-making process. Only if South African profitability for foreign capital declines drastically or if the basis of the global foreign policy of the United States undergoes major transformation is there any chance for a

shift in support of the United Nations posture regarding South Africa and Namibia.

Several times Mr. Mackler alludes to the "significance" of the South African economy for the American multinational corporations. His magnitudes are generally acceptable, although he understates the extent, as well as the diversity and complexity of their involvement. The trouble for the reader is that, while his position is incontestable, his criteria for "significance" impose narrow constraints on his assessment. There are several dimensions that he might have explored, but he has obviously set limits for himself, has made out a case as he sees it, and stops abruptly at the juncture. He ends the study without, for example, trying to apply his findings to the perplexing question of how to determine objectively whether American capitalism is dependent for growth on the operations of multinational corporations and overseas markets for capital, goods, and services, particularly in the Third World.

JULIAN R. FRIEDMAN
Syracuse University

East Indians in Trinidad: A Study in Minority Politics. By Yogendra K. Malik. (London, New York: Oxford University Press for The Institute of Race Relations, London, 1971. Pp. xv, 199. $9.75.)

The author, a citizen of India who teaches at the University of Akron, Ohio, describes here the history of East Indian political organization in the island of Trinidad. Of the main ethnic population groups of Trinidad, the "Creoles" of Euro-African descent, who comprise some 60 per cent of the population, provide the power basis for Prime Minister Eric Williams's *People's National Movement* which has governed the country of Trinidad-Tobago since 1956. The descendants of East Indian immigrant workers (36 per cent of the total population), heavily concentrated in the rural areas of Southern Trinidad, are still mostly occupied as cane workers and small farmers. Yet since the last few decades, increasing numbers of this ethnic group have become successful in commerce and the professions. Both in the civil services and in the professions they are, however, still underrepresented.

Professor Malik notes that the East Indians suffer from internal divisiveness caused by differences in caste, color, and religion. Mainly on the basis of interviews with 89 members of the East Indian élite (selected for their "institutional leadership and reputation" [p. xii]) he concludes that in their attitudes toward the Afro-European group, the Christian East Indians are the most tolerant and the Hindus the least tolerant, with the Muslims in between.

Yet even allowing for these internal differences, the group as a whole is described as bent on maintaining its ethnic and cultural identity, and as conservative in its political and economic views. Its political organization reached a peak of cohesiveness during the first years of Dr. Williams's government, when the East Indians' fear of impending independence (which would put them at the mercy of the majority ethnic bloc) was greatest. Political independence became reality, despite East Indian stalling tactics; and when it did not prove to be so disadvantageous to the East Indians as they had feared—partly thanks to Williams's statesmanship—their party's leadership began to lose its vigor.

The book provides interesting details on Asian minority-group thinking in the Caribbean. Trinidad's East Indians clearly were concerned about the effects their political actions might have on other East Indian populations in the area. Their ambivalence toward Guyana's Cheddi Jagan (who as a fellow East Indian could claim their loyalty, yet as a political "radical" provoked some distrust) nicely illustrates the complexities that attend the admixture of ethnicity and ideology.

One may question the significance of opinions expressed by very small subdivisions of the interviewed élite, as well as the representativeness of the 89 out of the sample of 130 persons who consented to be interviewed; nevertheless, my overall impression is that Professor Malik, by adding participant observation and analysis of written sources to his interviewing method, has succeeded in reporting on the group's political ideas and organization with commendable perceptiveness and a strong effort at impartiality.

H. HOETINK
University of Puerto Rico

Bolivia: The Uncompleted Revolution. By James M. Malloy. (Pittsburgh, Pa.: University of Pittsburgh Press, 1970. Pp. 396. $11.95.)

This is one of a growing list of books on the Bolivian Revolution of 1952, one of the three most basic revolutions to take place in Latin America in the 20th century. It is certainly one of the best. Professor Malloy knows Bolivia well, has visited the country a number of times for extended periods, and has studied the country's problems for almost a decade. This volume is the first installment in a series of Malloy's studies of the Bolivian National Revolution.

Professor Malloy, a young political scientist from the University of Pittsburgh, is particularly interested in tracing the political evolution of the revolutionary regime, and particularly of the party which led it, the Movimiento Nacionalista Revolucionario (MNR). He seeks to place this evolution against the background of Bolivia's economy, society, and polity, devoting the first

one-hundred fifty pages of the book to changes during the half century before the Revolution.

He discusses the era of domination of the Liberal Party from 1900 to 1920, during which the tin industry reached its apogee; in the same period the social discontent which culminated in the 1952 Revolution began to develop with the appearance of a modern working class and the core of a middle class. Malloy then traces the stagnation of tin mining after the middle of the 1920s, the impact of the Great Depression and the Chaco War, which gravely undermined the existing social order. Professor Malloy also traces the evolution of the Movimiento Nacionalista Revolucionario and other parties which arose in the wake of the Chaco War. He is particularly concerned with the relationship between the MNR and the country's Trotskyist party, the Partido Obrero Revolucionario (POR).

The middle section of the book deals with various aspects of the MNR regime and the transformations which it brought about. There are separate chapters on the first reforms undertaken by the MNR, the agrarian revolution, and "the problem of development." Two long chapters deal with the internal politics of the MNR during its period in power, 1952–64, and the party's relations with other parties and different segments of the community.

Although readers of this book will learn a great deal about the MNR and the Bolivian National Revolution, I feel that they will get a mistaken view of the forces at work within the MNR. The thesis of Professor Malloy is that there were three well-defined factions in the MNR—the right wing, the center core consisting of most of the old-timers of the party and led by Victor Paz Estenssoro, and the Left. The first two factions were predominantly middle-class, the Left was mainly working-class. This analysis is unobjectionable, but I do not agree with Malloy's picture of the nature of the MNR Left: Professor Malloy pictures the labor element which made up the bulk of the MNR Left as essentially outsiders, who joined the MNR during the six years between its participation in the government of Major Gualberto Villarroel (1943–56), and the 1952 Revolution. He also argues that these labor people were basically Marxist-Leninists and were sympathetic to the POR. He seems to imply that they looked upon the MNR regime as a transition to something more "fundamental."

This analysis is mistaken. Juan Lechin, the most important leader of the MNR Left was a member of the Movimiento Nacionalista Revolucionario virtually from its inception. His first political loyalty, and that of virtually all of the early MNR labor leaders, was first and foremost to the MNR until the last year of the party's tenure in power. Just before the 1952 Revolution and in the two years thereafter, important labor leaders who had theretofore belonged to the pro-Stalinist Partido de Izquierda Revolucionaria and to the POR, joined the MNR. Most of them, however, had thoroughly broken with their old parties and in the case of the ex-PIRistas, at least, with whatever Marxist-Leninist ideas they had once had.

I was able to observe at first hand in 1952, 1953 and 1954, the way in which the MNR labor leaders dealt with the POR and the Communist party, successor to the PIR. Juan Lechin and other MNR leaders of the Central Obrera Boliviana (COB) acted in that body and in the unions which belonged to it, as disciplined MNR members, and I have seen them pulverize both Trotskyists and Stalinists in debates in the COB. When the POR sought to use the very substantial influence which they were able to obtain in the COB to have the COB go on record against government policies, Lechin and his friends moved massively to oust the Trotskyists, who never again during the 1952–64 period had any real power.

Some of Professor Malloy's confusion may arise from the fact that between April and October 1952 the Trotskyists largely dominated the COB, for two reasons: First, the principal MNR labor leaders were very busy in activities in the government—Lechin was Minister of Mines and Petroleum, Nuflo Chavez was Minister of Peasant Affairs, and German Buitron was Minister of Labor. Second, the COB had a system whereby provincial union groups named "permanent delegates" to attend COB meetings, which were held at least once a week, and the POR were successful in getting a number of provincial groups to name Trotskyists in this capacity. After the Trotskyists put the COB on record against the government's law to expropriate the tin industry in October 1952, the MNR leaders saw to it that POR permanent delegates were replaced by MNR ones, and Lechin, Chavez and Buitron began to attend every meeting of the COB.

Professor Malloy does not, I think, sufficiently recognize the basic loyalty of the great majority of the MNR labor leaders to their party and to its "populist" philosophy. He very considerably overestimates the real power which the POR had in organized labor after April 1952, and attributes to the MNR Left a Marxist-Leninist philosophy which it did not have.

These criticisms, however, should not dissuade anyone from reading Professor Malloy's book. His point of view is certainly a respectable one, and he provides much valuable information con-

cerning the origins and progress of the Bolivian National Revolution.

ROBERT J. ALEXANDER
Rutgers University

Aspects of Religion in the Soviet Union, 1917–1967. Edited by Richard H. Marshall, Jr.; associate editors, Thomas E. Bird and Andrew Q. Blane. (Chicago: University of Chicago Press, 1971. Pp. 489. $19.75.)

This book is dedicated to Paul B. Anderson, the remarkable YMCA official who became an eloquent interpreter of Eastern Orthodox religious developments for American Protestants. Dr. Anderson's own reminiscences about Russia, going back to June 1917, constitute a fascinating chapter of the book. The remaining seventeen substantive chapters (in addition to a useful bibliography and a documentary appendix of Soviet laws on religion) provide a much more unified treatment than is common in *Festschriften.* To be sure, the book could have been shortened considerably if the contributors had been convinced that the general aspects of their topics were quite adequately covered in the authoritative chapters on "Religion and Atheism" by Bohdan R. Bociurkiw and "The Legal Status of Religion" by Joshua Rothenberg. As it is, each author seems to have felt obliged to present the general picture before getting down to his specific contribution on, for example, the Georgian Orthodox Church or the Mennonites. Still, even redundancy has its advantages. Most readers will probably use the book as a source for specific information on little-known aspects of religion in the USSR, rather than as continuous reading; they will therefore appreciate the fact that each chapter can be understood separately.

Four chapters are especially useful because so little material is available on the religious groups they treat. During the last few years several important books on the early development of the Armenian Gregorian Church have appeared, but not much material in English covers its position in the USSR. Mesreb K. Krikorian's chapter partially fills this gap by sketching the trying history of this church under Stalin, but provides only a few rather bland paragraphs on the post-Stalin situation. While the author mentions the highly significant role of the Gregorian Church among Armenian communities in the Middle East, it is regrettable that he did not probe the connection there between religion and politics. J. J. Gapanovich's survey of the Soviet regime's treatment of the animistic religions of the Siberian tribes has less direct political significance but is suggestive for comparative analysis. Religious missionaries (Moslem and Buddhist as well as Christian) in other parts of the world are often accused of destroying vital cultural complexes in their zeal to propagate higher religions. Gapanovich shows how, on a small scale, Leninist impact on the Siberian shamanistic cultures has probably been more severe, for Soviet persecution of religious leaders accompanied the extremely disruptive collectivization campaign. Evidently Soviet Marxism offers no solution to the painful dilemma arising from contact between tribal societies centering on animistic myths and the rationalizing drives of industrial civilization. Andrew Q. Blane's "Protestant Sectarians in the First Year of Soviet Rule" is a strictly historical discussion, but contains many implications for the contemporary plight in the USSR of fervent nonconforming groups like the Seventh Day Adventists and Jehovah's Witnesses. Frank H. Epp's treatment of the Mennonites, chronologically more comprehensive, will be interesting to students of the occasionally troubled relations of this denomination with North American governments.

Zvi Gitelman's treatment of relations between Jews and the early Bolsheviks is a summary of his comprehensive, revealing book, *Jewish Nationality and Soviet Politics* (Princeton, Princeton University Press, 1972), which I recently reviewed at length in the *American Jewish Historical Quarterly.* Joshua Rothenberg's analysis of the status of Soviet Jews covers much the same ground as Lionel Kochan, ed., *The Jews in Soviet Russia since 1917* (see my review in *APSR*, 66 [March 1972], 246–48). As summaries these chapters are valuable, as are Alexander Veinbergs's treatment of Lutheranism, Elie Melia's chapter on the Orthodox Church in Georgia, and Bernhard Wilhelm's on Moslems. Because it duplicates much of Rothenberg's chapter on legal restrictions, the article on "The Legal Position of the Russian Orthodox Church" by Alexander A. Bogolepov is likely to be less useful.

In a chronologically more limited framework, the chapters on "The Origins of Soviet Antireligious Organizations" (Joan Delaney) and "Khrushchev's Religious Policy" (Donald A. Lowrie and William C. Fletcher) provide some new interpretations of the role of the Orthodox Church within the general framework. Much of this ground was covered more completely in Fletcher's book, *Religion and Soviet Foreign Policy* (London, 1973), but even the professional observer will find new information in George L. Kline's remarkable survey of Christian perspectives in the writing of nearly all the most famous Soviet dissident belletrists.

Recent books on religion in the USSR, like those mentioned above, have devoted relatively little attention to Roman Catholics. Conse-

quently, I should like to discuss the two articles examining the complicated situation of my own co-religionists at slightly greater length. V. Stanley Vardys's treatment of Lithuanian Catholics and the more general coverage by Paul Mailleux, S.J. make a case that during very recent years the situation of Roman-rite Roman Catholics (this cumbersome neologism is a result of the fact that "Latin-rite" became an anachronism after the Second Vatican Council) has improved in areas (Lithuania and nearby portions of Latvia) *where Catholics constitute a majority.* While stressing the lasting effects of earlier intense persecution, Professor Vardys concludes that "the situation among the faithful, interestingly enough, is discouraging but relatively not as bad," and "in the villages church attendance is still very high, reception of communion is frequent, and religious loyalties are still strong" (pp. 397–98). Father Mailleux appears to be more optimistic, noting recent permission for comparatively free communication with the Vatican.

Suppression of *Byzantine-rite* Roman Catholics, however, remains more harshly complete than that of any other large religious body. Analysis of the 1970 census strongly suggests that the latter (overwhelmingly West Ukrainian) are nearly twice as numerous as Roman-rite Catholics. Yet official church publications and (as far as one can judge) Vatican diplomacy have devoted far more attention to incremental improvement of the status of Roman-rite Catholics, and especially to ecumenical contacts with the Russian Orthodox Church. The paradox of this approach is that it was precisely the latter body which (in 1946) acted as an instrument of the Soviet regime in completely destroying the Ukranian church organization, persuading a small minority of the priests to avow Orthodoxy, and executing or imprisoning the recalcitrants (including the entire hierarchy). Today, Orthodox churches are open throughout the Western Ukraine (possibly with fewer restrictions than elsewhere in the USSR) while Byzantine-rite Catholic services are relentlessly suppressed.

The Jesuit order, acting through the excellent training facilities of the Pontifical Collegium Russicum in Rome, has been the major force behind the effort at reconciliation with the Moscow Patriarchy. Indeed, Jesuit interest in "summit" contacts with Russian Orthodoxy goes back at least to Catherine II's sheltering of the Order while it was officially dissolved. Consequently it is to his credit that Mailleux (current Rector of the Russicum) candidly recognizes that "for those groups which have remained secretly united with Rome, these events [Vatican-Patriarchate negotiations] have caused no small amount of surprise and confusion" particularly when Orthodox clergy "have pointed to the change in the Roman attitude to demonstrate to Ukrainians and Carpatho-Russians who remain faithful to the Holy See that they are wrong in persisting in their loyalty" (p. 376). What Mailleux does not explore is the considerable evidence that these and earlier Orthodox attacks on Ukrainian Catholicism may actually have been instigated by the secret police. After all, if a devout Orthodox Christian like Solzhenitsyn must cry out that "a church [like the contemporary Russian] dictatorially ruled by atheists is a sight not seen in two thousand years" (*The New York Times*, April 9, 1972), negotiators of other faiths might reflect that the time is appropriate to combine the cunning of serpents with the innocence of doves. What has been most regrettable in the Roman Catholic approach is the scarcity even of extended, systematic analysis of the problem.

JOHN A. ARMSTRONG
University of Wisconsin

Burma and Pakistan: A Comparative Study of Development. By Mya Maung (N.Y.: Praeger Publishers, 1971. Pp. 178. $13.50.)

Professor Mya Maung opposes economic theories of development that would impose on the study of less developed countries (LDC's) models derived from advanced industrial countries. Such models "cannot cope with the sociocultural matrix of development." For him the noneconomic determinants of economic development merit equal if not superior status when studying development in LDC's. Thus, he decided to compare Pakistan and Burma in order to understand why certain types of economic philosophy and policy are pursued and to examine their success or failure. He elected to do a macroanalysis, omitting as "irrelevant" the "intraregional and intragroup" cultural variations.

The significance of noneconomic factors in development has been long recognized by students of the developing countries. It is therefore questionable whether the author should have devoted his first chapter to a general, however well-written, restatement of it. He would have been better advised to have devoted his effort in this all-too-brief monograph to a closer analysis not only of the macrodata but also of some of the intraregional and intragroup cultural variations which despite his allegation of irrelevancy profoundly affect the outcome of development in some LDC's, especially those he has chosen to compare.

The treatment of Pakistan demonstrates the faults inherent in Mya Maung's macroanalytical study, published just before the recent tragedy in East Bengal. He concludes that because Pakistan had adopted "open" developmental policies, it was therefore "relatively successful in nation-

building and development" (p. 37). Earlier students of Pakistan, not indifferent to intraregional and intragroup data, had noticed their dangerous implications *before* the eruption of Bangladesh, an event among others that refutes the author's optimistic conclusion.

When Mya Maung turns to Burma, he is on surer ground. His historical treatment draws on established sources, notably the work of British historian J. S. Furnivall who described Burma as a "plural society" in which the majority ethnic group, the Buddhist Burmans, were or felt they were deprived "outsiders" in their own country. Independence in 1948 was brought about largely by a nationalist, democratic socialist leadership, self-styled, the *Thakins* (i.e., those who had determined to be "masters" in their own "house"). The *Thakins* had been encouraged by politically minded members of the Buddhist *Sangha* and had easily acquired Burmese Buddhist mass support.

In a broad sense, Buddhism and some variety of loosely defined nationalism *cum* socialism have been the two dominant themes in Burmese life, prior to and certainly since independence. Development practices have necessarily incorporated in some way various measures of each from 1948 to present. To all this Mya Maung, again quoting Furnivall (p. 86), gives assent. His task is to discover what specific ways development has in fact responded to its "sociocultural matrix."

Chapter IV is crucial to the author's analysis, for in it he is concerned with demonstrating that the degree of societal openness accounts for the pace of development. He defines his yardstick as follows: "where social conflict and economic change are concerned . . . a social system can be viewed as closed with respect to the elasticity of social change, the intensity of social conflict and its policies of sociocultural insulation generated by the forces of a traditional social system" (p. 90). He points out that the traditional Burmese social system and Buddhism tend to support individualism (i.e., an egalitarian, socially mobile society with a resource base that easily avoided the extremes of poverty and exploitation); and that such individualism was or should have been compatible with an open economic system leading to development. Paradoxically, he holds that governments since independence chose to develop some form of evolutionary or "command" socialism because of the "social conflict and change that took place in the traditional social system during and after colonial rule" (p. 93). Hence Burma has failed to develop, especially under its more extreme form of "command" socialism. The "social conflicts and change" that resulted in making Burma more of a "closed system," and hence arrested her development, according to Mya Maung, were: (1) the cultural resistance of Buddhism to any new inflow of alien values and technology and the disvalue placed by Buddhism on desire for wealth, property, and worldly pleasure; (2) the indigenous training and education of the *Thakins*, the leaders of the nationalist movement; and (3) the impact of the Cold War on Burma, her unhappy geopolitical situation between China and India, and her "historic antipathy towards the West," all of which have caused her to adopt a neutralist foreign policy.

As a result of these factors, Burmese leadership since independence has apparently been willing to continue, if not to enhance, the sociocultural parameters of a relatively "closed" Buddhist society, adding to the latter in less or greater degree a "command" or closed economy in pursuit of development. The result is "economic stagnation" (pp. 119–125). The "closedness" of the society, "the illusive" but real "affinity" between Burmese Buddhism and Marxism, strengthened by the traditional nationalism and traumatic experiences under colonial rule, have been the main reasons for adopting socialism in Burma. Socialism, especially the total state command variety as practiced in Burma since the 1962 military coup, has resulted in developmental chaos because it has not been able to tune "the basic anti-materialistic tenets of Buddhism to the historical materialism of Marx" (pp. 136–138).

Mya Maung's *conclusion* about Burma's economic stagnation has long been generally recognized, even by General Ne Win, Head of State and government, who has described the consequences of the so-called "Burmese Way to Socialism" as a "mess." The author's causal explanation for it, however, as summarized in the above three essential sets of factors must be seriously faulted as being insufficient and partly inaccurate.

Although Buddhism's emphasis on nonaccumulation is, in theory, incompatible with one of the major requisites of economic development, it may be noted that Japan, influenced in major ways by Mahayana Buddhism, was able to transform its traditional economy into a completely modern one. Moreover, the Theravada Buddhist countries of Ceylon, Thailand, Laos, and Cambodia have varied tremendously in their respective economic systems and in their degree of success or failure. Thus, from the standpoint of comparative analysis the religious variable may well be questioned as a determining factor in economic development.

It is true that the *Thakins* were almost all "trained at home," but they went to secondary schools and the University or entered the professions that were almost exclusively Western and British-oriented. Even their adoption of modified forms of Marxism in the middle 1930s and later was obviously of Western origin. There is no evidence that because they were educated in Burma

they became initiators or supporters of a "realtively closed society." Though the present regime has adopted an absurd statist scheme called the "Burmese Way to Socialism," the reasons for its adoption can hardly be attributed either to the leadership's being Buddhist or to its being trained at home. The reasons for failure must be sought elsewhere.

There is no doubt that Burma, at least since 1949–50, has followed a genuinely neutralist policy in foreign affairs. But her reasons for this are only partially presented by Mya Maung. Before the Cold War reached mainland Southeast Asia, Burmese leadership sought the help of "big allies" when her fledgling independent government in early 1948 came under attack from Communist insurrectionists. She did *not* get the help she needed either from her then ally-by-defense-treaty, the United Kingdom, or from the United States. Under continuous attack from Communist insurrectionists (oriented toward Moscow or Peking or both) she has pursued a cautious international policy so as to avoid as much as possible the active enmity of powers whose domestic proxies *to this day* mount insurrectionary attacks against every Burmese government, including the present "command" Socialist one. Her neutralism is less a subspecies of the familiar Asian "closed" or "hermit" kingdom than a posture of self-defense in a world not always willing to support troubled LDC's.

Finally, the failure of development in Burma can in no small measure be attributed to the persisting problem of ethnic pluralism which has impeded national integration. A major weakness of the present study is that it fails to take such intragroup and intraregional issues into account in its analysis of the developmental process.

Burma and Pakistan: A Comparative Study in Development is worth reading even though it fails to fulfill its promise to explain why development does or does not take place in less developed countries.

FRANK N. TRAGER
New York University

Southeast Asia's Economy: Development Policies in the 1970's. By Hla Myint, (New York: Praeger Publishers, 1971. Pp. 189. $2.95, paper.)

The reader should be warned that this interesting paperback is in essence the extended first chapter of a previously published report (Asian Development Bank, *Southeast Asia's Economy in the 1970s*, Longman and Praeger, 1971). Having said that, one should quickly add that this chapter in its extended form was well worth making available to a wider public. Professor Myint's insight into and understanding of the problems in his home region—he is of Burmese origin—are excellent. His views as a well-known development economist are somewhat predictable, but no less serious and interesting for that. The report stands head and shoulders above the normal level of collective group reports commissioned within a tight time schedule by various international bodies.

Professor Myint's own convictions, as well as the Asian Development Bank/SEATO/Ford Foundation framework in which the original report was prepared, will make it clear that a fairly conservative or establishment point of view is presented. But it is a *neo*-establishment point of view with its emphasis on outward-looking development in place of import substitution and on development of markets and incentives rather than state control and regulation, as well as in its emphasis on getting factor prices right, which in one place is described as "more than half the battle" (p. 77). All these views are presented with a consciousness of the need to get away from élitist styles of development and to benefit the smaller farmers, and masses in urban shantytowns.

I have no real quarrel with this recent trend in development analysis (see the studies by Little et al. for the OECD, and Bela Balassa et al. for the World Bank). I do feel, however, that the approach results in some blind spots, such as an absence of any real analysis of some of the more harmful impacts of private foreign investment, or of the needs and problems of developing indigenous technological capacities, and a failure to specify the more active government policies required for a policy of redistribution with growth.

The benefit of hindsight produces some other nagging doubts: What will the oil crisis do to Myint's trump card, the projected rapid growth of Japan? Will the impact of the oil crisis on the rich western countries permit them to engage in the combined policies of trade liberalization and adjustment assistance which the author so convincingly advises? If the countries of Southeast Asia—and other countries—follow in the footsteps of Korea, Taiwan, and Hong Kong in extending their manufactured exports, what resistances will be created in the importing countries? Will not the very success of the policies suggested provoke the resistance and obstacles which will force the countries of Southeast Asia back to other styles of development?

The original report was written in 1969 and bears the imprint of SEATO politics of that time. China looms silently in the background, only mentioned for the possibility (although stated to be improbable) of "massive assault or nuclear threats" (p. 132), not for any useful lessons for the countries of the region. The suggestion is actually made (p. 135) that international agencies

should be less coy in financing projects connected with the military. Even given the author's emphasis that the military should be more closely linked with economic development—as of course it is in China, (although this is not discussed in the book) —this is a most dangerous suggestion.

Politically, the chapter dealing with the economic impact of the ending of the Vietnam hostilities and the reduction of British military presence in Malaysia and Singapore is particularly interesting. But as far as Vietnam is concerned, unfortunately the assumptions made at that time look very unrealistic now. It is assumed that "Vietnam hostilities will effectively end by 1973 through a Hanoi decision to withdraw and switch to a low-level insurgency" (p. 121). Alas!

The book is well written and can be recommended to all students of the Southeast Asian region. It is a pity, however, that in the editing there remain numerous references to the report in its original form, rather than to the published form which is much more readily available.

H. W. SINGER
Institute of Development Studies, University of Sussex, England

Studies in Ibo Political Systems: Chieftaincy and Politics in Four Niger States. By Ikenna Nzimiro. (Berkeley: University of California Press, 1972. Pp. xviii, 287. $12.75.)

The Ibo of Nigeria have long commanded the attention of international scholars, expatriate missionaries, officialdom—and wary neighbors. An intelligent and energetic people, they were alert early on to the opportunities provided by colonial rule and mission education to establish a leading presence in the country. Following the Second World War, Ibo youth swelled the ranks of nationalist dissent, and their leaders helped spearhead the drive to independence in 1960. A restless people, they have managed to ingratiate and to offend, to inspire admiration for their diverse talents and fear of their ambitions—real and imagined. Defeated in Nigeria's recent civil war and packed into a shrunken homeland, they have begun to push outward again—albeit slowly and more cautiously than heretofore. Interest in this remarkable group is not likely soon to subside.

Ibo ethnography and political commentary have generally emphasized the acephalous, decentralized character of the indigenous social structure. Viewing the extended family or kindred groups formed by several such families as the core of social organization, Western scholars have tended to lay stress on the autonomous village or village group as the highest and most extensive political unit. Occasionally attention has been called to the existence of still wider unity—in Ibo consciousness-and empirically. Many Ibo clans are organized across local jurisdictions, with the members aware of kinship deriving from a putative common ancestry and extending beyond family and village levels. And there is a tradition of hereditary kingship in some areas of Iboland, with kingdoms which are usually larger than the village group. *Studies in Ibo Political Systems* probes the articulation of monarchy and statehood in Ibo culture. Such a work is long overdue. The analysis by Ikenna Nzimiro—an anthropologist at the University of Nigeria, Nsukka—adds richly to Ibo ethnography by extending its parameters.

The study concentrates on four Ibo communities bordering the Niger River—Onitsha, Abo, Oguta, Osomari—and is organized in two parts. Part One, which draws on colonial records and on the author's field interviews and personal observations, examines each state's myth of origin; its political structure and overriding goals and purposes; and the ways in which the king and a graded hierarchy of chiefs execute the functions of policy making and implementation, adjudication, and defense. Whereas the founders of Onitsha, Abo, and Oguta modeled their new states after the Benin Kingdom, from which they reportedly fled east, the progenitors of Osomari copied key elements of the Igala Kingdom at Idah, from which they had journeyed south.

The second part of the book focuses on the monarchical institution, analyzing its day-to-day operations in the several states and its effective embodiment of the community's political values. The sacred character of the institution is also examined, as are the rituals which underpin it. Emphasis is laid upon the king's role as a secular political leader—bound by law and separable from his office in the event of misfeasance or infirmity. A hierarchically organized system of councils and courts is intended, says Nzimiro, to resist autocracy and to enhance the vitality of constitutional government.

Pressured on all sides by migration and by the need to operate within the larger and ever more constricting framework of modern government, each kingdom has nevertheless managed to maintain its identity and its essential institutions. The stresses of social change which have intensified since the early days of colonial rule are absorbed, in part, by the sense of continuity of kingship and statehood which dominates the members' consciousness. As Nzimiro tells it:

> At a reconciliation meeting organized by the former Premier of the Eastern Region to settle the chieftaincy dispute at Oguta, the Premier [who comes from a part of Iboland which does not have the monarchical institution] wondered how the two political blocks [*sic*] consisting of leaders with university training, profes-

sional men, such as lawyers, doctors, engineers and also wealthy businessmen, could dissipate their energies over an institution he regarded as "anachronistic" in the modern society. Someone, however, retorted, "You do not understand what kingship means in our life, because not being a member, you have not been nurtured in the emotional streams of life that pervade the office" (p. xvii).

Studies in Ibo Political Systems does not examine separately or in any great detail those values which Western scholars are wont to attribute to Ibo culture, viz., social status acquired individually, not inherited, and three main principles of role allocation: universalism; achievement-orientation; and to some degree, specialization, especially where, according to S. N. Eisenstadt, activities are related to the attainment of instrumental gratification and economic and social goals. Pertinent material which may be culled from the study does suggest, however, that that attribution may be overdrawn. Clearly not all Ibo communities are acephalous. Nor, as Nzimiro points out in his four case studies, is Ibo society without distinctions based on ascriptive, hereditary criteria: royals, commoners, slaves, and so forth. These two findings together underscore the existence of identifiable culture areas *within* Iboland. The conclusion which Nzimiro draws from all this is an appropriate one: "that to understand the political system of the Ibos, each of [their] culture areas should be investigated in detail and the pattern of [their] political structure[s] described and defined" (p. xiii). This caveat extends to other African peoples as well, including some in Nigeria. I have in mind, for example, Robert A. LeVine's comparative study of achievement motivation in Nigeria (*Dreams and Deeds*, [Chicago: University of Chicago Press, 1966]), wherein he imputes central tendencies in status mobility throughout Hausa society from a cursory examination of a single area, Zaria, in Hausaland. *Studies in Ibo Political Systems* is to be commended both for calling attention to the risks inherent in making such imputations in social research and for filling in the interstices of political life in four Ibo states.

ALVIN MAGID
SUNY, Albany

Peasants in Revolt: A Chilean Case Study, 1965–1971. By James Petras and Hugo Zemelman Merino. (Austin: University of Texas Press, 1972. Pp. 154. $6.50.)

This is an odd and unsatisfactory book. Although it is quite short and is, supposedly, a case study, there are many pages of theory, not always necessary or novel, and relatively few pages about the case under review, the seizure of the estate Culiprán by Chilean peasants in 1965. Though the subtitle indicates that the book covers the period 1965 to 1971, there are in fact three pages covering the period after 1965, which is a great pity. I would gladly have exchanged all the wordy speculation about psychological typologies of peasant consciousness for some examination of how the estate was run after the peasants took control.

One aim of the book (p. xi) is to dissent from the "standard image of peasants as passive, ignorant, submissive; in other words, traditional subjects." But surely this image, if widely held (even though not necessarily true for, say, Ecuador), was hardly widespread in Chile in 1965. Moreover, examination of an estate *seizure* is hardly likely to confirm an image of passivity. Professors Petras and Zemelman also equate the desire of the peasants for land after the takeover as a process of bourgeoisification. But why? In the first place this theory of *embourgeoisement* is hardly proven in the book, given the totally inadequate discussion of the post-takeover period. And a desire for land is surely a perfectly understandable desire for security in an unstable political and economic context, and one where, at least in the period before Allende, the predominant economic mode was capitalism.

How representative was Culiprán of similar upheavals in the Chilean countryside? This question is difficult to evaluate because we are given practically no information about the economy of the farm. In fact it was a relatively traditional farm, with a low level of mechanization and in serious economic difficulties (facts which are not discussed by the authors). The crisis represented a decline rather than a response to the pressures for a more commercial agricultural system. Where those pressures existed, the more typical response was to create a rural trade union and engage in collective bargaining rather as the urban unions do. Little in the book situates Culiprán in the context of Chile's agrarian structure and the changes of the 'sixties. There is a short and inadequate chapter on the political background, with some very misleading tables. For example Table 8 (p. 11) compares "Populist" and Socialist voting trends in the 1952 and 1958 presidential elections. But the so-called "Populist" candidate in 1952 was, in fact, supported by the majority Socialist party; the other Socialist candidate had the support only of the minor party, and of the Communist party which was at that time illegal and whose members were largely disenfranchised.

On the positive side, the book contains some interesting material, including some good case studies of individuals, but these remain at a rather abstract level of psychological speculation. There are also some interesting ideas, but they too remain at the level of abstract theory and are not

really related to the process supposedly under examination. Reading this book is, therefore, a rather frustrating experience.

ALAN ANGELL

Oxford University

The French Revolution of 1830. By David H. Pinkney. (Princeton, N.J.: Princeton University Press, 1972. Pp. viii, 397. $16.00, cloth; $8.50, paper.)

Professor Pinkney's account of the French Revolution of 1830 has no obvious rival even in French, and it is most unlikely to be emulated, let alone superseded, by a work written in English for some decades. It records some novel and interesting research of Pinkney's own and it is agreeably, if unremarkably, written. It will be widely and deservedly used by students of French history. As a piece of historical analysis, it is competent, sensible, and at times very perceptive. There is one perspective, however, in which it achieves something less than a complete success. Political scientists can look to it for a number of illuminating insights into revolutionary processes and for some instructive items of skepticism: they will not be tempted to regard it as a model for the causal analysis of revolutionary processes. Indeed they may well conclude after reading through his narrative that Pinkney is unable to offer them any causal explanation of the occurrence of the revolution whatever.

It is not unreasonable in a historical work intended for a specialist audience tacitly to assume much knowledge as shared between author and readers and to cast one's explanatory account accordingly. A reader who came to the present narrative without a detailed knowledge of French political structures, society, and economy in the Restoration period might, by the time he had completed his reading of it, have arrived at a reasonably clear and vivid picture of the events of the revolution of 1830. He might even have an adequate idea of the sort of political and military process which plainly constituted the core of the revolution. But he simply would not have a sufficiently clear and systematic conception of the political, social, and economic structures within which this process took place to be able to offer any determinate explanation of why it was *this* and not some radically different process which *did* occur. This failure to provide any clear framework for his narrative shows up with particular sharpness when Pinkney himself essays a more ambitious level of analysis, particularly, for example, in chapter 9. Here he treats the well-known view that the revolution of 1830 represents the political displacement of the aristocracy by the *haute bourgeoisie* as a hypothesis solely about the social status of legislators and senior public officials. It may be that this hypothesis is badly conceived and it certainly is often none too clearly stated. But if it has any standing at all it is as a hypothesis about the relationship between changes in economic and social structure and changes in political power. Most of Pinkney's discussion fails entirely to address this issue.

Besides this more general analytical disengagement, there are two particular themes over which Pinkney passes with disappointing haste. For the first of these, the part played by the exponents of the French revolutionary tradition in the events of 1830, there are at least sound historiographical reasons for his decision. Revolutionaries do appear to have been less important in 1830 than they were in 1848 or 1871, let alone in revolutions of the twentieth century. But even so the treatment of the part played by republicans in 1830 is both skimpy and dubiously consistent, and it certainly gives a misleading impression of the relation of the revolution of 1830 to the French revolutionary tradition to omit a single mention of figures like Buonarroti and Auguste Blanqui from either text or bibliography. The other virtual omission, a discussion of the very interesting series of recent papers in which Charles Tilly has attempted to relate the process of economic and social modernization in nineteenth-century France to changes in forms of mass political violence is still more regrettable. (See esp: James Rule and Charles Tilly, "1830 and the Unnatural History of Revolution," *Journal of Social Issues*, 28 [1972] 49–76.) Perhaps the most substantial compensation for these omissions is Pinkney's graphic description of how the government lost military control over Paris in the three days of late July 1830, "*Les Trois Glorieuses*," and the ensuing explanation of why the new government contrived so soon to restore an admittedly rather shaky control. These pages bear the mark of an able historian of the city of Paris. They also provide a fair sample of the sort of insight for which political scientists may reasonably look to historians and for which they ought, after professional demarcation disputes are concluded, to be duly grateful.

JOHN DUNN

Kings College, Cambridge

Swaziland: The Dynamics of Political Modernization. By Christian P. Potholm. (Berkeley: University of California Press, 1972. Pp. 193. $8.00.)

Although he has ruled his nation since 1921 probably few students of comparative politics have heard of the ngwenyama of Swaziland, Sobhuza II. Yet anyone who is seriously concerned with the role of traditional authorities in the process of political change should read this slim book to learn about the success of the ngwenyama and his traditionalist supporters in

maintaining and expanding their power during the post-1960 decade when "modernity" shifted the contours of politics in one of Africa's smallest states. Christian Potholm, Associate Professor of Government at Bowdoin College, argues convincingly that the Swaziland case offers "an amendment to that portion of developmental theory that stressed the basic incompatibility between 'modern' and 'traditional' political forces and the fragility of traditional authorities in postindependence Africa" (pp. 129–130).

When "modern" politics intruded in the form of (1) a British colonial administration seemingly determined to impose a one-man, one-vote constitutional monarchy oriented toward detribalized Swazis, (2) white settlers and their South African allies across the border eager to secure their privileged position while simultaneously advancing goals of the policy of separate development enunciated by Pretoria, and (3) political parties led by African political figures dedicated to the elimination of the traditional political system, Sobhuza and his supporters reacted by confronting their adversaries within the new political arena rather than retiring defensively within the traditional political institutions. Potholm delineates the successful utilization by the traditionalists of "moddern" devices, including political parties, associational groupings, and mass involvement, to entrench their position in the evolving political system so that at independence in September, 1968, the ngwenyama and a traditionally based political elite responsive to him held hegemony not only over Swazi national institutions but also over the machinery of state bequeathed by the departing British.

In Potholm's view the triumph of the Swazi traditionalists is attributable to a variety of factors. He points to the rejuvenative features of a traditional political system which contains structures and techniques for absorbing new elements and providing avenues of upward mobility within society. He recognizes that the demographic homogeneity of Swaziland prevented Sobhuza's opponents from utilizing ethnic divisions to undermine his position. Of further significance was the distinctive white-dominated southern African context in which Swaziland remained relatively isolated from the winds of change sweeping the continent to the north while at the same time the Swazi traditionalists could play off the conflicting views of the British colonial authorities and the South African government to their own advantage. But Potholm also gives great credit to the factor of political skill—both the abundance of it exhibited by Sobhuza and the lack of it displayed by the leaders of the "modern" political parties who consistently substituted "wishful thinking" for "political realism and organization" (p. 71).

Recent events in Swaziland testify to the continuing timeliness of Potholm's analysis. In a royal coup in April, 1973, Sobhuza assumed full state powers, dissolved parliament, banned all political parties, instituted a 60-day detention law, and abrogated the country's Westminster-style independence constitution. Subsequently he established a constitutional commission to devise political arrangements in keeping with the "Swazi national character." Sobhuza's moves were a response to a series of limited successes by the opposition in the "modern" sector commencing with the first post-independence general election of May, 1972, in which the major opposition party gained its first toehold in parliament (3 out of 24 elected seats). In the ongoing struggle with his "modern" opponents Sobhuza has again shown his determination to thwart any threat to the hegemony of traditionally based institutions. The almost complete lack of opposition to his measures bears witness to Sobhuza's present domination of Swaziland's political life. The ultimate test of Sobhuza's success, as Potholm notes in his concluding chapter, will occur only after Sobhuza's death. In Potholm's estimation the traditionalists will contain challenges from the "modern" sector, including stresses generated in the unevenly and rapidly expanding Swaziland economy.

In closing Potholm places considerable weight upon the convergence of interests between the present Swaziland government and its powerful white-led neighbors. Although he is careful to point out that the Swaziland government opposes *apartheid* and endorses African majority rule in South Africa (e.g., p. 122, 143), Potholm characterizes the Swazi leadership as "committed to the status quo" (p. 143) and unwilling to "challenge the structure of Southern Africa in the foreseeable future" (p. 156). From this perspective Potholm's volume is not only a case study of particular interest to students of comparative politics, but also an analysis significantly suggestive about the patterns of dependence in Africa's most racially troubled region.

SHERIDAN W. JOHNS, III
Duke University

Government and Economic Development. Edited by Gustav Ranis. (New Haven: Yale University Press, 1971. Pp. 567. $17.50.)

The volume contains papers prepared by staff members of the Yale Economic Growth Center for a conference organized by the Center on "The Role of Government in Economic Development." This collection is a by-product of the Center's country analysis program that has covered some twenty-five developing countries.

The papers have been classified according to particular governmental activities in the economic sphere in selected countries. These activities

include the apparent priorities of the government (Colombia and Malaysia), the operation of the public enterprise sector (Turkey and the former British Africa), the regulating function of the government (Ghana, Chile, Argentina, and Israel), and public finance (Taiwan, Mexico, and Peru). Finally, comparative studies deal with topics such as changes in policy orientation during the postwar period, the effects of currency devaluations in developing countries, and the relationship between public sector saving and capital formation.

The comparative studies offer especial interest to the reader. The Cohen-Ranis paper describes the adverse effects of extreme import substitution policies and the move towards trade liberalization following the realization of the failure of these policies. In turn, Cooper shows that devaluations have been generally successful in developing countries without involving appreciable changes in the terms of trade and that they tended to be accompanied by increased foreign aid and domestic savings. Finally, Lloyd Reynolds reaches the conclusion that developing countries have a higher share of public investment than present-day developed nations and that such investment is sustained by an inflow of foreign aid.

While most of the country papers are limited to the discussion of a particular governmental function, several of them consider policy formation as a whole. Among the latter, Hymer provides an interesting review of policies followed in the colonial and the postcolonial period in Ghana, although he may be excessively harsh on the colonial government and is apparently unwilling to criticize the policies followed by Nkrumah. In turn, Diaz Alejandro correctly notes the deficiencies of both macropolicies and the public sector in Argentina and makes suggestions for future improvements. In view of Israel's excellent growth record, Pack, on the other hand, may overemphasize inefficiencies in resource allocation and in the use of macro policy instruments. Finally, Ho provides a balanced picture of Japanese policy in Taiwan during the interwar period and the Second World War.

These contributions, as well as papers by Land on the public enterprise sector in Turkey and by Frank on that sector in former British Africa, Mamalakis's discussion of the Chilean nitrate sector, and Clark Reynolds's study of public finance in Mexico emphasize the effects of governmental action on economic growth. Finally, Hunt examines the relationship between economic growth and income distribution in Peru, and income distributional considerations also enter into the discussion of governmental objectives by Berry in Colombia and by Snodgrass in Malaysia.

Despite these last three efforts, income distribution plays a secondary role in the volume, reflecting the general tendency in economics at the time the papers were written. It may be interesting therefore to speculate on how the role of the government might be approached today. First, greater attention would need to be paid to the objectives of employment and equitable income distribution and to the trade-off between these objectives and economic growth. Second, one should attempt to derive a preference function, expressing the relative valuation of these objectives. Third, consideration would have to be given to the appraisal of investment projects by the government, involving the use of shadow prices and an explicit preference function.

The last paragraph may be considered an agenda for a future conference on the government's role in the developing countries and should not be taken as a criticism of the volume under review. The authors of the papers have done a workmanlike job, and they deserve our gratitude for having examined various fields of economic action by governments in developing countries.

BELA BALASSA
The Johns Hopkins University and Development Research Center, World Bank

The Making of A Model Citizen in Communist China. By Charles Price Ridley, Paul H. B. Godwin, and Dennis J. Doolin. (Stanford, Calif.: The Hoover Institution Press, 1971. Pp. 404. $9.95.)

In the early 1960s the People's Publishing House in Shanghai published a set of Chinese language *Readers* for use in the first five grades of elementary school. The set comprised about 1,100 pages in ten volumes, there being two volumes for each grade. Assuming that the texts reflect the basic values which the Chinese Communist leadership wishes to instill in its primary school students, the U.S. Department of Health, Education, and Welfare some time ago commissioned a research project to "determine the significance of the *Readers* as a means of socialization." The product of that undertaking, this book under review, consists of a content analysis of all the 385 lessons in the ten *Readers* and a full English translation of 155 selected units.

Constituting the second half of the book, the translation is superbly done and apparently provides a comprehensive sample of the complete set of the *Readers*. The analysis takes up seven chapters. The first one contains, among other things, a description of the *Readers* and a general appraisal of their role in political socialization. It is surprising that the authors fail to consider that language texts are not the only elementary school textbooks used to mold young children's value orientation and behaviorial patterns as well as to

teach them factual knowledge, although they do point out that elementary education itself is but one part of Chinese children's interaction with the society. This oversight not only makes it virtually impossible to assess the relative importance of the *Readers* as a means of socialization but it may also have led the authors subsequently to engage in elaborate but unrewarding statistical exercises, especially those dealing with the frequency and distribution of informational, political, and behavorial themes by grade level, where differences seem mostly inconsequential. Besides, there is the assertion (p. 6) that Mao's distrust of the intelligentsia is based upon the realization that the core of China's intellectuals were educated in the West. If Mao eschews only Western influence, why has there been a concerted campaign against Confucianism and many other aspects of traditional Chinese culture?

The next chapter purports to examine "the spirit and content of elementary education since 1949," but it completely ignores all the important changes made since the onset of the Cultural Revolution. This omission, unremedied elsewhere in the book, is likely to give a novice reader the impression that all the fundamental values and behavorial patterns extolled in the *Readers* still represent current policy. For example, he cannot learn from this work that, instead of emphasizing "obedience and deference" as recommended behavorial characteristics (as the Peking regime did in the past through the *Readers* and many other media), the Maoist leadership since the Cultural Revolution has been vehemently condemning the policy of "training docile tools."

Chapter III inquires into techniques for "the teaching of reading" for the stated purpose of providing "further evidence that the stories in the *Readers* are used in the classroom for conscious political ends." Since Peking openly insists on the use of education, literature, and even arts for that end, is this piece of evidence really necessary?

Chapter IV lists, by categories, all the themes in the *Readers*, tabulating the frequency of their occurrence, and makes generally appropriate and helpful commentaries on the themes' significance to the attempt by the Chinese leadership to develop the nation's agriculture, industry, culture, and defense, while reforming man and society. Chapter V offers a statistical analysis of the kinds of "topics" or stories used by the *Readers* to project the themes identified in the preceding chapter. The effort, however, is more redundant than illuminating because of the considerable overlapping between "topics" and "themes," as the authors themselves admit (p. 162).

More redundancy afflicts the next chapter, where all the factual knowledge, beliefs, values, and personal characteristics that the *Readers* seek to inculcate are once again listed to yield the profile of the ideal child-citizen which could have been concisely drawn in the concluding chapter. What appears in the latter is a discussion of perceived "conflicting values in the *Readers*." In at least one case, however, the so-called contradiction clearly is the result of the authors' own inconsistency. I refer to the alleged conflict between "themes of achievements" and "themes of collective behavior." This point is raised in complete disregard of their own earlier remark on p. 140 which reads as follows: "It should be pointed out that 'achievement' in the sense used here, in keeping with the social character of the Communist ethic, refers not so much to *personal* achievement as it is related to wider social goals."

In conclusion, this book could have been far better if it had avoided the flaws I have mentioned and if it had included a comparison of the Chinese practice with that of other countries also utilizing school texts in the interest of political integration and social change.

FRANKLIN W. HOUN
University of Massachusetts

Origins of Franco's Spain: The Right, the Republic and Revolution, 1931–1936. By Richard A. H. Robinson. (Pittsburgh, Pa.: University of Pittsburgh Press, 1970. Pp. 475. $9.95.)

As in old Randolph Scott movies, so in the mainstream of historical writing about the Spanish Civil War it is always clear who the bad guys are. They wear blue shirts or red berets or khaki uniforms or black cowls. They issue jeremiads about the decadence of modern society, the evils of materialism, and the threat to order and decency posed by communism, socialism, and liberalism. Collectively, they are called the Right, and most non-Spaniards have very little good to say about them. Stanley Payne, of course, is a notable exception to this rule.

Some promising studies are on the horizon. R. Martin Blinkhorn has been working for a decade on what will surely be a first-rate study of the Carlist movement; and Payne has set his sights on the Basques, many of whom were quite conservative on every issue save one, regional autonomy. But most work on the Right to date has been either stridently polemical or seriously flawed.

Unfortunately, the new book by Richard A. H. Robinson, *The Origins of Franco's Spain*, fits into the latter category. Much impressive research has been used to poor effect in this classic example of the "one-damned-thing-after-another" school of historical writing. Robinson amply demonstrates that a large number of different groups with distinctive political views constituted "the Right," but he does not explain why there were so many,

how they differed from one another (except in the most hair-splitting, ideological terms), what social and economic factors affected their development, and what their significance is in a larger time frame of Spanish history. For example, differences in political persuasion among groups on the Right are explained in terms of abstruse variations on the themes of Monarchy and Nation, not in terms of the social origins or economic interests represented among the various Rightist political factions. Robinson probably knows as much about the CEDA in particular and the politics of the Right in general as anyone in the world, but this knowledge is not employed to inform reasoned analyses which might clarify a dizzying picture of right-wing fragments coalescing to do successful battle with left-wing fragments. Robinson's bibliography is comprehensive, and his detailed notes are particularly valuable. His prose is pedestrian, and his organization of material is unimaginative.

At one point, however, Robinson's relentlessly chronological approach does serve him well. In the last chapter, which covers the period from December, 1935 to July, 1936, he presents substantial evidence to support his thesis that the military rebellion occurred in reaction to the Republican government's inability to maintain public order. In Robinson's view, the pattern of increasing street violence and social unrest in the months after the Popular Front's election victory in February, 1936, was more cause than effect of the increased militancy and willingness by groups on the Right to countenance armed revolt against the Republic.

But explaining the Rightist coalescence around plans for an armed uprising primarily as a function of the Popular Front Government's inability to keep the streets safe and to stop strikes is telling only half a story. Before 1936, the Left in Spain threatened the prerogatives of the Church and the Army. Rightist political factions bridled at this attack on cherished institutions but did not feel sufficiently injured to come together in support of General Sanjurjo's armed uprising against the Republican regime. Agrarian reform, the only serious threat to the economic power of the Right (and only a small segment of the Right, at that), was emasculated by proper parliamentary methods after the Right's successful venture in electoral politics in the fall of 1933. After February, 1936, however, the Left emerged as a clear and present danger to the most hallowed of institutions, Property. The ascension of Largo Caballero and his newly discovered radicalism within the Socialist Party, the eclipse of Prieto and Azaña, and the increasing independence of trade unions marked a shift in the focus of the Left's attack on the Right. An increasing number of Rightists came to use Spain as a euphemism for Property, thus clothing class interest in the regal garments of selfless patriotism. Members of the CEDA leadership and other erstwhile parliamentary rightists came together in active or tacit support of the military conspiracy and uprising because they had come to perceive the Left as a fundamental threat to their basic class interests. Under Azaña, the Left was a nuisance which could be fought within the existing parliamentary mechanism; under a radicalized Largo Caballero (and his even more militant supporters), the Left seemed to threaten what really counted: Property.

As a detailed chronology of the rise of the Spanish Right during the 1930s, Robinson's *Origins of Franco's Spain* is a useful addition to the bibliography of the Second Republic. But as an analysis of the causes and significance of that rise, the book falls short. The truly important story of the Spanish Right, one comparable to Edward Malefakis's superb analysis of the Spanish Left, remains to be written.

GLEN A. WAGGONER
Columbia University

The Political System of Brazil: Emergence of a "Modernizing" Authoritarian Regime, 1964–1970. By Ronald M. Schneider. (New York & London: Columbia University Press, 1971. Pp. 431. $13.50.)

In the late 1960s it became evident that democracy and totalitarianism provided inadequate models for both prediction and explanation in analyzing Third World political systems; political authoritarianism has emerged as an alternative. Ronald Schneider's workmanlike volume is one of the most recent efforts to use the authoritarian framework to explain Third World politics, specifically of Brazil since the *coup d'état* of March 31, 1964. When they combine Schneider's book with Thomas E. Skidmore's *Politics in Brazil, 1930–1964* (New York: Oxford University Press, 1967), social scientists with a yen for facts and figures about Brazil should be satiated for some time to come. Unfortunately, the search for the conceptual relevance of political authoritarianism will need to continue beyond Professor Schneider's volume.

Schneider provides an intelligently detailed chronological exposition of the emergence of the post-1964 regime. After chapter 1, which deals with the theoretical issues I discuss below, the author summarizes (chapters 2 & 3) the theme of a second volume in preparation: the historical role of the Armed Forces from the end of the Empire in 1889 to the overthrow of João Goulart in 1964. The first three chapters occupy almost one-third of the book.

The theme of "manipulated democracy" (1964–1967), and the central focus of the book, emerges

in chapter 4. The details of the early jockeying for power among civilian and military leaders are carefully documented, and the author traces carefully the appearance of a "tutelary" regime. Divisions within the regime, austerity, and accentuated dissension emerge in Schneider's analysis of the post-1964 system. Chapter 5 discusses the Castelo Branco "dictatorship" and the period following the crucial elections of October 1965. The failure of "humanization" during the ill-fated government of General Costa e Silva from 1967 to 1969 (chapter 6) and "the descent into dictatorship" (chapter 7) follow.

Chapters 8 and 9 consider the sixth year of the Revolution, the first of President Emilio Garrastazú Medici, in terms of "disunity, succession, and the quest for viability" (p. 297). Chapter 9 treats in particular the issue of the institutionalization of a "new authoritarian order" (p. 330). Schneider interprets the state of the Revolution, in late 1970, as one in which "deeper cleavages appear to be developing within the officer corps on both questions of fundamental policy and who should occupy the seats of power and prestige" (p. 361). He tells us that General Medici's "prospects for serving out his full presidential term would seem to be no better than the average for the Brazilian Republic—less than a fifty-fifty proposition" (pp. 361–62).

As of this writing, Brazil has peacefully selected a new military president to succeed Medici without crisis or breakdown. Normalcy, within the framework of the authoritarian regime that has become rapidly institutionalized since 1964, is the order of the day. Deep divisions within the Armed Forces have not emerged; there appears to be little visible social and political discontent as the bureaucratic-authoritarian regime enters its second decade. It is fair to ask then, why were Professor Schneider's predictions excessively pessimistic? To answer that question we must turn to the theoretical assumptions of his book.

Utilizing a framework that emphasizes the imbalance between levels of institutionalization and social and economic change before 1964 in Brazil, Schneider states that the regime crisis in March 1964 resulted in "the replacement of a populist, albeit reconciliationist, regime by the military's first experience with direct exercise of governmental authority and political power since the 1889–1894 period" (p. xi). From the demise of the reconciliationist regime (which was at least superficially "semi-pluralist" by 1964), Schneider proceeds to catalog the emergence of an authoritarian regime that he equates with dictatorship, repression and depoliticization. Employing an authoritarian framework ostensibly, the author insists on comparing Brazil's authoritarian system with a democratic model. The volume is replete with references to the failure of the kinds of institutions and processes endemic in *democratic* regimes.

If Schneider had employed criteria other than democratic ones, he might well have evaluated regime performance differently and, in so doing, enhanced our conceptual knowledge of political authoritarianism as well as of Brazilian political history. There is sufficient evidence in the research of Juan Linz, Guillermo O'Donnell, and others that there is indeed something distinctively different about authoritarian regimes. Such regimes are neither democratic nor totalitarian nor aberrations of either. By utilizing criteria represented by concepts such as corporatism, bureaucratic-authoritarianism, or patrimonialism, Schneider would have been able to analyze the dynamics of the regime and deal with questions such as why and how Brazil has been able to do what it has done, i.e., foster successfully its twin doctrines of national security and economic developmentalism. A record of "success" emerges if democratic criteria are held in abeyance and those of political authoritarianism are utilized: the regime had dealt successfully with three presidential successions; the economy flourishes; there is little discernible social discontent or political rebellion; and the armed forces appear as united as any such heterogeneous organization can be in a developing society. Employing the authoritarian framework rigorously, Schneider also could have dealt with the devastating criticisms of the regime that focus on social inequality, income distribution, repression, torture, regional imbalance and a host of other issues that can and should be examined in the context of the operating rules of the political system created post-1964.

It is of little relevance to state that the "modernization of Brazil under the aegis of the Brazilian armed forces might have become a possibility if military leadership came to accept the need for political institutions which are capable of integrating emerging groups, effectively articulating and aggregating interests (and thus mediating group conflict while building consensus), and fostering a sense of community" (p. 329). The military-civilian leadership of Brazil's authoritarian regime has determined that it is precisely the absence of these institutions and processes associated with democracy that facilitates their claim to and use of power. It also provides a sense of legitimacy based on economic performance and social "peace" that appeals to the middle and upper strata and it permits the development of Brazil on the regime's terms. The regime can best be dealt with as an example of an authoritarian order that has survived for a decade without serious challenge, having avoided critical social questions, emphasized economic growth at the expense of societal modernization, and created a

new standard of state authority and centralized political control.

The criticism of the theoretical framework of the volume should not detract from its richness as a documented history of the Brazilian regime from 1964 to 1970. From this perspective, it is an invaluable reference work and an important contribution to our knowledge of contemporary Brazil.

RIORDAN ROETT
School of Advanced International Studies
The Johns Hopkins University

Collective Violence. Edited by J. F. Short, Jr., and M. E. Wolfgang. (Chicago: Aldine Atherton, 1972. Pp. viii, 387. $12.50, cloth; $4.95, paper.)

We are nowadays offered so many grab-bag readers about everything and anything, that we are stirred when we come upon one which claims conceptual unity and editorial orchestration toward theory, or at least the advancement of research. The claim is not always substantiated by the offering. Such books should be submitted to the test of whether their component articles taken together yield a sequential narrative or analytic tendency coherent enough to engage the reader in a disciplined activity. This book fails that test.

Not to derogate the quality of individual contributions, whose usefulness the specialist reader will recognize, I conclude that when placed together under the loose rubric of violence and governed by a chatty editorial without focus, the papers yield no intelligible challenge to the reader. One simply does not know what to do with the book. In bringing together a set of papers previously published by the Annals of the American Academy of Political and Social Science with those prepared for a symposium of the American Anthropological Association, and topping these up with several others prepared for different purposes but now published for the first time, the editors do not succeed in providing, as they claim, "one of the most comprehensive comparative examinations of the topic ever published," but they do offer, as they also claim, "a comprehensive and updated compendium." The dual claim in which a compendium is equated with a comparative examination reveals the perennial confusion that arrests the progress of comparative analysis.

In the following summary I have condensed the thesis of each chapter into a sentence, arranged in the sequence of the book, to give a picture of what is offered. I have tried fairly to represent the book, and to avoid parody. I have not been able to discover any sense in which it may be regarded as an interdisciplinary enterprise.

Superordinate-subordinate relationships based on social categories are, we are told by A. D. Grimshaw, inherently unstable, and social violence is very likely to occur when accommodative structures lose their viability. Gary T. Marx points out that failure to make distinctions among riots in terms of their relation to ideology, social movements, and social change, produces confusion about the meaning and significance of riots. Currie and Skolnick aver that conventional "crowd theory" mistakenly assumes that collective behavior is irrational and necessarily based on distorted beliefs, while in fact it should be seen as an outgrowth of social, economic, and political conditions. In rebuttal, Neil J. Smelser questions the methodological validity and sophistication of his critics. H. L. Nieburg then suggests that much political behavior is ritualized as a "low-risk energy-conserving method of experimentation," and ritual rebellion contains a self-limiting tendency since it is impossible to sustain the level of excitement. While much attention has been devoted to the manner in which people may learn violent actions through the media, it would be more useful, says Sandra J. Ball-Rokeach, to investigate how evaluative reactions to violence are learned, that is, the manner in which the media influence the legitimation of violence. R. A. Berk points out that it is more difficult to gather data about episodes of collective behavior as they occur than about the conditions preceding crowd behavior.

The patterns of international warfare over the last century and a half, say Small and Singer, provide sufficient data for an assault [*sic*] on the causes of the war problem. T. R. Gurr offers the view that internal strife in "the West" could be predicted on the basis of factors relating to the scope and intensity of collective discontent, combined with the strength of normative justifications for rebellion and the coercive balance between contending groups; when these latter are held constant, non-violent strife increases when relative deprivation is diminished, while violence is more likely to occur when objective measures of relative deprivation show an increase. L. R. Schwartz concludes that in Mexico, where the legitimacy of the social system is solidly supported, and where high levels of violence and social conflict coexist but are not coextensive, strong social mechanisms operate to prevent violence between subordinate/superordinate groups. R. B. Edgerton finds that among tribal societies in East Africa which lack authoritative institutions capable of ordering force in maintaining social order, forms of collective violence evolve as mechanisms for maintaining social order. Similarly, L. L. Langness suggests that in New Guinea, violent self-help is prescribed as a method of social control, in the absence of authoritative institutions. In Burma, claims M. E. Spiro, when the British

suppressed traditional forms of institutionalized aggressive behavior, increased insurgency and political violence ensued, attributable to a "restructuring of the traditional hierarchical order of the response repertoire for agression" (p. 191). (Is this perhaps synonymous with the observation that its victims violently resented British rule?) A study of five "exotic" societies together with that of New England by J. W. M. Whiting reveals that societies that induce a high level of sex-anxiety and inhibition are likely to permit and even ritually to require the sadistic treatment of prisoners of war.

H. D. Graham contends that so far as the United States is concerned, dissidents are likely to focus increasingly on political targets, thereby undermining the legitimacy of political institutions and increasing the likelihood of further violence. Sheldon Levy follows with the view that persons who feel unrewarded by the political system tend toward psychological rigidity and identification with authority as a means of reducing anxiety. R. B. Smith finds that Vietnam "doves" are more liberal on domestic issues than "hawks," and less favorable to repressive action by authorities. John P. Spiegel considers that campus unrest can best be understood in terms of value conflicts, and could be ameliorated by changes in university values in the areas of authority relationships, the time-dimension of decision making, and in the relation of man to nature and to activity:

> With respect to the man/nature dimension, Spiegel proposes greater emphasis on harmony with nature, supplanting mastery over nature as the dominant value and moving subjugation to [*sic*] nature to the lowest priority. He points out that the proposed change is consistent with the new awareness of environmental pollution and emphasis on the quality of life (Eds., p. 17).

R. B. Smith suggests that the macrosociology of violence, in terms of belief systems and the social systems surrounding the university, does not exhaustively explain the violent events, but should be supplemented by a microsociology of the cumulative effects of the events themselves, as they unfold. W. B. Morgan notes that youthful dissent, which has replaced labor antagonisms as a major source of political violence, shares with its predecessor a tendency to press toward formalized influence procedures, where these are lacking. As to ghetto violence, Rossi and Berk find that grievances about merchant practices and police conduct have objective bases in experience, which, although manifest locally, can be studied macrosociologically by viewing the local community as a social system. Berk, in a case-study of a tense situation in which violence did not occur, shows that it was avoided by the failure of protest leaders to alter the payoff expectations of followers. Gamson and McEvoy find that privileged groups tend to support police violence, while G. E. Misner's study suggests that although police response is often a contributing factor to the escalation of violence, the police can have only a marginal impact on the problems underlying collective violence.

The book concludes with two short explorations of possible alternatives to violence by A. P. Hare and J. S. Campbell, in which optimism is expressed about the contribution that might be made by further close investigation of the problems by social scientists and public commissions.

The book offers the reader a welter of stimulating ideas and hypotheses, among which he may well find something new and usable to suit his own interest. As a collective enterprise, however, it resembles a crowd rather than a group.

N. LUCAS

University of Sheffield

When a Great Tradition Modernizes: An Anthropological Approach to Indian Civilization. By Milton Singer. Foreword by M. N. Srinivas. (New York: Praeger Publishers, 1972. Pp. 430. $15.00.)

Milton Singer reports in this book the results of seventeen years study of India. He first undertook this activity in the early 1950s as an associate of the late Robert Redfield in a project to describe and compare the great and enduring cultural traditions of mankind. Readers will find generous interpretations and precise detail in these discussions of modern Indian society and the social change within it, elaborated both with theoretical and empirical evidence. Meticulous readers will discover that this also is a record of intellectual debt to many in India and elsewhere who, over the years, have contributed to a better understanding of the text and context of Indian civilization. Finally, one notes that the remarkably able group of Indianists at Chicago, much of whose research also is cited in this book at appropriate places, was itself a creation of Milton Singer during the same period of research. Robert Redfield, we can be certain, would be most pleased with the results of his colleague's India venture.

Singer's book, however, will not appeal to most political scientists. Indeed, the chapters on the "Social Organization of Sanskritic Hinduism," on "*Bhakti* in the City," and on the "Rādhā-Krishna *Bhajanas*," while enormously well written and useful to specialists, will appear to others to be remote. These chapters, however, linked with others, form a logical sequence that comes to grips with major theoretical problems relating to social and cultural change in modern India. Singer chose to understand the social organization of Hinduism by a close observation, over time, of its working out in Madras City. In doing this job

properly as an experienced anthropologist, he observed, sometimes participated, asked questions, sought interpretations, and then carefully recorded his findings before going on to the next procedure. The book, in this sense, is a formal record of field notes and interpretations laid out roughly in the chronological order of their development. Some parts have been published before. Some segments also contain preliminary remarks that are repeated in more than one place. Familiar references now and then are rather comforting, however, in an analysis as complex as this, and no harm is done by the repetition.

In my view the capstone comes in Chapter 8, "Industrial Leadership, the Hindu Ethic, and the Spirit of Socialism," a study of the behavior of leading industrialists in Madras City, in which Singer demonstrates convincingly, and contrary to Max Weber, that Hinduism does not impede modernization. Singer's conclusion is based on his observation of the individual and family adaptive strategies used by the Madras industrialists in solving situations of potential conflict between ritual duty and business acumen. Resolutions of conflict appeared to be made relatively easily, if necessary by compartmentalizing one part of life (ritual) from the other (business). It may be that Singer's theory has less a ring of the general to it than Weber's "law of ritual," but it has the still greater advantage of being plausible.

This is a book about India, but it is more a book about Madras. And in Madras, it is more about Smārta Brahmans than any other group. Fortunately, Milton Singer had many able advisers in Madras to help him sort out the complexities of ritual life in that great city. None was more important than Professor V. Raghavan, a humane and learned man whose knowledge of Madras and of Smārta Brahmanical lore is unexcelled. It is typical of Singer's work that Professor Raghavan's contribution is thoroughly attested, as is the influence of M. N. Srinivas on anthropological issues of an all-India character.

Milton Singer concludes in this work on India that there is no necessary schism between tradition and modernity. Change takes place, led by individuals and families, and this change, in turn, may have its impact on organized religious and social life. The legitimization of tradition seeks then to strike a balance in the changing scene. Even the very new prefers the patina of a heritage, if it can be managed.

The trouble is that this natural process of social and cultural change may prove to be inadequate to harness the strain of the new political and economic demands for change being placed on this old society at the end of the twentieth century. Unfortunately, the pace of change is not discussed in this work. Nor are the objectives of change highlighted. To the most radical in India, a growing number, most or all of what Singer writes about is absolute mumbo-jumbo: a record of what should be scrapped, not revered. This is, of course, a minority view; more a cry of outrage than careful social science. Singer and others have demonstrated the linkage in India of tradition and modernization. Unfortunately, that does not help the human situation very much. It does force one to find other reasons to explain why nothing much happens quite the way it should in India

RICHARD L. PARK
The University of Michigan

Resources Devoted to Military Research and Development: An International Comparison. By Stockholm International Peace Research Institute. (New York: Humanities Press, Inc., 1972. Pp. 112. $3.75, paper.)

The question of the relative size of military research and development (R & D) programs in the United States, the Soviet Union, and other nations has bedeviled defense planners and students of military affairs for many years. The field is plagued by data insufficiency, by analyses governed more by political objectives than by the canons of scientific investigation, and by conceptual difficulties concerning the meaning of fiscal comparisons among sharply differing economic and political systems. In 1971, the Stockholm International Peace Research Institute undertook a comprehensive study of military R & D with a view toward clarifying some of these ambiguities and estimating total worldwide expenditures on military R & D. Their analysis, within the degree permitted by shortcomings in available data, is admirable. And this volume, which reports their findings and presents the supporting data, should be valuable to students of military affairs, arms control, and comparative politics.

Two kinds of data are examined. The main analysis focuses on published data regarding national R & D expenditures. Such data are available for about twenty nations—the United States, Canada, most of the states in Western Europe, plus Japan, India, and Australia. Unfortunately, this analysis, which would have provided the most satisfactory basis for the desired estimates, could not be extended to the Soviet Union, China, or their allies, because none of these states make public such detailed information on their military programs.

Analysis of expenditure data is supplemented by a survey of the number and kinds of weapon development programs under way in various nations. But this analysis too, which is based primarily on material appearing in standard, publicly available sources—such as *Jane's Yearbooks*

and *The Military Balance*—also suffers from data deficiencies caused by the secrecy imposed on such matters in many states. *Jane's Fighting Ships*, for example, can report only those Soviet warships which have been developed to the point that they are readily visible to foreign intelligence systems, but plans for U.S. warships are discussed publicly long before the vessels themselves are constructed, and are reported as such in *Jane's*. Consequently, there is likely to be a bias in the weapon-system data leading to lower estimates of the size of Soviet R & D programs.

The authors are well aware of the difficulties involved in estimating Soviet R & D expenditures and treat the subject gingerly. In fact, a separate appendix is included on just this topic. And SIPRI's own estimate of Soviet R & D expenditures in the 1960s—three-fifths to two-thirds those of the United States—does not appear unreasonable to me.

The one weakness apparent in the report, aside from those traceable to insufficient data, is a somewhat excessive zeal to further the cause of reduced military spending. This bias leads the authors, for example, to ignore the possibility that some R & D programs may lead to less dangerous weapons (such as the slower reaction times made possible by modern ICBMs as compared to the first-generation liquid-fueled monstrosities). It also causes them to downplay the significance of the leveling off of investment in military R & D evident during the past several years.

On the whole, though, the SIPRI report is factual and even-handed. And in providing a comprehensive, empirically-based, and objective analysis of this highly political subject, the Institute has performed a valuable service.

BARRY M. BLECHMAN
The Brookings Institution

The Rope of Moka: Big-Men and Ceremonial Exchange in Mount Hagen, New Guinea. By Andrew Strathern. (New York: Cambridge University Press, 1971. Pp. 254. $17.50.)

Papua New Guinea has been a gold-mine for anthropological studies for some time now. *The Rope of Moka*, another anthropological undertaking cast in the context of the New Guinea Highlands, shows, however, that political scientists can find as much value in the study of preliterate societies as anthropologists can. The book examines the process by which "big-men" emerge in small segmentary societies and points to the role they play in intergroup diplomacy specifically in instigating, mediating, and settling disputes.

The New Guinea Highlands, until recently opened up to external western influence, has been one of the few areas in the world with societies still solidly caught up in stone-age technology. No tribal empires or lofty pyramidal authority structures exist here. The tribes are numerous and speak in most cases mutually unintelligible languages. The typical clan in which an individual defines most of his life contains 70 to 3,000 persons. No formal governmental structures with established offices or inherited echelons exist. The units are economically self-sufficient, and until the arrival of the *kiap* (the Australian patrol officer), clan communities were engaged in constant endemic warfare with at least some of their neighbors.

For the student of comparative politics steeped in the study of political processes that are associated with easily identifiable structures in more differentiated and complex societies, the absence of formal political bodies in New Guinea could be bewildering. The "big-man," one of the few visible political figures in these societies, is a person who does not himself hold a monopoly on collective community decision making, has no direct physical coercive method to implement his will, and is generally one of several "big-men" in the clan; above all, he may not remain a big man all his life, and even if he does so, usually he cannot bequeath his influence to his sons. These societies are veritable meritocracies in which influence and a following are derived from performance in accumulating and distributing wealth in the form of pigs, pearl shells, and, nowadays, Australian money.

The significance of Professor Strathern's book lies in its systematic analysis of how power and influence are acquired and lost in these segmentary societies. *Moka* ceremonies are the forum in which participants derive or lose prestige. "Moka" in a gift exchange ceremony is the excess that one partner gives over another. The *moka* gift is the stuff of political influence since it is regarded not as a disinterested presentation but a form of obligation. In a typical *moka* transaction, the person who gives the bigger gift is regarded as superior. A big-man is a skilled giver who wins prestige by manipulating networks of partnership exchange relationships within and across clan lines.

Strathern argues that while big-men win influence in clan affairs through *moka*-making, certain latent social functions are performed simultaneously by these ceremonies. Given that each neighboring clan is a potential enemy, *moka* exchanges initiated by big-men and extending across clan boundaries serve to establish and stabilize peaceful linkages between groups. *Moka* transactions promote social and regional integration, therefore, and big-men act as "international" diplomats who manage these relations. Further, the author contends that given the termination of warfare in the Highlands, *moka* transactions with

their superiority-inferiority implications serve as surrogates in which warfare is carried out by other means: *moka* gifts "are a true functional alternative to warfare" (p. 128).

The *Rope of Moka* is a skilful description of how "leaders" are acquired in New Guinea's segmentary societies. For the political scientist unaccustomed to the language tools of the anthropologist, the book periodically becomes tedious, but this is partly overcome by the author's lucid style. The work could serve as a supplement for introductory students of political science as well as for comparativists to show how influence and power are acquired and exercised and how collective decision making is facilitated in polities which are diffused, informal, and sporadic.

RALPH R. PREMDAS
University of Papua New Guinea

The French Pacific Islands: French Polynesia and New Caledonia. By Virginia Thompson and Richard Adloff. (Berkeley: University of California Press, 1971. Pp. 547. $16.75.)

Serious work in English on the French Pacific territories can be counted on the fingers of one hand, so Thompson's and Adloff's volume is a welcome addition to a small shelf of books. Welcome not only because it fills a gap but also because it is a careful compendium of information on the French territories which digests the only slightly more extensive French literature as well as English, and adds to them the results of the authors' own work on the recent politics of the two island groups.

French Polynesia and New Caledonia as territories have little in common with each other except common French control. That, of course, is a great deal. Because of the French policy of "assimilation," in the more limited sense of assimilating the political and administrative organization of both territories to that of metropolitan France, Tahiti and the scattered small islands of French Polynesia, and the continental island of Melanesian New Caledonia, have almost identical institutions of government: a local assembly with limited powers which can advise and sometimes in local matters decide, but not ultimately control, the French governor who is analogous to the prefect of a *département* of metropolitan France. But such a similar structure of government in French Polynesia and New Caledonia works not in a vacuum but in local circumstances which are quite different. In French Polynesia, as a result of missionary activity and French education, of intermarriage and interbreeding of legendary fame, a society dependent on primary produce and tourism, plus the incidental benefits of the establishment for the French nuclear testing programme, behaves in ways not very dissimilar from the remoter, non-Parisian areas of France itself. In New Caledonia, the administration and urban work force of the nickel mining industry, and a country population which has not experienced the inter-breeding of Polynesia, exhibit much more obvious cleavages in society, of wealth, life style and color. The real interest of this book is the careful documentation of the way in which these differences work themselves out in party politics.

On the surface, the authors' description of the formation, the splitting, the re-combination of parties and factions in the local Assemblies looks very much like the political life of France during the Fourth Republic, which continued, in these territories where de Gaulle was a distant Glory and his party only one faction, under the Fifth. But only on the surface. As I showed in 1961, *Political Advancement in the South Pacific*, (Oxford Univ. Press) the French electoral process in Tahiti, with such a charismatic figure as the majority party leader Pouvanaa a Oopa, might rather be explained as a cargo-cult than a modern political system. The authors extend this view to the workings of political parties, especially with the career of Lenormand, in New Caledonia where the countryside, beyond the one major urban and industrial settlement of Noumea, has much more in common, at election time, with the millenarian movements of Melanesia than Polynesia ever had.

This local analysis is the strength of the book, the more valuable because of the authors' previous experience with other colonial areas. As a case study of what has happened in these two French Pacific territories to the date of publication, it has no rival. As a case study, too, it contains few inaccuracies of detail in its summaries of geographical, historical, and contemporary descriptions. Such reservations as I have are not on the local content of the book but on the consequences of this otherwise admirable concentration upon the island territories themselves.

Europeans writing on colonial and former colonial areas, currently hold that the "wave of the future" is to emphasize the local (in this case the Oceanic) setting, the Oceanic reaction to what colonial powers have done. It is as surprising in the Pacific as it is in Africa to find that this emphasis on the local, as distinct from the metropolitan aspect, is peculiar to Anglo-Saxon historians. Of course the local setting—geographically and culturally—is important in any description of what happens, but so is the metropolitan setting, especially when the colonial power is France. It is quite as necessary to understand why French governments, French politicians and administrators, and individual French men and women behave as they do, if one is to explain their

contact with Polynesians or Melanesians, as it is to explain the islanders' reaction. Both are part of the colonial and neocolonial situation. Indeed, given the concept of "France Overseas" it is particularly important to understand metropolitan attitudes and values. This is a dimension not exactly missing but certainly given much less importance than it deserves in this book. We may all agree that an Olympian view of colonial rule is misleading, but the proper alternative is not a "worm's eye view."

FRANCIS WEST
University College at Buckingham, England

German Resistance to Hitler: Count von Moltke and the Kreisau Circle. By Ger van Roon. Translated by Peter Ludlow. (New York: Van Nostrand Reinhold, 1971. Pp. 400. $12.00.)

The Kreisau Circle was a loosely constituted group of anti-Nazi Germans. Many *Kreisauer* were aristocrats; most were under 45 and held middle-level positions in the military or civilian bureaucracy. The name of the circle was derived from the Silesian estate of Count Helmuth James von Moltke where many of its gatherings were held.

Roon's book is a translation of a revised, abridged version of his *Neuordnung im Widerstand: Der Kreisauer Kreis innerhalb der deutschen Widerstandsbewegung* (Munich: R. Oldenbourg, 1967). Most of the footnotes are deleted, as are entire chapters. None of the circle's memoranda on economic matters is included in a long appendix containing documents.

Roon seeks to demonstrate the diversity of the circle's membership and contacts, ranging from Social Democrats to Jesuits and Protestant clergymen. He insists upon the innovativeness of its political ideas, the intensely Christian character of many of these ideas, the circle's pro-Western attitudes, and the differences that set off its members from the mostly older and more conservative anti-Hitler conspirators associated with Carl Goerdeler. Although some *Kreisauer* participated in the plot to kill Hitler on July 20, 1944, Roon believes that Moltke, who had been jailed some months earlier, remained steadfastly opposed to assassination, a practice he thought as bad as those of the hated Hitler regime.

In an introductory chapter unfortunately omitted from the English-language version Roon seeks to dissociate his work from the moralizing tradition of dealing with resistance movements in Germany, but he is still caught up in this tradition in more ways than he may be aware. Although also written in the same tradition, the moving biography of Moltke by two of his British friends, Michael Balfour and Julian Frisby, *Helmuth von Moltke: A Leader against Hitler* (London: Macmillan, 1972), is superior because its authors indicate more cognizance of their own preconceptions. Yet neither this biography nor the English-language version of Roon's work will foster much awareness among their readers of the issues involved in the controversies of the past four decades over German resistance.

A few examples may help to set Roon's approach in perspective. He fails to acknowledge—as a conservative survivor of the circle complains (O. H. von der Gablentz in the *Politische Vierteljahresschrift*, IX [1968], 592–600)—that most of the basic ideas of the *Kreisauer* had been developed by certain segments of the Right during the Weimar Republic. Indeed, anyone familiar with neoconservatism in pre-Nazi Germany will find little new in the programmatic statements drawn up by the circle. (See Walter Struve, *Elites against Democracy: Leadership Ideals in Bourgeois Political Thought in Germany, 1890–1933* [Princeton: Princeton University Press, 1973].)

Other possible historical continuities are also slighted by Roon. He finds only an unbridgeable gulf between the policies of Nazi Germany and the circle's plans for a federated Europe in which Germany would play a major role. The effective presentation of his own case requires more careful consideration of the relationship of these plans to the assumptions and objectives of other middle- and upper-class Germans, both before and during the Nazi era.

He does not confront the class character of the *Kreisauer* plans for post-Nazi Germany. Roon alludes to the wishes of the circle to avoid class struggle, but he fails to relate this desire to the repeated attempts by Moltke and others to obtain agreement that there would be no nationwide trade-union organization in the new Germany—a demand rejected by some of the trade unionists in the circle. Using the term "socialist" freely and describing Social Democracy during the Weimar Republic as "Marxist," Roon glosses over the significance of the absence of Marxists in the circle. If he had paid more attention to the rapidly growing body of literature on working-class resistance, he might have been able to explain better why anti-Nazi workers and bourgeois seldom cooperated. Works such as Kurt Klotzbach's *Gegen den Nationalsozialismus: Widerstand und Verfolgung in Dortmund 1930–1945* (Hannover: Verlag für Literatur und Zeitgeschehen, 1969) suggest continuities overlooked by Roon in the social and political struggles under the Weimar Republic and the Third Reich.

Still another issue has played too great a role in postwar historiography to be left unmentioned in this review. Although some Soviet and East German scholars have gone out of their way to em-

phasize pro-Soviet aspects of the *Kreisauer* (see, for example, Daniil Melnikow, *20. Juli 1944: Legende und Wirklichkeit*, trans. Friedrich Rehak [3rd ed., Berlin: Deutscher Verlag der Wissenschaften 1971]), Roon tends to ignore most such evidence.

Despite its limitations the German edition of Roon's work made an important contribution to the ongoing study of resistance movements. Unfortunately the same cannot be said of the English-language version.

WALTER STRUVE

City College, CUNY

Rébellions au Congo. By Benoit Verhaegen. (Brussels: Centre de Recherches et d'Information Socio-Politiques [CRISP], 1966. Volume I: Pp. 568; Volume II: Pp. 830. No price indicated.)

Ten years have passed since tattered bands of rebel Simbas brought to the former Belgian Congo (now Zaire) the promise of a "second independence" and nearly destroyed the fragile edifice constructed in the aftermath of the first: like hot lava pouring from a volcano, violence engulfed entire provinces; in less than six months more than half of the Zairian territory had fallen under rebel control; hundreds of thousands were killed by the insurgents, perhaps as many by the National Congolese Army (ANC). Only recently, however, have we come to appreciate the full impact of these events. Now that the seeds of the Congo rebellion have germinated in other territories (as in Burundi in 1972), producing further upheavals and violence by "artificial insemination," as it were, Professor Verhaegen's massive study seems all the more essential for anyone interested in the recent history of ex-Belgian Africa.

The volumes under review provide the most elaborate account in print of the genesis, unfolding, and ultimate collapse of the 1964 Congo rebellion, or indeed of any comparable phenomenon in independent Africa. One would look in vain for a similarly detailed analysis of the Biafran civil war, of the Southern Sudan rebellion, or the Rwanda revolution. The nearest equivalent for Biafra is Anthony Kirk-Greene's *Crisis and Conflict in Nigeria: A Documentary Source-book* (London: Oxford University Press, 1971, 2 vols.), which, however valuable, never reaches the broad sweep, meticulousness and clinical accuracy of the Verhaegen study. *Rébellions au Congo* is more than a richly documented, carefully researched case-study; it is a landmark in the literature on postindependence politics in Zaire, because of the imaginativeness with which the author collects, sifts, and utilizes his data, because of the extensive evidence he has to offer, and because of what all this tells us about the dynamics of revolutionary change in postindependence Africa.

Some of the themes explored by Verhaegen have already been treated in two excellent articles by Crawford Young ("Rebellion and the Congo," in Robert I. Rotberg, *Rebellion in Black Africa* [London: Oxford University Press, 1971], pp. 209–245) and Renée Fox, Willy De Craemer and Jean-Marie de Ribeaucourt ("The Second Independence: A Case Study of the Kwilu Rebellion in the Congo," *Comparative Studies in Society and History*, VIII [1965], 93–118). Many of the hypotheses advanced in these and other articles—that the rebellion owed its initial successes more to the receptivity of the milieu in which it grew than to the solidity of its organizational apparatus; that rebel tactics relied very heavily on the use of magic and hemp; that much of the momentum behind the rebellion came from the ability of the rural cadres to capitalize upon local issues; and that its ultimate collapse had at least as much to do with military defeat on the terrain as with internal dissensions within the rebel leadership—are once again confirmed by Verhaegen. In support of these hypotheses, however, the author brings a mine of additional factual information, and in so doing suggests new interpretations and insights.

Each volume bears witness to the author's privileged access to a wide range of confidential documents, most of them drawn from rebel archives. Volume I focusses on the Kwilu, South Kivu and North Katanga rebellions; volume II deals with the Maniema rebellion: each is a model of painstaking research, providing the reader with unusual insights into many critical aspects of rebel behavior and organization. Volume II, for example, devotes no less than 50 pages to a discussion of the "Forms and Functions of Magic" (in which we learn about the variable degrees of immunity obtained through different categories of *dawa*, or magic, how much each type of amulet sold for, what the ritual of immunization consisted of, who the witch-doctors were, etc.). With the same meticulousness, the author analyzes the organization and structure of the rebel government in the Maniema, dissects the internal rifts within the rebel leadership and brings us in contact with the issues and personalities involved.

Verhaegen shows an uncanny gift for bringing to life the key personalities that made up the rebel leadership: Soumialot, the central figure in the eastern rebellion, is depicted not as an anonymous, faceless sycophant, but as a man who, shortly after the capture of Stanleyville (now Kisangani), enjoins his *chef de cabinet* to enquire about the 19 goats he had left behind in Kindu, and takes pride in notifying in his cables that he is supporting his mistress out of his own pocket,

even though her promotion to the rank of *attachée de protocole* might have entitled her to a salary. His innumerable tiffs with Martin Kasongo, Minister of Press and Information, tell us as much about Soumialot's methods as it does about Kasongo's shifty and erratic behavior. Victor Tshombaz, Colonel in charge of military operations in the eastern region, emerges in a particularly sinister light; Ramazani, on the other hand, as *commissaire extraordinaire* in "liberated areas," strikes us as just the kind of a person some of us would like to have as departmental chairman—warm, fair, and intelligent. As the story goes on, the rebellion takes on the quality of an intensely captivating—sometimes nauseating—human drama. If this is what Verhaegen means by "*histoire immédiate*," it is history at its best.

Better than anyone else so far Verhaegen shows the complexity of the linkages between the local *focos* and the rebel movement. The incorporation of dissident local factions into the fold of the rebellion, its relationship to local and provincial arenas, its ethnic underpinnings and social bases are discussed in considerable detail. What all this adds up to is not one but several rebellions, each drawing strength from specific factors and circumstances, yet ultimately coalescing into a broad framework of revolutionary aspirations. Equally revealing are the chapters dealing with the organization of power in "liberated areas"; the impression one gains is that of an elaborate bureaucratic apparatus that never really gets off the ground, of a machinery permanently stalled by ethnic and personal rivalries and rampant corruption.

Seizing power is one thing; enlarging and institutionalizing revolutionary power is a very different matter. This is what the Congo rebels were never able to achieve; this is also where they shared disabilities very similar to those faced by the legitimate authorities. Ethnic favoritism, rampant corruption, overall inefficiency, and brutality characterized both the rebel and legitimate governments. The latter, however, enjoyed superior military power, thanks in part to the military backing of European and South African mercenaries, and that is what made the difference. In terms of sheer brutality one wonders indeed whether the ANC fared any better than the rebels. Unfortunately, the reader is told very little about the excesses of the ANC soldiery. Though it is a critical feature of the "crisis situation" described by the author, and therefore a legitimate area of investigation for any devotee of *l'histoire immédiate*, the impact of counter-revolutionary violence is nowhere adequately discussed. The "bad guys" are always the same in Verhaegen's story.

If for no other reason one is impelled to wonder whether the author's lengthy disquisition on the merits of *l'histoire immédiate* (appearing, oddly enough, at the beginning of volume II) is fully justified by his discussion. For if this new genre "always takes as its point of departure a crisis situation" (p. xxi), why not carry the story to its ultimate dénouement and look at the ANC repression with the same unprejudiced mind? Furthermore, if the aims of *histoire immédiate* are to deal with crisis situations in a variable time perspective, and on the basis of oral data, how different is this approach from that of behaviorally oriented political scientists? Is the label merely another term for what in this country is known as political science and in much of Europe still as constitutional history? Or is it a new kind of history? Merely to raise these questions is enough to cast doubts on the value of Verhaegen's theoretical effort; that they are being raised at the beginning of the second volume makes much of this effort sound like an afterthought, and at times like an apology for not having observed all the canons of *histoire immédiate* in the previous volume.

Once stripped of its pretentious theoretical wrapping, Verhaegen's study stands as a monumental contribution to our understanding of the 1964 rebellion. It is indispensable reading for anyone seeking to comprehend the genesis of Mobutu's Zaire, and indeed for anyone interested in comparative political violence. At a time when investigations of domestic strife are becoming increasingly fashionable among political scientists, Verhaegen's contribution can no longer be ignored. One hopes that each volume will soon be made available to American and English readers.

RENE LEMARCHAND
Washington, D.C.

Politics and the Power Structure: A Rural Community in the Dominican Republic. By Malcolm T. Walker. (New York: Teachers College Press, Columbia University, 1972. Pp. 177. $9.00.)

This book is a case study of community life in Constanza, a municipality located in the mountainous central region of the Dominican Republic. It is based on the author's field research while living in the subject community with his family in 1967 and 1968, gathering material for his doctoral dissertation in Anthropology at Columbia University. This book is the result of a reworking of that material.

A need unquestionably exists for the study of local politics in Latin America, one of the major gaps in our understanding of this region. The paucity of case studies on this governmental level, not to mention the absence of full-scale comparative analysis, prompts an examination of Malcolm

T. Walker's modest discussion of a single municipality in the Dominican Republic. The general subject under investigation is an important one and the specific setting is well-chosen—as Lambros Comitas says in his Editor's Note, "the Dominican Republic is an anthropological *terra incognita*." But all in all Walker's book is a limited contribution. As I shall try to make clear, the work has shortcomings as an exercise in political analysis, not as an examination of Constanza culture.

Organization of the study is straightforward, logically arranged on a topical basis. After a brief introduction, a chapter sets the stage with a general description of Constanza and its environs. Other chapters assess the community's history, economic life, social patterns, and politics. Topics such as family and interpersonal relations and formal associations are examined. The book ends with a set of much too succinct conclusions (two pages). Maps and a number of "real life" photographs add to the interest of the study. A very brief bibliography (28 entries) is appended; there is no index.

On the one hand this work is stimulating as a professional anthropologist's first-hand observations of the cultural and social dynamics of a largely unknown corner of the world. The volume, well written, serious, broad in its topical concerns and containing a number of valuable insights, reveals the author as both an objective and sensitive scholar. But the title is misleading, for politics and the power structure are treated only briefly in the overall analysis. Only the last two chapters deal directly with politics, comprising a scant 41 pages out of a total 173 pages of text.

One of these chapters is on the power structure and patronage, assessing the process by which community elites maintain their favored positions and control local politics. The major theme is the competition for scarce party-directed patronage rewards. Walker found, not surprisingly, that friendships and party allegiance far outweighed job qualifications in securing appointments. The various forms of political coercion used to maintain and perpetuate the status quo are also described. The other chapter is a fascinating examination of the events and crises surrounding the campaign and municipal elections of May 1968, exemplifying political conflict in the community and its resolution. While these two chapters are well done, they inadequately cover the subject which a reader could justifiably expect to be analyzed. More "events analysis" along the lines of the treatment of the 1968 elections in other political contexts and situations would have gone far to make the political compass more satisfactory.

The rich descriptive material about Constanza life throughout the book is not connected by a logical unifying approach. What, for example, does the chapter titled "The Life Cycle," dealing with childhood, courtship, marriage, old age, death, and mourning, tell us about politics when the author does not link these cultural contexts to the political system? Walker states his intention to view "the aspects of Constanza life that are given the most consideration in this study" (i.e., "the power structure and sources of power") in "systemic terms" (pp. 1 and 2). But his systems approach is neither sufficiently explained nor explicitly followed. The development of a precise model would have avoided the inclusion of nonpolitical material and the slighting of important topics (such as the crucial area of decision-making activities). Part of the role of a model, after all, is to help select and organize data. It would also have improved the book by providing a basis for comparability with other locales and establishing a framework for further research. Walker's study is concerned with a highly specialized subject which, to enlighten those not primarily concerned with the Dominican Republic or the Caribbean area, should validate its relevance to a broader problem through a conceptual approach such as systems analysis.

Perhaps the work should simply have been titled differently and its purpose stated so as to conform with what the author actually did. But I regret that Walker did not devote his demonstrated skills of observation and analysis to a more comprehensive treatment of the political aspects of Constanza community life.

G. Pope Atkins
United States Naval Academy

France, 1848–1945. Volume I: Ambition, Love and Politics. By Theodore Zeldin. (Oxford History of Modern Europe) (Oxford: Clarendon Press, 1973. Pp. 823. £6.00.)

What a good book! New facts and old facts in new perspective, thumbnail sketches of known and less known figures, fresh views and canny comments make this probably the best, certainly the freshest, volume so far in a distinguished series.

The best, and the most original. As the subtitle suggests, Dr. Zeldin has set chronology aside for a more topical and, as he calls it, analytical approach. The book is divided into three parts. *Ambition* treats of social groups, their ways and values: how did people make their way, their name, their money (or lose them), rise in their professions, introduce new techniques or ideas, use dowry or inheritance to achieve ambitions, live on *rentes* (at a time when money kept its value, and one could retire at 40 or inherit at 20, enough to last a lifetime). *Love* (?) deals with the private

aspects of life: marriage, morals, how children were brought up, how women fared at one social level or another, how both sexes were trained to fear their parents and each other, the role of routine (that of love seems negligible), and that of disease (at the turn of the century nearly 15 per cent of all deaths were from syphilis). Finally comes the political dimension—longest section in the book—where ideologies and parties are brilliantly reflected by a number of representative individuals, from Thiers to Combes and on.

The great interpretative tradition, here, is largely set aside in favor of description. The concretization and personalization of his themes sprinkles Zeldin's chapters with portraits of doctors and notaries, bankers and peasants, intellectuals and entrepreneurs, whose different backgrounds, careers and personalities illustrate the true variety of faceless categories too often imposed on unruly reality. Historical writing inflicts an unreal order on shifting, diverse existence; it structures living incoherence into something comprehensible, hence probably false to life. By introducing an *ondoyant et divers* tone into his book, Zeldin accepts the risk of perplexing some readers the better to excite and intrigue others. The perplexed will do well to supplement Zeldin by reference to Brogan or to Gordon Wright. But the risk was worth the taking.

A second volume will pursue the analysis into the fields of personal behavior which, it is true, are only thinly treated in the section dubiously (or ironically) titled "Love"; of changing fashions and mentalities; of provincial diversity and national unity; and of relations with other countries and with colonies. It will also contain the bibliography, whose absence in this volume is compensated by copious notes.

Theodore Zeldin knows his France: its realities as well as its literature and scholarship. French society appears in its nuances and contradictions: the clash of innovation and prejudice, the temptations of change and the survival of traditional modes, the continuity preserved through apparent breaks, the diversity of a highly centralized nation where pursuit of the golden mean often made for meanness, the glorification of small men and small virtues for pettyness.

Discussion is thoughtful, discriminating, informed. Item: "The proportion of young people who were both healthy and attractive was comparatively small" (p. 304). Item: " . . . the child remained an instrument for the gratification of parental aspirations" (p. 327). Item: "To concentrate on the scandals [of the 1890s] is to give excessive importance to symptoms and to lose sight of continuities and breaks of deeper significance" (p. 640). Item: "French politics can become clearer if one investigates not simply what the parties demanded but also the seemingly incoherent, colorless, compromising realities which followed from their demands . . . " (p. 698).

There is a judicious treatment of the Dreyfus case, taken largely in its human context: social psychology rather than politics, unless that is what modern politics is! There are rather too many misspellings (as on p. 313 where Montesquieu should read Montesquiou), to be corrected in the next edition. And, in the end, there is the vigorously brushed picture of an exasperating, awkward, uncomfortable, angry people—a people of *routiniers* and of *bricoleurs*—engaged in seemingly endless family quarrels that sharpen intellectual faculties as they do suspicion, mistrust, and great capacity for contempt. Timid and aggressive, brilliantly prejudiced, logically absurd, deeply embedded in a confusion of clear ideas, using words to avoid communication and courtesy to avert familiarity, the French continue to offer a fascinating conundrum, not least to themselves.

Zeldin has gone far to document their ambiguities and illuminate the inexplicable. Now for volume II!

EUGEN WEBER
University of California, Los Angeles

Bureaucracy, the Marshall Plan, and the National Interest. By Hadley Arkes. (Princeton, N. J.: Princeton University Press, 1972. Pp. 395. $12.50.)

In these days when book titles are frequently characterized by pop rhetoric or shameless overpromising, *Bureaucracy, the Marshall Plan, and the National Interest* is indeed descriptive of the content and major themes of this work. Professor Arkes's primary aim is to develop a theory of and practical methodology for describing the national interest. He argues that the national interest is fundamentally the preservation of an "essential core of values" and practices or the "way of life" of a political community (pp. 6–7). He employs the term "regime" from Aristotle via Leo Strauss to characterize this taproot of the national interest, defining regime as "the *character* of particular systems—the *particular* structure of authority and the *peculiar* pattern of values that distinguish one political system from another" (p. 7, emphasis in original). Drawing on Max Weber, he notes that "there is a fundamental relation . . . between the political regime and the character of the bureaucracy" (p. 13). Thus, if the choices made in creating and operating a bureaucracy like the Economic Cooperation Administration (ECA) are examined, then some of the policy values of the regime will be discovered. Further, since the regime is the basis of the national interest, examining the bureaucracy of a program like the Marshall Plan, which "stands out to us today as a

significant program, a successful policy, and a wise construction of the 'national interest'," will bring to light at least part of a concrete definition of the national interest in foreign policy (p. 4). The independent variable for determining the policy values and priorities in the establishment and operations of the ECA are "presumptions." Presumptions "distribute burdens of reasoned argument" among rival actors and agencies, and therefore assign priorities to the values that these actors and agencies represent (p. 182). The bulk of the book examines the creation and operations of the ECA, arriving at a formulation of the policy values it embodied. These values are then linked to the regime and the national interest.

The student of American foreign policy will find this attempt to operationalize the national interest rewarding and interesting. The use of the concept "regime," however, seems to substitute a new slippery term for the already available collection of slippery terms such as "political culture," "organic constitution," or even Montesquieu's "spirit of the laws." While this book does not establish a new level of clarity and rigor in the general debate over the definition of the national interest, its focus on bureaucratic "presumptions" does offer a handle on what the national interest is in practice in a given program during the life of that program and the agency created to administer it.

It is disappointing that the author does not explore the relations between his attempt to operationalize with bureaucratic "presumptions" the concept of regime and the survey research techniques employed in the studies of political culture. Would or should the elite behavior that produces bureaucratic presumptions highlight the same policy-value hierarchy as surveys of mass attitudes, and of what significance would the differences be for a theory of the national interest?

The author also insufficiently emphasizes that his examination of the Marshall Plan allows him to make statements about the national interest in terms of the value priorities *for that program.* Presumably, to arrive at a more complete description of the national interest for any historical period, the bureaucratic presumptions in several agencies and programs could be examined. How then could the collection of values that would be discovered in the history of the founding and operations of the Defense Department and the Atomic Energy Commission, which also began in the mid-'40s, be integrated with the values of the Marshall Plan to form this broader description of the national interest?

Students of organizational theory will find the concept of "presumption" particularly intriguing and useful. Chapters 9 and 10, which develop and define this concept and apply it to the Marshall Plan as a technique for discovering priorities of policy values, are the most thought-provoking of the book. This concept is of interest and utility because it focuses on the behavior of political actors which they rightly perceive to be important in how they get their jobs done and what they can do. For example, who can act unilaterally, who can act only after consultation, and what are the rights of appeal to higher authority of a dissatisfied actor? These questions are central to this part of the book, which is written with particular skill and sophistication and is enriched by examples not only from the Marshall Plan but also from such diverse sources as President Lincoln's views of the Fugitive Slave Act, American constitutional law, and President Truman's orders to MacArthur.

Students of the Marshall Plan will find a well-developed argument for a position between those who view the program as a pure expression of American altruism and those who see it as naked capitalist imperialism. The author's mining of the files of the Bureau of the Budget and the Brookings Institution brings some significant new data to light on the organization and operation of the Marshall Plan.

Students of the Congress will find a case study of strong congressional initiative and activism in shaping American foreign assistance policy. The argument concerning how congressmen abandon the standard cliches of administrative theory in order to build their value priorities into a new bureaucracy like ECA is very insightful.

This book is intended to be an interpretative essay using the case study of the Marshall Plan to illustrate an approach and to make theoretical arguments, in much the same way that Allison uses the Cuban missile crisis or Wohlstetter the attack on Pearl Harbor. The major defect of the book is that the various audiences, noted above, who might find the book useful unfortunately will probably also find it inaccessible. In analyzing the organization and operation of the ECA, Arkes uses terms like "drawing rights," "counterpart funds," and "letters of commitment" with great frequency and without defining or explaining them. Those without a background in international trade and finance will find the case study material, and thus much of the book, difficult and obscure.

THOMAS R. WOLANIN
University of Wisconsin, Madison

The Role of a National Delegation in the General Assembly. By Peter Baehr. (New York: Carnegie Endowment for International Peace, Occasional Paper Number 9, December 1970. Pp. 90. $1.00, paper.)

Mr. Peter Baehr is a lecturer in international

relations at the University of Amsterdam, and an editor of *Acta Politica*, the journal of the Dutch Political Science Association. In this monograph he seeks to take us inside the Netherland's National Delegation to the United Nations at the time of the twenty-third General Assembly session in 1968. His objectives are to expand the limited information available on the internal operations of a national delegation, on their decision-making processes, and on these delegations interactions with one another.

Mr. Baehr is aware that the usefulness of his findings is " . . . limited to those member states of the United Nations whose diplomatic practice is somewhat similar to that of the Netherlands" (p. 4). He indicates that most European states (east and west of the old iron curtain) would probably be included in such a grouping (e.g., those states with strictly instructed delegations). Delegations of many new African and Asian states would be excluded.

In examining the "Activities Within A National Delegation," Mr. Baehr notes that the Netherland's delegation is composed of professional foreign service officers and parliamentarians. In treating the impact of its composition upon the delegation's decision-making process he suggests the hypothesis that " . . . the more rigid a government's instructions, the more chance there is of tensions and conflicts between the professional diplomats of the permanent mission and those auxiliary members of the delegation who are not foreign ministry officials" (p. 25). Parliamentarians, he asserts, are less willing to submit to the discipline of a foreign ministry. Mr. Baehr also notes that a delegation's latitude in the decision-making process is enhanced when (1) a quick decision is required, (2) it is operating with broad instructions from its government, (3) the government asks the delegation for its opinion.

In examining the national delegation in "regional group" meetings (the Netherlands is in the Western group), Baehr states that "there are few definite rules as to which countries should participate in meetings of the Western group at the committee level" (p. 37). Meetings vary in their degree of formality, and informal meetings are sometimes used to exclude certain states when dealing with certain issues. Subjects of regional group meetings include "1. Requests for support in elections, . . . 2. Appeals for support or rejection of certain proposals, . . . 3. Exchange of information, . . . " (pp. 43–53). He also suggests that informal group meetings occur at the inter-regional level and are important in the life or death of any proposed resolution.

In an appendix Mr. Baehr offers a well-drafted "Proposal for a Study of Parliamentarians in Governmental Delegations to International Organizations."

This study is a worthwhile addition to the literature of the field. But it has problems. The use of the experience of the Netherlands delegation in 1968, as an example of the inner workings of UN delegations in general has its limits, as Mr. Baehr is well aware. The majority of UN delegations are, of course, not from Europe. But perhaps we should be satisfied to have a peek inside any fairly representative national delegation.

Early in the study Mr. Baehr acknowledges his gratitude to the Foreign Ministry of the Netherlands which allowed him access to the meetings of the delegation and "confidential files." "It was understood that no quotation or substantive information from discussions within the delegation would be made public. At no point was the author requested by official sources to alter the text in any way" (p. 9). The lack of official intrusion is easy to understand, and it is not a reflection on the openness or tolerance of the Foreign Ministry. Rather it is indicative of the almost total lack of detail and insight which this observer feels is necessary in a quasi-case study of this type.

Finally, as an examination of the politics in UN decision making, the volume really contains little that is not already known via Hovet, Keohane, Hadwen and Kaufmann, Alger, and Kay, all of whom Mr. Baehr quite properly utilizes.

Specialists interested in the functioning of the United Nations and its decision-making process may find this a useful volume to consult, but they should not expect to be surprised.

JOHN O. LINDELL
Hartwick College

Problems of War and Peace: A Critical Analysis of Bourgeois Theories. Translated from the Russian by Bryan Bean. (Moscow: Progress Publishers, 1972. Pp. 390. $3.00.)

This book is the nearest approach to a serious Soviet academic work on international relations so far to have appeared in translation in the West. The authors, who are mainly members of the staff of the Institute of Philosophy in the Academy of Sciences, set out "to examine some of the most widespread sociological and socio-political theories of war and peace in the capitalist west, and confront them with the scientific Marxist standpoint" (p. 9).

The book falls into three parts. The first is devoted to "The Quest for Peace in the Social Thought of the Past," and reviews the theories of peace of the "utopian socialists," particularly Winstanley and Babeuf, and the "bourgeois humanists," from Erasmus to Kant.

The second part, on "The Origins and Essence of War" upholds the class analysis of the causes of war against theories which see its origins in human nature, national sovereignty, international "anarchy," Malthusian demographic processes,

or the psychological factors adduced by Freud or Fromm. There is a strong defence of the view that nuclear weapons have not altered the relationship between war and politics. There is also a defence of the doctrine of just war against pacifist approaches, including that of Johan Galtung.

The third part, entitled "Against the Ideology of Militarism and Reaction," treats a series of standard Western approaches to international relations—"power politics," the theory of games, "ideological disarmament," bargaining theory, arms control theory"—as essentially justifications of Western imperialism. There are a few kind words about "progressive" Western scientists and intellectuals, and about the Pugwash movement.

What this book reveals is that Soviet specialists in international relations are aware of the main body of Western academic literature about international relations, and feel a need to come to grips with it. There is in fact a real need for a critique of this Western literature that would bring out, as a Marxist analysis best can, the ideological functions it sometimes serves. Unfortunately, this work does not provide a searching and probing critique but has all the characteristics that are familiar in less academically pretentious Soviet writing about international affairs. It is self-righteous and polemical; it contains no breath of self-criticism or of questioning of Soviet policy; and it relies on name-calling, ranting, and smearing, in place of argument.

As an attempt to work out a Marxist theory of international relations this book fails entirely. The core of this theory is that international conflict is essentially class conflict, and will not survive the coming to power of the working class in socialist states. The principal difficulty for this theory is, of course, the history so far of relations among the socialist states, particularly between the Soviet Union and China. This subject, however, is not even mentioned, let alone discussed. Intellectual advance can take place only by recognising difficulties and confronting them, not—as here—by pretending that they do not exist. The book, however, is worth reading as evidence of the need recognized by Soviet experts in International Relations to plot a position in relation to academic writings on this subject in the West.

HEDLEY BULL
The Australian National University, Canberra

The Migration of Workers in the United Kingdom and the European Community. By W. R. Böhning. (New York: Oxford University Press for Institute of Race Relations, London, 1972. Pp. xvi, 167. $10.25.)

This book is primarily about the migration of workers to and between the original six member-countries of the European Economic Community (EEC), and the possible movement of workers between the U.K. and the other EEC countries consequent upon Britain's accession to the Rome Treaty. It is not about the migration of workers *in* the United Kingdom, and to that extent the title is perhaps misleading.

As the author, an economist, rightly notes, most discussion of immigration in Britain has centered on the presence of black immigrants from the "New Commonwealth"; consequently consideration of the impact of EEC membership on the movement of labor has largely gone by default. This obsession with black immigration has led to many misapprehensions concerning the EEC system; and, indeed, Böhning demonstrates that government departments concerned with migration policies have not been free from very mistaken notions.

Böhning aims to correct this situation by providing information on the EEC migration policies and experiences. Thus he takes us through the often labyrinthine EEC regulations on the movement of labor. This survey is followed by a detailed discussion of the experiences of migration of each of the original six member countries. Almost as a digression, Böhning constructs an exceedingly interesting model of migration "from low wage to post-industrial countries with a liberal capitalist structure" (chapter 4).

In the second part of the book he postulates the probable future developments, country by country, in terms of labor supply and demand. Finally, in Part Three, Böhning examines the impact of Britain's accession: the probable movement across the channel between Britain and the Continent; the "problem," as he puts it, of U.K. nationality, with reference to Britain's black immigrants.

This is a useful book. Into a mere 167 pages the author manages to pack a wealth of information. It is written dispassionately, from the perspective of a "convinced European." While Böhning claims to be writing neither as a pro-Marketeer nor an anti-Marketeer (p. 7), it is clear that he sees the EEC as at least one stage on the road to the "European ideal." At the same time, the essentially exploitative nature of the EEC labor system comes out very clearly. Similarly, the discriminatory treatment accorded Europe's—and particularly Britain's—black immigrants, as exceptions to free movement, is well documented. Thus Böhning's optimism does not quite square with the record.

Although I found the theoretical model the most interesting part, I do not mean to depreciate the value of the whole work. The text on the EEC regulations is exceedingly complex: this probably reflects the complexity of the material *per se*, though possibly some prior knowledge is assumed. An index, too, would have been a useful addition.

Given its subject matter, this book is bound to

hold most immediate appeal for English readers. As a compact but well-documented summary work on EEC labor policies it might well find, and deserve, a wider audience.

DENNIS BROOKS
University of Warwick, England

Power and Equilibrium in the 1970s. By Alastair Buchan. (New York: Praeger Publishers, 1973. Pp. 116. $6.00.)

Armchair speculations about the future have a notoriously short life. Simple extrapolation of what seem to be the most important or salient current trends is a virtual guarantee of quick obsolescence. The first "surprise" significant enough to challenge the conventional wisdom about the shape of the near future cannot be long in coming: the sudden emergence of the oil crisis, especially in terms of the conditions of supply rather than the adequacy of demand, is merely the latest case in point.

Nevertheless, the demand for such speculations never seems to diminish, and in fact usually accelerates sharply in periods when the old order seems to be crumbling and the form of a new order remains hazy and obscure. Thus the onset of the Nixon presidency, the relative decline of American involvement in Vietnam, the SALT agreement, the attempts to restructure the international economic system, a perceived rise in the power and influence of China and Japan, the apparent détente between the United States and the Soviet Union, and the domestic political and social difficulties of the industrial states, among other things, seemed to call for a new image of the international system that would prevail in the 1970s and 1980s. In response to these developments, Professor Alastair Buchan of Oxford University was asked to deliver the Russell C. Leffingwell Lectures for the Council on Foreign Relations, subsequently published as *Power and Equilibrium in the 1970s.*

Mr. Buchan's views are hardly novel, for they reflect the consensus that seemed to be emerging in the early 1970s on what the system would look like in 1980. He notes that "there are now five major centers of military, political or economic influence in the industrial world" (p. 48). This implies that "a multiple power balance is re-emerging" (p. 42), although it will be a balance more easily understood by the metaphor of the mobile than the traditional scales. While Mr. Buchan does make some effort to question the utility of the idea of the balance in the light of current trends, his analysis is too sketchy to be useful. Thus he never explains exactly what he means by a multiple balance of power: what conditions it rests on (beyond the mere existence of five centers of influence), what behavior it implies, and what norms must be accepted before balancing becomes something more than a metaphor that obscures reality.

At any rate, starting from the premise of a plural world, or what others have described just as loosely as a "pentagonal" world, Mr. Buchan briefly touches upon various transformations in strategic nuclear power, conventional military power, political influence, and economic strength. Most of his analysis concentrates on the level of political or psychological influence, for he believes that it is this level of interaction that will most sharply differentiate the 1970s from the 1960s (pp. 20f.). The underlying premise justifying this contention is a continuing decline in American influence, status, and will.

Whatever the accuracy of this judgment, it permits Mr. Buchan to treat a number of crucial strategic and economic issues in an extraordinarily superficial fashion. Economic issues are, in fact, almost ignored, despite the fact that Mr. Buchan believes that strategic and conventional force probably will not be used. In this circumstance, surely more attention must be paid to economic and social interactions.

Many of Mr. Buchan's other views are also debatable. For example, he contends, "I find it hard to see how any level of Soviet strategic power, except of a size that would bankrupt the country, can seriously threaten American security, and this view seems to have been ratified by the SALT treaty . . . " (p. 14). There are many strategic analysts, not all Pentagon flacks, who would disagree. Mr. Buchan is also very optimistic about controlling nuclear proliferation, even though his own argument about a sharp decline in American influence surely ought to limit his optimism. In any event, analysis of the problem rather than assertions of optimism might have been appropriate. And Mr. Buchan's belief that if Japan acquires nuclear weapons "Japan herself would lose much of the influence and respect she is slowly regaining in the small Asian states" (p. 103) can serve as a final illustration of judgments that are either dubious or unargued. After all, Great Britain, France, and China have become nuclear powers without losing influence and respect—at least not from the nuclear decision *per se.*

Perhaps it is the task itself, rather than any shortcomings in Mr. Buchan's imagination and insight, that is primarily responsible for the inadequacies of this book. The form of analysis that the author has relied upon—very traditional speculations about emerging political configurations—simply cannot deal with a world increasingly dominated by very complex interdependencies between different levels and forms of interaction between states, individuals and groups.

Description and extrapolation of a few political trends is clearly insufficient; at the very least, a more substantial effort to look at where we want the international system to be going (and what we can do to get there) might have been helpful. The failures in Mr. Buchan's approach should also alert the rest of us to the need for new and more imaginative approaches to the problem of political forecasting.

ROBERT L. ROTHSTEIN
Woodrow Wilson International Center for Scholars

A Secret War: Americans in China, 1944–1945. By Oliver J. Caldwell. (Carbondale: Southern Illinois University Press, 1972. Pp. 218. $5.95.)

Dragon by the Tail. By John Paton Davies, Jr. (New York: W. W. Norton & Co., 1972. Pp. 448. $10.00.)

General Stilwell in China, 1942–1944: The Full Story. By Chin-tung Liang. (Jamaica, New York: St. John's University Press, 1972. Pp. 321. $6.95.)

Sino-American Relations, 1949–1971. Edited by Roderick MacFarquhar. (New York: Praeger Publishers, 1972. Pp. 267. $2.95.)

Americans and Chinese Communists, 1927–1945: A Persuading Encounter. By Kenneth E. Shewmaker. (Ithaca, New York: Cornell University Press, 1971. Pp. 387. $10.00.)

Partly by coincidence and partly by design, a spate of books on the interaction between America and China has followed President Richard M. Nixon's historic trip to Peking. This latest group includes two scholarly histories (Liang and Shewmaker), two personal reminiscences (Caldwell and Davies), and one documentary textbook with interpretative essays (MacFarquhar). Their utility for political scientists is twofold. First, except for the last named, they enhance our historical knowledge by offering heretofore unavailable information. Second, they provide a wealth of data that may be fruitfully recast according to different models of political analysis, whether perceptual, organizational, or bureaucratic, breaking through the convenient but confining monolithic mold of the national state as actor.

For unique source material Professor Liang's volume is the most valuable because it draws extensively on the personal archives of Chiang Kai-shek and other Nationalist materials previously published in Chinese. An explicit rebuttal of Barbara Tuchman's *Stilwell and the American Experience In China, 1911–1945* (Macmillan, 1970), it presents a Chinese view of events too often seen from an exclusively American perspective. By comparison with Davies's acerbic memoir of the same period, Liang, like the White Rabbit, takes one through the looking-glass of politics where praise and blame are precisely reversed.

Unfortunately Professor Liang's study has limited reliability as a historical account. Its inaccurate use of English-language sources vitiates the validity of voluminous footnotes which seemingly document every statement. In a negative characterization of Stilwell, the author claims, "When Stilwell was in France and saw many of his classmates ranking above him, he nicknamed them the Peanut Club (see Tuchman, p. 53)" (Liang, p. 28, footnote 24). However the original reference specifically notes this "was Stilwell's designation for millionaires, politicians, and social darlings who have been given commissions of Major and up and soft jobs," with no implication of "classmates" being the category from which these were drawn. Similarly Liang asserts that Stilwell "regarded Chang Tsung-chang as China's best strategist" (Liang, p. 28, footnote 27), but the original Tuchman citation (p. 118) obviously employs sarcasm in alluding to Chang as "the greatest Chinese master of 'the strategic retreat' [which] was to be a major source of Stilwell's own future frustration."

More substantively serious is Professor Liang's paraphrase of U.S. foreign service officer reports. A lengthy summary of dispatches purports to show how the "Davies-Service [John Stewart Service] group in the headquarters of General Stilwell apparently were working under American Communist influence, whether consciously or unconsciously, in furtherance of major efforts to establish a 'Red China' " (Liang, p. 231). Thus Davies's lengthy analysis of June 24, 1943 (incorrectly given as July 24) allegedly claimed the Chinese Communists "are agrarian reformers rather than Communists" (Liang, p. 199). The original report, however, only asserted that "*Foreign observers* who have recently visited the Communist area agree that the Communist regime *in present policy* is far removed from orthodox Communism . . . that it is described less accurately as communist than as agrarian democratic" (*Foreign Relations of the United States, 1943: China*, Washington, D.C., 1957, p. 260, italics added). The shift in attribution and nuance is important in assessing Davies's judgment and reliability.

In parallel fashion, Professor Liang omits Davies's warnings of Soviet influence in the CCP, such as the observation "It is suspected that the political leaders of the Party retain their pro-Russian orientation and that they are, notwithstanding the dissolution of the Comintern, likely to be susceptible to Moscow direction" (*Foreign Relations*, p. 260). The omission grossly distorts his subsequent allusion to "the trend toward na-

tionalism . . . among the troops and guerrillas" (Liang, p. 199). Whereas Davies recommended an American "*readiness to support* a new coalition" regime Professor Liang paraphrases this as "*prepare to set up* a coalition government" (Liang, p. 201; *Foreign Relations, op. cit.*, p. 399; italics added). He similarly summarizes Service's careful itemization of suggested restrictions in aid as bluntly urging "support to Chiang's government be discontinued" (Liang, p. 203; John Stewart Service, *The Amerasia Papers: Some Problems in the History of U.S.-China Relations*, Berkeley, Center for Chinese Studies, 1971, p. 203). The cumulative effect of these errors is to caricature the foreign service reports as written by fools or knaves, giving them a leftward bias as compared with a full reading of the original dispatches.

Presumably Professor Liang is more careful with Chiang Kai-shek's memoranda and cables. In any event since these clearly are translated, their tenor rather than their specific wording provides reliable evidence about how a dependent power views and manipulates a donor relationship. The vantage-point of Chungking shows American and British behavior in quite a different light from that of the White House and Whitehall, although too much attention is given to purely military matters without even minimally useful maps for the armchair strategist. The book sounds a series of complaints, some justified and many plausible, which anticipate the later litany of accusations leveled by Peking against Moscow's handling of aid programs and alliance issues. Indeed, to the extent that the Russian and American roles were analogous, additional insight into the Sino-Soviet dispute may be gained from Professor Liang's account of Chinese perceptions and reactions.

Despite their obvious differences, Liang and Davies complement one another in their detailed recounting of the organizational and bureaucratic battles fought within and between Chungking and Washington. The Chinese exploitation of American disputes, widely reported at the time through the Chennault-Stilwell feud, is now susceptible to cross-verification and amplification from opposite sources. Davies's diary and his family letters add a human touch of wit and urbanity to the internecine conflicts but do not conceal the mixed coalitions of interest which joined Americans and Chinese in transnational but opposing camps that fought within both governments.

Particularly titillating in this regard is the "secret war" of Professor Caldwell, a self-defined "triple agent . . . an American army officer assigned to OSS, which had detailed me to Tai Li's Secret Military Police [where] . . . I worked with the secret societies . . . dedicated to overthrowing Chiang Kai-shek and Tai Li" (p. 77). Despite the purple prose which overdramatizes and exaggerates his activity and attitudes, the author nonetheless provides a glimpse of how Tai Li maneuvered in the conflict between the OSS and the U.S. Navy which won a secondary role in supporting Chinese Nationalist covert operations far from the Pacific Ocean. Caldwell is long on assertion and short on documentation, but he gives a sharp counter to the claims of Rear Admiral Milton E. Miles who was personally assigned to Tai Li (Milton E. Miles, *A Different Kind of War*, Doubleday, 1967).

Caldwell reflects with considerable self-criticism on how the preconceptions of Americans in China distorted perception and policy, a theme which looms large in the Davies and Shewmaker volumes. No less relevant, however, is the implication of his narrative for Chinese perceptions, both Nationalist and Communist. The ability to manipulate interagency rivalries, endemic in interaction among governments, was enhanced by the unique relationship between the Chiang regime and the U.S. Navy. Dating back to the 'twenties, this relationship crystallized through the Tai Li operation into an affiliation which was to provide leverage for Chiang down through the 'fifties, as reflected in the positions of Admiral Arthur W. Radford during the crises of the offshore islands during 1954–55. The obverse relationship necessarily applied for the Chinese Communists because of the way in which SACO (Sino-American Cooperative Organization) provided men, money, and munitions that resulted in anti-Communist rather than anti-Japanese forces. Whatever hands-off policy may have been designed in Washington with respect to the nascent civil war, the covert cooperation among Chinese Nationalist, U.S. Navy, and Japanese elements under SACO aegis in 1945 contributed to Yenan's assumptions that American policy was basically antithetical to CCP interests and committed to the survival of Chiang's government.

Davies and Shewmaker are nicely paired in their explicit examination of how perceptions determine postures and ultimately policy, a framework within which Liang and Caldwell are also useful. Davies is the more far-ranging and less systematic, embracing "American, British, Japanese, and Russian encounters with China and one another." Damned by Professor Liang as a pro-Communist conspirator, Davies wears his martyrdom as a victim of Senator Joseph McCarthy lightly. He combines an incisive pen, a brilliant intellect, and many years in China as a missionary child and foreign service officer, to produce the most detailed, dispassionate, and damaging account of bureaucratic infighting, Chinese and American, yet to come from a first-hand participant.

Professor Liang's attribution of "agrarian reformer" to Davies's description of the CCP highlights a central point in the polemic between Left and Right (within which the Center was crushed). Ironically, Chou En-lai took personal exception to the term when speaking with Ambassador George Marshall in December 1945, "You must realize we are not that; we are Communists and Marxists with all that means now and for the future" (John F. Melby, *The Mandate of Heaven*, Anchor, 1971, p. 76). Professor Shewmaker has written the definitive analysis of this controversy over who wrote what about China and why, submitting the writings of Americans and Europeans (in English) to the most systematic and scholarly analysis possible, short of using quantitative techniques. He destroys completely the accusations of conspiracy and concealment leveled against the majority of journalists and commentators who addressed the subject of China and communism during the years 1927–1945. Instead he indicts the accusers as well as the accused for failing to understand and articulate the complex interaction of values, personality, and politics which plague American communication on China with a resultant distortion of reality that has proved hazardous, and at times disastrous, for both audience and communicator.

Professor Shewmaker has pursued his subject with diligence and discipline. His footnotes reveal a wide range of sources, including correspondence and interviews with most of the persons under examination. He hews to his line, letting the chips fall where they may on radical, liberal, conservative, and reactionary alike. He focuses on public communicators whose output helped to shape public images and therefore excludes private official writings. But in so doing he gives a useful template to lay over the dispatches of Davies, Service, and others for comparison with those of their journalistic cohorts.

What Professor Shewmaker calls "the American perspective" consists of three fundamental determinants, "a bias toward social egalitarianism . . . a concern for promoting the welfare of the common man, especially through the elimination of hardship and poverty . . . and a strong preference for democracy . . . different to each individual but cherished by all as an ideal" (pp. 299–301). This explains both the near-universal negative reaction to the Chinese Nationalists as well as the more varied but basically positive reaction to the Chinese Communists among two dozen American writers and correspondents of extremely different political viewpoint.

The coverage is encyclopedic—first chronological, then topical, and finally retrospectively analytical. Shewmaker has relied on an impressive array of scholarly works which emerged subsequent to the writings under analysis, finding they generally support the impressions given by wartime reporting. He also provides sensitive portraits, warts and all, of such stalwart but often stereotyped journalists as Edgar Snow, Anna Louise Strong, and Agnes Smedley, whose work provided a unique if biased view of what was to become the most important revolutionary movement in Asia.

These books are not merely for understanding the past. Since 1971 American writing on China has produced a surge of "love literature" after two decades of "hate literature," both based more strongly on emotional perception than on objective reality. The preconceptions and misperceptions chronicled by Liang, Davies, Caldwell, and Shewmaker, are echoed by more recent travellers to the Great Wall. An excellent bridge between the two periods is the MacFarquhar reader with introductory essays by Morton Halperin, A. M. Halpern, and Donald W. Klein on foreign policy in Washington and Peking *circa* 1968–71. The volume excerpts a wide span of Chinese and American documents which trace official perceptions and policies over two decades of confrontation. Although this treatment omits the actions, covert and overt, of cold and hot war, it provides valuable teaching material for examining how public rhetoric and political rationalization all too often become reality for domestic and foreign audiences.

ALLEN S. WHITING
The University of Michigan

Public Papers of the Secretaries-General of the United Nations. Edited by Andrew W. Cordier and Wilder Foote. (New York: Columbia University Press; **Vol. 1: Trygve Lie, 1946–1953.** [1969, Pp. xiv, 535. $15.00.], **Vol. 2: Dag Hammarskjöld, 1953–1956.** [1972, Pp. xiv, 716. $22.50.], **Vol. 3: Dag Hammarskjöld, 1956–1957.** [1973, Pp. xv, 729. $22.50.])

These are remarkable volumes. One must not be misled by the title "Public Papers"; this is no mere collection of documents. It is an unusual type of biography, especially of Dag Hammarskjöld. It is also a history of the United Nations—not to be taken alone but as an absolutely essential supplement to more conventional studies of the organization. The work could not have been done, as it has been fascinatingly done, by anyone who was not an intimate participant in the activity of the UN during its early years. They call themselves "editors," but they exceed the limits of what one might anticipate in confronting the editing of "public papers." Andrew Cordier was executive assistant to the secretary-general, with the rank of under-secretary, from the beginning of the United Nations until 1962. Wilder Foote

was assistant to the secretary of state at San Francisco in 1945, and from the early days of the United Nations until December 1960 was its director of press and publications and spokesman to the press for Secretaries-General Trygve Lie and Dag Hammarskjöld. Both of the "editors" subsequently were at the School of International Affairs at Columbia University, Cordier serving as dean of the school and later as president of the university. The "public papers" or documents included in these volumes seem to me like photographs one finds in biographies, but the story, the history, is in the voluminous notes in which the editors not only give the setting for the documents, but supply hitherto unpublished material which reveals what often happened behind the scenes. Any twentieth-century practitioner of diplomacy (which includes multilateral or "parliamentary diplomacy" in the United Nations) will find here a record of remarkable skills in negotiation, in press relations, and in human relations. In picking up one of these volumes and glancing at the Table of Contents, a reader will not be aware of how many pages of smaller type contain the intimate record of what occurred and why. These notes are scattered among the documents and they supplement—sometimes, indeed, a bit repetitiously—the editors' formal "Note on the Series," and their "Introduction" to each volume.

The editors have been skillful in their selection of the papers. Many of them are reprints of United Nations official documents, or of essential parts thereof. Here one counts the secretary-general's introduction to his annual reports which were originated and given their cast by Trygve Lie. Here also are public speeches or interventions in UN debates in the General Assembly or in the Councils or in some of the specialized agencies. Of course, such documents had to be included, but they are not necessarily so revealing of the man since, as noted by the editors and by Lie and Hammarskjöld, the Secretary-General obviously had to have the help of his staff in putting together these statements. Although detailed authorship is not generally revealed, one can be confident that Andrew Cordier and Wilder Foote both were often involved in either drafts or final texts. I think it can be said also that on technical matters, for example legal points, Lie depended on Abraham Feller (cf. Vol. 1, p. 485), and Hammarskjöld on Oscar Schacter, both international lawyers of skill and standing. Some of the speeches, however, are illuminating glimpses of the inner man; prime examples are Hammarskjöld's contribution to Edward R. Murrow's "This I Believe" (Vol. 2, p. 194) and his inaugural address in taking his seat as a Member of the Swedish Academy (Vol. 2, p. 399).

The editors had the advantage of Trygve Lie's own memoirs (*In the Cause of Peace*), and they cite other writings, especially about Hammarskjöld, including Brian Urquart's full length biography. Notably in Volume 3, which covers the Middle East and Hungarian crises of 1956–57, they make good use of published memoirs of Eden, Eisenhower, Nutting, Truman, and General Burns. They acknowledge their debt to archival material in the UN Dag Hammarskjöld library and in the Columbia School of International Affairs.

Volume I covers the Secretary-Generalship of Trygve Lie from 1946 to 1953; Volumes 2 and 3 are the first two of four volumes dealing with Hammarskjöld from 1953 until his death in 1961. Volume 3 carries the story up through 1957. Although Lie is covered in one volume, his role is by no means played down, and indeed, the first secretary-general of the UN emerges as a man of stature. In his term he did not face developments as dramatic as those which his successor confronted. He did not have Hammarskjöld's flair and was not, as Hammarskjöld mentioned he had been styled by a New York newspaper, an "unashamed intellectual." Lie's memoirs in style and philosophy, bear no resemblance to Hammarskjöld's posthumously published *Markings*, (cf. Vol. 2, pp. 20–21). The editors' introductions give sketches of the two men and there is no need to elaborate comparisons here, although I did have the privilege of knowing and working with both of them, more with Lie than with Hammarskjöld, since I represented the United States at the UN in the period 1948–1953. Especially revealing, I find, are the abundant transcripts of press conferences which are not only illuminating commentaries on historical episodes but are also portraits from life of the two secretaries-general, both of whom handled the press well.

Lie's term did include the Korean War and it was his firm stand on the need for United Nations action that resulted in the Soviet Union's refusal to recognize him in his last years as secretary-general when the General Assembly (with dubious legality) extended his term for an additional three years. Before that, Lie was diligent in trying to minimize the cold war, and the cold warriors in the State Department at times felt he leaned too far in the direction of Moscow. In the spring of 1950, before the Korean War, he began "a one man campaign" for a Twenty-Year Program for Peace, which at first had a more friendly reception in Moscow than in the Western capitals, but it came to nothing. (See Vol. I, pp. 268 ff. and 376.) During his last six months he suffered from the McCarthy-McCarran attacks on the UN and perhaps yielded too much to pressure from Washington. It remained for Hammarskjöld to complete

staff regulations to cover the role of the secretary-general in determining criteria for "loyalty." (See Vol. I, pp. 487 ff. and Vol. II, pp. 110 ff.).

Since Hammarskjöld was a more dramatic personality than Lie, Volumes 2 and 3 are more exciting. In many respects they contribute new information based on the inside knowledge of the editors. One senses this at once in Volume 2 at pages 23 to 28 where the editors tell how Hammarskjöld came to be selected and elected. The account of his mission to Peking in January 1954 to secure the release of the American airmen who were held prisoner by the Chinese contains insights not previously available, and the editors properly stress the extraordinarily skillful diplomacy which eventually led to the success of his efforts (Vol. 2, pp. 415 ff. and 572), although Krishna Mennon of India tried to steal the credit (*cf.* pp. 491 and 580).

In Volume 3, the editors deal with the Suez and Hungarian crises which simultaneously imposed on the secretary-general and his advisers an almost inhuman burden. The editors properly separate the material on the two cases while pointing out to the reader that the events were contemporaneous. Here is authoritative description of the plot of Israel, France, and England in 1956 to eliminate Nasser by their combined illegal attack on Suez. The continued recalcitrance of Israel in the face of resolutions by the Security Council and the efforts of the secretary-general stand out, as does the support which President Eisenhower and Ambassador Henry Cabot Lodge gave to the United Nations despite the propaganda pressures and lobbying on behalf of Israel. According to Michael Guhin (*John Foster Dulles, a Statesman and his Times*, p. 292), Lodge used his close relations with Eisenhower to ignore instructions from the State Department. As this review is written in the midst of new conflict between Israel and the Arabs, there is much which reveals parallels, as in the Saudi-Arabian oil boycott against Britain and France (p. 340). Here, too, in intimate detail, is the story of the creation of UNEF—the United Nations Emergency Force, set up by Hammarskjöld almost overnight on novel principles. The editors print Hammarskjöld's "private aide-mémoire" of August 5, 1957 giving his own interpretation of the agreement with Egypt on the stationing of UNEF on Egyptian territory, It was this document which Ernest Gross made public in 1967 after the Six Day War in connection with Secretary-General U Thant's controversial withdrawal of the Force. (See pp. 369–382.)

It is not possible within the scope of this review to refer to the details of the Hungarian crisis, to the remarkable success of the UN's clearing of the Canal, or to the personalities, including the Editors and especially Ralph Bunche, on whom Hammarskjöld relied. Hammarskjöld was marvelously inventive, wise and skillful, but as the editors note in connection with the Hungarian crisis, he could not pass miracles (p. 415). It is also impossible here to do justice to important discussions of UN administrative practices and to such activities as those of ECOSOC and the Atomic Energy Agency.

PHILIP C. JESSUP

Norfolk, Conn.

International Disputes: The Legal Aspects. Report of a Study Group of the David Davies Memorial Institute of International Studies. (London: Europa Publications, 1972. Pp. 325. $11.95.)

If war is too important a matter to be left to the generals, then, on the evidence of this volume, international disputes are too important a matter to be left to the lawyers. While not all international lawyers are as hidebound in their views as those in this volume it is disquieting that in their discussion of "the traditional means of settlement and how they might be improved" (p. x) the contributors have managed to be both timorous and unrealistic. In his introduction Sir Francis Vallet lets the cat out of the bag (but is unheeded by his colleagues) by arguing that "it must never be forgotten that the legal element in international disputes is by no means always predominant. Consequently, not all international disputes lend themselves to judicial or quasi-judicial settlement. Even disputes in which the legal element predominates may respond more readily and more satisfactorily to some other approach" (p. x). But this volume cannot be dismissed out of hand since its intention is serious, it represents a long-standing, if old-fashioned approach, its contributors are respected in their field, and it is part of a large project involving a parallel examination of the political aspects of international disputes.

The volume is made up of a report by a distinguished study group chaired by Sir Humphrey Waldock, together with the memoranda prepared to assist the group in its deliberations. The present report and memoranda constitute a revised version of those first published in 1966. The *déjà vu* element, however, arises not merely from their long period of gestation and revision but rather from their concentration on failed techniques and their inability to generate new and more relevant techniques.

The legal aspects of international disputes are worth discussing, but the contributors to this volume seem at no time to have addressed themselves seriously to the wider implications of their chosen topic. For example, if legal judgment is behaviorally an integral part of the political process, then reactions to it will be political and will need to be analyzed as such. Any analysis

which ignores the political and sociological functions of law, especially in conflict situations, is irrelevant. Most legal systems reflect and protect values of a past era. In the case of international law these are capitalistic, imperialistic and European values, and since values in world society have changed, then, to the extent that the legal aspects of international disputes reflect old values, they will be rejected by some of the participants. Again, international law deals with interstate relations, but many conflicts are only partially interstate in their conduct and origin. Unless the international legal aspects are seen as part of, or adapted to, the whole, they will be ignored. The world will not be made to fit the pristine exigencies of international law as interpreted by many of the contributors to this volume. Moreover, the volume almost totally ignores the techniques used for handling disputes in domestic society, despite their increasing use in the context of international disputes. Such techniques rightly stress participation and responsiveness; and it is only when the legal aspects of the disputes enhance these values that they become supportive and resolution-oriented rather than repressive and, ultimately, self-defeating or ignored. Basic considerations such as these are absent from this volume. What, then, are its principal areas of concern?

The report is essentially a description of the existing legal aspects. No international lawyer will learn anything new, although he might want to suggest it to a beginner. The presentation is old-fashioned and unimaginative besides being devastatingly platitudinous. (For example, the reader is grandly informed that "negotiation unalloyed by any third influence tends to favour the stronger against the weaker Power" [p. 36].) Four of the memoranda are prepared by H. G. Darwin: General Introduction, Negotiation, Mediation and Good Offices, Fact-finding and Commissions of Inquiry. Like the members of the study group, Mr. Darwin is politically naive and often deals in platitudes, (e.g., Section IV, p. 64 ff; p. 71, para. 42; p. 75, para. 49). He never faces squarely the implications for a legal system of its existence in a society characterized by a fundamental value dissensus. When Mr. Darwin turns to Negotiation he deliberately omits many of the most interesting questions, so it is hardly surprising that his effort is banal and formalistic (e.g., p. 78, para. 3) as well as revealing a considerable ignorance of the literature. He is a little better when he gets to Mediation and Good Offices but on Factfinding and Commissions of Inquiry he can only offer a string of examples with no analysis. Mrs. Hazel Fox's two memoranda comprise a piece on Conciliation which is a long and uninteresting exercise at definition, and one on Arbitration, which is both more realistic and a genuine survey.

Mr. Allott is assigned the ICJ. He tells us nothing new, but his views will hardly go unchallenged if he insists on arguing that international law is not a means of asserting interests or that negotiation is an "essentially primitive situation" (p. 128). Moreover, his discussion of the role of law is naive, confusing description and prescription, and he appears to be blissfully unaware of the extent to which his discussion is value-laden. For Mr. Allott "the success of the Court must to a large extent depend on the quality of its decisions . . . on the quality of its judges" (p. 139). But what of the relevance of the law and the values it incorporates? They reside in the "instinct in relation to the concretisation of values into rules of law" (p. 145) which Mr. Allott ascribes to International Court judges and international lawyers in general. No wonder parties will not accept such a farcical lottery.

Happily, the final two memoranda have more merit. Mr. Bowett contributes a sensible, if brief, analysis of the legal background to peacekeeping and to the work of the Security Council, General Assembly and Secretary-General. Likewise the survey of Settlement of Disputes in Special Fields by Messrs. McMahon and Akehurst is a balanced, realistic, and extremely useful reference survey. The organizations and subjects covered include eleven specialized agencies, five regional organizations, foreign investment and property and the GATT, International Commodity Agreements, Fishing Agreements and International River Commissions and Boundaries. These contributions are well done.

This is on balance, a very disappointing volume, but at least it causes us to reflect upon the inadequacies of this approach to the legal aspects of international disputes. Fortunately, we can turn to others for better guidance both in Britain and, more especially, in the United States.

A. J. R. GROOM
University College London

Roosevelt and World War II. By Robert A. Divine. (Baltimore: The Johns Hopkins University Press, 1969. Pp. 107. $5.95.)

During the first two decades after the Second World War, two schools of thought dominated the debate over Franklin D. Roosevelt's diplomacy. Generally, so-called "revisionists" asserted that a devious President led the United States into the European war through the Pacific backdoor and then surrendered the fruits of victory in Eastern Europe and Asia to the Soviet Union. More traditional analysts (sometimes dubbed "court historians") usually envisioned Roosevelt as a latter-day Wilsonian who was constrained by the legacies of the First World War and by isolationists but who nonetheless

gradually led Americans to intervene against Axis aggression and then, while mediating and moderating British and Russian imperial designs, to the brink of victory and establishment of the United Nations.

Divine's *Roosevelt and World War II*, four brief essays presented as the Albert Shaw Lectures in 1968, provides correctives for revisionist and traditionalist views. Divine's central thesis is that Roosevelt was neither especially devious nor naive, nor especially prescient or idealistic, but a cautious, pragmatic, and realistic politician who applied the techniques with which he had succeeded so well in domestic politics to international affairs and achieved mixed results. "Roosevelt the Isolationist" sought to avoid foreign involvements at almost any price during the 1930s, Divine asserts, primarily because he feared and hated war. His "moral embargo" during the Italo-Ethiopian war was intended less to buttress collective action than to keep Americans from the entanglements of a war trade. Divine holds that the president's fulminations against war in his 1936 Chautauqua speech were sincere, his "quarantine the aggressor" speech in 1937 only a groping for some new policy, and his eschewing any political commitment during the Munich crisis provided Hitler with one more "green light" (p. 22).

In 1939, and especially after the spring of 1940, "Roosevelt the Interventionist" grew bolder, but whether the issue was neutrality revision, destroyers for Great Britain, Lend Lease, convoys, or undeclared war in the Atlantic, he proceeded obliquely and sought to convince himself and everyone else that these actions would keep America out of war. Probably he half believed this, but as Divine writes, when Roosevelt finally concluded that entry into the European war was a matter of American national security, his earlier disclaimers about avoiding war precluded his being direct, and he could only edge toward war.

As for organizing a structure for international peace, Divine asserts that "Roosevelt the Realist" was never a true believer in an idealistic Wilsonian-style League of Nations or collective security, but preferred the concept of the "Four Policemen" who would form "a new Holy Alliance of the victors to preserve the status quo" (p. 65). In dealing with the Russians, Divine insists that "Roosevelt the Pragmatist" had few illusions, but since his primary concern was preserving the wartime alliance, he postponed crucial decisions which contained the seeds of discord. Hence, while inclined toward having the British reach a political settlement with the Russians in 1942 over the Baltic and Eastern Europe, he gave in to State Department strictures about the Atlantic Charter, which only heightened Soviet suspicions. Nor did Roosevelt deal forthrightly either with the issue of Poland's boundaries or with the question of who would constitute Poland's new government because he feared the domestic political reaction of the American Polish community. This temporizing led to the vague Yalta declaration which the Russians regarded "as a veiled American surrender of Poland to the Soviets" (p. 97). In short, Divine insists, Roosevelt's postponement tactics led only to the misunderstandings and the "cold war" he had sought to avoid.

Most of Divine's points are well taken, though it is unfair to link Roosevelt's vague Four Policemen idea—his proposal of modest means to police unprovoked aggression—with either the concept of a Holy Alliance (which aimed at a domestic and international status quo for the privileged) or Fortress America, which also had conservative sociopolitical roots. Nor is it fair to assert that "unlike Churchill, the President had little genuine concern for the fate of Poland" (p. 97). That alleged tragedy resulted as much from the intransigence of the London Poles (to which Churchill attested) and military realities as anything else. More important is the fact that Divine has not grappled with many truly significant issues. How did Roosevelt view other societies and how did this influence, for example, his attitude toward Anglo-Japanese negotiations in 1933–1935, or his own efforts to appease Germany during 1937–1940, or his negotiations with Japan in 1940–1941, or his assessments of China's political stability despite ominous reports as early as 1935? To what extent was Roosevelt in accord with, or overcome by, the anti-imperial, anti-British and anti-Soviet views of Secretary Hull and the State Department whether applied to the Far East, Eastern Europe, or even the Eastern Mediterranean? Since the publication of this book in 1969, historians and political scientists have been probing these and many other questions which must be answered before anyone can approach an ultimate judgment about Roosevelt's diplomacy.

ARNOLD A. OFFNER
Boston University

Public Diplomacy and the Behavioral Sciences. By Glen H. Fisher. (Bloomington: Indiana University Press, 1972. Pp. 180. $6.00.)

This slim book is an interesting but not entirely successful effort to bring the findings of the behavioral and social sciences to an audience of foreign affairs practitioners. Written by the Dean of the Center for Area and Country Studies of the State Department's Foreign Service Institute, it deserves our attention because of its concern with bridging the gap between behavioral and social scientists and policy officials.

Let us deal first with some of the problems of the book, and then consider the matter of transmitting behavioral and social science knowledge into practice. "Public diplomacy," posited as a major change in international affairs in recent years, concerns the interaction between public attitudes and opinions and the development and execution of foreign policies. This "public psychological dimension" has arisen as modern communications technology has led to the increased participation in foreign affairs by the public in all countries. But Fisher's use of "public diplomacy" poses the following difficulties: its meaning is not well defined; its determinants are asserted and not established; its scope is not clearly delineated; its relation to traditional diplomacy is not clarified; and critical problems of elite-mass interaction and relationships among elites are not explored.

The basic proposition of the book is that the behavioral sciences—especially sociology, cultural anthropology, social psychology, and "the new [behavioral] political science"—have "scientifically developed concepts, knowledge, analytical procedures, and data which have been well-matured and tested" (p. 10). Though large portions of these disciplines are not regarded as directly applicable to foreign affairs, Fisher believes that "with proper selection and adaptation [they] suggest an intellectual orientation which could contribute considerably to modern diplomacy and make it a more precise and professional field" (p. 10).

Fisher deals with a number of behavioral science concepts—perception, cognition, personality, culture, social structure, stratification, and role—in a cursory manner which social scientists will not find especially helpful. Further, his analysis is largely social psychological in nature, and he has attempted an excessively bold leap from the behavior of individuals and groups to the behavior of nations. In this he demonstrates again that the micro models of individual and group behavior have yet to be linked effectively with macro models of organizations, social systems, or nations.

The intended audience for the book is Fisher's colleagues in the foreign affairs community and students interested in the psychological and cultural aspects of foreign affairs. The likelihood that social scientists will find the book disappointing, therefore, is not an entirely adequate criterion for judging its merit. Its utility must be considered relative to its intended audience.

One might well ask how training in the behavioral and social sciences is better for foreign affairs professionals than the training they now receive. One might also consider how such training leads to improved performance of these professionals. These questions are not adequately dealt with in my estimation.

The examples in the book of how behavioral and social science knowledge has been used to increase understanding of particular events and developments in foreign affairs reveal two fundamental difficulties. First, the examples demonstrate clearly that the behavioral and social sciences have not developed to where they can be used by policy officials for prediction of foreign policy-related events in any complex society. A more reasonable criticism, however, is that the behavioral and social sciences have not led to the development of decision rules, including rules of thumb, which policy officials might apply in the many contingent situations they confront. In other words, knowledge of the behavioral and social sciences does not necessarily lead to an increased repertoire of behavioral strategies and tactics for policy officials in foreign affairs. What such knowledge does contribute, apparently, is an increased capability of understanding what is happening, not one of extended operational skills. But this outcome, however, merely re-raises the need for evaluating the training of policy officials in the behavioral and social sciences relative to other forms of training.

We are thus led to two observations, neither of which can be explored here. First, relatively little investment is made by the behavioral and social sciences in translating research findings into operationally useful knowledge. Analysis, not synthesis, is what the reward system supports, but synthesis is what is needed far more by the policy community. Fisher comes off rather poorly in this regard, but he cannot be faulted for the effort. Second, relatively little attention is paid by the research community to the organizational and institutional processes by which behavioral and social science knowledge is transmitted to policy officials. One institutional channel of communication is represented by the movement of people between the academic world and the policy world. Fisher, again, is an example of this process at work, having developed his current concerns in part while on leave from the Foreign Service to Tufts University. But both the conceptual and institutional problems of translating behavioral and social science knowledge into practice need substantially greater attention by the research community, as well as the policy community, if we are serious about making the results of our research useful to society.

RICHARD A. RETTIG
Ohio State University

The United States and Malaysia. By James W. Gould. (Cambridge, Mass.: Harvard University Press, 1969. Pp. 267. $8.00.)

James Gould, a former Foreign Service Officer in Southeast Asia and for several years Director of the Peace Corps Program in Malaysia, has pro-

duced the first Southeast Asian study to appear in the 28-year history of Harvard's American Foreign Policy Library series. In nine chapters Mr. Gould takes us through the ecological, anthropological, and historical settings (chaps. 1–3), politics and political integration (4 and 5), economy and society (6 and 7), and international relations, with special attention to U.S.-Malaysian relations (8 and 9). A much abbreviated "conclusion" (pp. 240–41) is followed by a six-table appendix presenting factual data on Malaysia and a brief but useful bibliographical essay suggesting further reading (pp. 247–52). The style is light and readable, and except for the occasional piece of misinformation, the material presented is factually reliable. There are problems, however.

Presumably the Foreign Policy Library is not intended for the specialist, and persons with a sound grounding in the Southeast Asian area will find little here of value. (Indeed, they may be surprised to learn on p. 209 that Carlos Garcia succeeded Diasdado Macapagal as President of the Philippines in 1965, when history records that Macapagal defeated Garcia in 1962 only to be beaten by Ferdinand Marcos in 1965.) Much of this volume should be useful and interesting to the inquiring lay reader, but several words of caution are also in order for the unsuspecting layman. First, while Mr. Gould writes with understandable pride about the accomplishments of the three states (the author includes Malaysia, Singapore, and Brunei in his definition of "Malaysia"), he paints such a rosy picture that readers may understandably come away wondering how such idyllic third-world nations have failed to gain due recognition.

Second, the study appeared dated as soon as it came out in print (1969), and five years later it often seems painfully out of touch with present-day Malaysia. A review written in 1967 or 1968 when this manuscript was apparently completed would probably have been considerably more favorable than one written today, for much has happened since publication. The author was unable to consider the Kuala Lumpur riots of May 1969, the ensuing 21-month suspension of Parliament, the 1971 retirement of Prime Minister Tunku Abdul Rahman, the long postponed elections in Eastern Malaysia, British "disengagement" east of Suez, the toothless Five-Power Defense Agreement, the openly pro-Malay New Economic Policy, the demonstrated economic successes of independent Singapore, and many other significant events that had not yet occurred at the time this manuscript was completed.

Malaysia, Singapore, and to a much less extent Brunei, are all healthy today on almost any measure applied to the developing countries, but the often romantic optimism of Mr. Gould contrasts sharply with the hardheaded realism of most politicians, businessmen, bankers, and economists in the area. Given the climate of the day, the late Minister of Interior of Malaysia may have been excessively emotional when in the midst of the 1969 riots he proclaimed on the state radio that "democracy is dead in Malaysia," but he was at least as close to the truth as was Mr. Gould when he concluded that "democracy seems fully established" (p. 240, and implicitly and explicitly throughout the book). Democracy was not, and is not, "fully established," and indeed there are many persons in and out of the three governments who think that "democracy," as Americans often view it, is not even appropriate to their situations. Malaysia and Singapore have impressive though brief histories, and they have many strengths favoring them, but the obstacles and pitfalls are so numerous that constant efforts to avert disaster are essential. Happily there are many Malaysians and Singaporians who are fully aware of this and who will not be lulled into complacency by interesting but overly optimistic studies such as this. American readers should be equally cautious.

ROBERT O. TILMAN
North Carolina State University

International Crises: Insights from Behavioral Research. By Charles F. Hermann. (New York: Free Press, 1972. Pp. 334. $12.95.)

One problem confronting those engaged in developing a scientific approach to the study of international politics has been the lack of cumulative research. A significant contribution toward remedying this situation by aiding the cumulative growth of systematic knowledge in one field of international politics, namely the study of crises, has been made by this collection of essays edited by Charles Hermann. The essays in this volume were first presented at a symposium at Princeton University in 1967, which brought together some of the major scholars actively engaged in empirical research on international crises. The resulting book presents some of the best examples of this research in a format which provides an unusual degree of coherence for a collection of research studies. A series of introductions by the editor relates each selection to the work of other researchers and includes elements of the discussion from the Princeton symposium.

The major contribution toward cumulative research from this book is found in the excellent concluding chapter by Charles Hermann and Linda Brady. They have abstracted 311 propositions from the preceding eleven chapters and have attempted to integrate them, first, into four alternative models of crisis behavior, which are, in turn, combined into one integrated framework for the analysis of international crisis.

The first model, called the "individual stress model" is based largely upon the survey in one

chapter by Thomas Milburn of relevant propositions from the social-psychological literature on stress as well as Ole Holsti's excellent application of some of these propositions to the crisis leading up to the outbreak of World War I in 1914 and to the Cuban missile crisis of 1962. The model assumes that crises threaten both national goals and the goals of individual decision makers, inducing high levels of stress. Beyond some threshhold this stress tends to disrupt the decision makers' coping mechanisms and their cognitive processes. One result is that decision makers may make inadequate analogies to past situations and repeat actions which were successful in the past, so that innovative and adaptive responses are unlikely. Thus the individual stress model often accounts for crisis escalation.

The second approach, called the "organizational response model," is reflected in Glenn Paige's comparison of decision making in the United States in response to the outbreak of the Korean War in 1950 and in response to the Cuban missile crisis of 1962, and also in Howard Lentner's analysis of perceptions of crisis held by U.S. State Department officers who have served in the Operations Center. This model predicts that crises will cause the locus of decision to move quickly to the top of a decision-making hierarchy, thereby reducing the fragmentation of decision making which often occurs in large complex organizations. These and other similar organizational characteristics may contribute to effective crisis "management."

Third, the "hostile interaction model" assumes that crises escalate through a stimulus-response pattern in which nations respond to threats and hostility eminating from others by increasingly hostile behavior on their own part. Thus actors may readily become "locked in" to a conflict spiral. This approach is reflected in this book in the study by Dina Zinnes, Joseph Zinnes, and Robert McClure of the escalation of the 1914 crisis in central Europe, and in Charles McClelland's analysis of uncertainty in interactions in the continuing series of crises over Berlin and the Taiwan Straits.

Finally, a "cost calculation model" attempts to apply assumptions of "rational" choice by actors engaged in a bargaining situation to the analysis of crisis. Glenn Snyder's comprehensive chapter on crisis bargaining suggests that decision makers will respond to those elements in a crisis situation which threaten essential national goals in order to protect those goals at the expense of nonsurvival goals. In order to do this, they will seek to negotiate a solution, including accommodation if necessary, and to refrain from actions which might reduce the likelihood of a settlement. On the other hand, if survival goals are not initially threatened, hostilities are likely to increase as each party tries to coerce the other into submission while avoiding disasters if survival goals do become threatened.

Hermann and Brady have tentatively merged these four "models" into one general framework, which provides a useful first step towards an integrated approach to the analysis of international crises. Their synthesis makes use of "branching points" or "switches" which determine which of several paths a crisis will be likely to follow. Two primary factors affect these switching points. First, the degree of threat to survival goals greatly affects the risks that actors will be willing to take. Second, the presence of disruptive stress greatly affects how the decision makers will respond, especially whether they will tend to get "locked in" to the escalation spiral or be able to bargain rationally for solutions which do not endanger their survival goals. At this point in the development of this framework, however, the specifics of these branching conditions and the long-range processes which are likely to result have not been specified sufficiently. For example, the branch in the framework (diagrammed on p. 302) in which survival goals are not threatened and in which decision makers do experience disruptive stress leads to the outcome that one actor initiates hostile behavior. Yet this is certainly not a complete analysis of the processes occurring in such a crisis, as the initiation of hostile behavior may only mark the beginning of the crisis, eventually producing conditions in which the survival goals of one or more actors do in fact become threatened. In short, interrelations among alternative branches must be specified with greater precision, and the dynamics of crisis from onset to outcome must be included in the framework before the synthesized model has much power in explaining and predicting the etymology of international crises. Nevertheless, the Hermann and Brady framework is a definite step in the right direction. In combination with the extensive propositional inventory, where interrelated and contradictory propositions may all be found, the foundations have been laid for improved communication among those exploring empirically the phenomenon of crisis.

The individual studies within this volume also frequently contribute to this overall effort. Nevertheless, there are a few major limitations to some of these studies, often arising from problems of indicator validity, which may limit the cumulative value of the work reported here. For example, Charles McClelland's chapter on the crises in Berlin and the Taiwan Straits is based upon a measurement of crisis interactions in terms of increases in "uncertainty," defined in information theory terms. While this is an interesting model of crisis behavior, the operational measure of un-

certainty employed in this research is directly associated with an increase in the frequency of high conflict events. In this instance, it is not clear that much is gained from the employment of a sophisticated model of uncertainty. Another example is found in the article by Dina Zinnes, Joseph Zinnes, and Robert McClure on escalating hostility in the 1914 crisis. They employ an interesting Markov process model to analyze the effects of hostile interactions in crisis escalation and to try to refute the hypothesis that escalation is based upon misperceptions of hostility. Yet they operationalize hostility in a dichotomous fashion which makes it impossible to draw inferences about the *intensity* of hostility, upon which most of the research they have tried to refute is based. In this manner, these and other authors in this collection of essays too often claim to be testing comparable hypotheses when they are in fact not doing so, because of significantly different measures of the central concepts. This often limits the cumulative nature of the research reported in this book.

Furthermore, there is little agreement even about the definition of the central concept itself—namely, crisis. Charles Hermann, for example, views crisis from the perspective of the decision maker as a situation in which there is a high threat to central values, short time for response, and surprise. Charles McClelland focuses on crisis more from the perspective of the international system, viewing it as a situation in which the normal equilibrium of international interactions is interrupted. Glenn Snyder tries to synthesize these two perspectives by defining a crisis as occurring when there is a perceived increase in the probability of war. While these disparate definitions and operationalizations do detract from cumulative research, the authors must be congratulated for making these sufficiently explicit so that differences can be evaluated by the reader and perhaps eventually resolved.

International Crises, therefore, reflects the current state of progress in the scientific study of this important topic. It presents some of the major middle-range theories and empirical findings arrived at to date. Furthermore, through some of its limitations, it suggests some things which need to be done in order to improve the cumulative study of international crises. First, we must identify scope conditions which will determine the circumstances under which alternative models and sets of propositions will be applicable. For example, we do not yet know with any reliability under what conditions crises will subject decision makers to such stress that their cognitive processes will be adversely affected or under what alternative conditions they will be able to respond to the crisis in a relatively "rational" manner. Second, we need to develop further integrated deductive theories, with greater attention to modeling the *processes* which occur throughout crises, a point noted also by James Robinson in one of the introductory essays to this volume. Third, greater attention must be paid to variable operationalizations in order to make findings more valid and more comparable. Finally, there is a need to explore further the linkages between findings based on empirical theories about crisis and applied policy analysis; Thomas Milburn's effort in one chapter to derive a set of decision rules based on a set of empirically based propositions about crisis is an excellent example of how we might begin in this direction. This book, therefore, represents essential reading for those who are interested in undertaking further empirical research in the directions just noted in order to improve our ability to understand, to cope with, and to avoid serious international crises.

P. TERRENCE HOPMANN
University of Minnesota

American Religious Groups View Foreign Policy: Trends in Rank-and-File Opinion, 1937–1969. By Alfred O. Hero, Jr. (Durham, North Carolina: Duke University Press, 1973. Pp. 552. $9.75.)

Alfred O. Hero, Jr., of the World Peace Foundation, has for nearly two decades pursued single-mindedly the influence of religious institutions on American foreign policy. This study, the culmination of his work, is based on interviews with more than 150 religious leaders, congressmen, and policy makers, as well as the secondary analysis of survey data from Gallup, Roper, NORC, and the Survey Research Center. It traces the attitudes and shifts of thinking of Catholics, Protestants, and Jews on leading international issues from World War II to the present. It provides a gold mine of information often at odds with hunches and first impressions about dominant religious groupings. For example, Catholics, on the eve of World War II, were probably the most isolationist and least well-informed, but by the 1960s this had changed. By then Catholics were more optimistic and affirmative than the other groups on negotiating the relaxation of tensions, the admission of China to the United Nations, and nonmilitary foreign aid. Moreover, the differences were sustained at each of the educational and socioeconomic levels studied. Within Protestantism, nonwhites who, in the 1950s were less aware of international developments than whites, had by the 1960s become at least as aware; and on certain issues, such as the United Nations, negotiating with the Russians, and economic aid, they were more liberal than whites.

These and numerous other findings bring a degree of understanding and comprehension that

most of us have lacked, and this is the unquestioned merit of the book. Its methodological subtlety and sophistication and cautious use of data are further strengths. The concluding chapters on "Explanations and Interpretations" and "Toward More Effective Churches" are no less important than the heavily documented earlier sections. Indeed, one might have wished the author had been somewhat freer and more boldly analytical, especially in philosophical terms, throughout the book. On the most important issues he tends to list and enumerate the factors which might explain behavior, but rarely does he come down strongly on priorities among the factors. I have in mind his discussion of "Why Jews are Liberal," "Why the National Council of Churches' Programs in International Affairs Are More Often Judged to be Failures than Other 'Social Action' Programs," and "Why More Liberal Forms of Catholic Leadership are Acceptable for High Church Leaders but have Little Impact on Rank and File Catholics?" (Incidentally, if the latter is true, how does Hero explain his view that Catholics as a whole have moved well beyond Protestants in a more liberal internationalism?)

In reflecting on Hero's approach I am reminded of two great professors of International Relations in "the Golden Age" at the University of Chicago. Quincy Wright followed the practice of seeking multiple explanations for every phenomenon; Hans Morgenthau sought to explain by rank ordering the factors and subordinating for analytical purposes all others to the overwhelmingly most dominant one. Hero is much closer to Wright than to Morgenthau, and this largely determines what he contributes. He gives more attention to quantitative than qualitative factors. He uses standards of selection that are hard to fathom in his choice of witnesses on ethics and foreign policy. For example, he has one passing reference to Reinhold Niebuhr and six to Richard Nixon. His case studies of organizations which in religious sectors may have pointed the way are more often footnotes than serious discussions or evaluations. (For example, if Hero feels seminars and training in ethics and foreign policy are important for clergymen and churchmen, he ought to do more than lump the Council on Religion and International Affairs, whose Dr. William Loos has pioneered in this area for 15 years, with "other sophisticated groups.")

In short, Hero's book is an invaluable resource for enhancing our knowledge of what religious groups have done in international relations. If he were to write a sequel, I could wish he might begin with his valuable single page on ethics and foreign policy (p. 250) and test the extent to which anyone has used this proposition as a basis for generating sustaining interest in foreign policy among church groups. What would happen if one group at least did not begin with the aim of producing resolutions on each successive world issue but proceeded on the assumption that "no clear demarcations between right and wrong are readily apparent"? (p. 250).) My suspicion is that this could be more fruitful than inaugurating ten new programs in seminaries or bringing in experts on pedagogy, two of Hero's recommendations. But unless both Hero's prescriptions and mine are tried, we shall never know.

KENNETH W. THOMPSON
International Council on Educational Development

Soviet Russia and the Middle East. By Aaron S. Klieman. (Baltimore, Maryland: The Johns Hopkins University Press, 1970. Pp. 106. $6.50.)

This study is an essay in interpretation based solely on the literature available in English. With this restriction, Professor Klieman makes a fair attempt to identify what he considers the signposts of Soviet Middle Eastern policy—their penetration into the "revolutionary" Arab world and their tactics, i.e., exploitation of internal Arab diversity and of the Arab-Israeli conflict. What follows is a well-digested and sometimes irrelevant summary of the historical perspective of Russia in the Middle East. The weakness of this monograph, however, is in the analysis of the relationship between Soviet purpose (policy) and the dynamics of Soviet intervention. Although Russia's purpose may have had "historical" orientations, the dynamism of policy did not follow purpose. Instead, it exploited rather pragmatically the weaknesses of Arab regimes, their diversity, and the Arab-Israeli conflict.

The narrative sets out to capture "the full extent and swiftness of Soviet penetration" (p. 39). No serious attempt is made, however, to analyze the swift Soviet penetration in Egypt, which, since 1967, has provided the key issue in Soviet-Middle East relation. It was Russia who exploited Arab-Israeli animosity, thus inspiring the "war of attrition" between Egypt and Israel (March '69–August '70).

The author interprets Soviet strategy (since June, 1967) as aimed at remedying the 1967 failure via U. S. pressure on Israel to withdraw its forces from the occupied Arab territories. She pursued this goal through diplomacy bolstered by a Soviet inspired war of attrition, which served as a gentle remainder to the United States of the conflict's potential explosiveness. In terms of the U. S. response, the Soviets were successful. The Rogers Plan, which closely resembled Soviet-Arab interpretation of the "67 UN resolution, was produced in December, 1969. In two respects, however, the Russians failed: (1) The Russian-trained Egyptian army collapsed and the Israelis turned

the war of attrition against Egypt. (2) The Israelis refused to budge. In other words, Russia's "background music," the war of attrition, once again, lent legitimacy to Israeli claims to the territories. And those Israelis, who in 1967 were willing to relinquish all territories save Jerusalem, began to sing a different song.

Analyzing Soviet-Arab relations in the Middle East, Professor Klieman states:

> The Soviet-Arab relationship is thus the interplay of regional, and global conflicts and alignments with inherent contradictions. The Russians aspire to a special status in the southern tier of the Arab Middle East. The Arabs strive for unity and freedom and have been under the mistaken impression that Soviet friendship can be exploited for such ends. The Soviets, for their part, emphasize the need for integration in this relationship, while the Arabs view it as the foundation for greater independence (p. 82).

The question, however, "Will the Arabs manage to have the Soviet Union commit itself fully on their behalf?" is not analyzed within the context of Soviet-Arab dynamics. And in discussing the Soviets' ability to exploit Arab diversity, the author fails to mention three important debacles.

(1) The Soviet failure to reconcile the two wings of the Syrian Ba'th. Soviet failure to create a "popular" government Ba'th-Communist alliance between 1968–1970.
(2) The Soviet failure to strengthen the left wing of the Egyptian ASU and Sadat's elimination of ASU's Soviet clients and its left wing.
(3) The most colossal failure—the communist abortive coup in the Sudan.

The important issue, which should be analyzed, has to do with Soviet expectations and goals in the Middle East. In terms of what Russia can achieve short of paying the price of "Vietnamization" of the area, are these expectations high, middle or low? Most important, given the dynamics of the post-Krushchev era, is the collective leadership in a position to harness the forces it has unleashed in the Middle East? This question is particularly pertinent given the lack of Soviet bureaucratic consensus (civil-military and military-intelligence) concerning the opportunity versus risk in their Middle East adventure.

My overall judgment is that this monograph has failed to achieve a coherent introductory analysis of the role, the dynamics and the types of options available to the USSR in the Middle East. The important Soviet problem in the Middle East is not whether the tails of its clients will wag, but the business of defining its political purpose in the area.

AMOS PERLMUTTER

American University

International Law: Collected Papers of Hersch Lauterpacht. Volume I: General Works. Edited by E. Lauterpacht. (Cambridge: Cambridge University Press, 1970. Pp. xxiii, 539. $37.50.)

Sir Hersch Lauterpacht (1897–1960), for a long time Whewell Professor of International Law at the University of Cambridge, and in his last years judge of the International Court of Justice, was during the period 1930–60 one of the most outstanding authorities on international law. His voluminous output of monographs and articles was crowned by his elaborate revisions of the classical book on *International Law* of L. Oppenheim, the most popular introduction to international law in the whole English-speaking world since 1905.

At the time of his death, Judge Lauterpacht was engaged in the preparation of the ninth edition of Oppenheim and completed a complete revision of its general part, which fittingly forms the introductory part of the first volume of his *Collected Papers*, edited by his son, E. Lauterpacht. This introduction, for the first time printed here, is accompanied by two other general works of Judge Lauterpacht, the original English version, not hitherto available, of his lectures in French at The Hague in 1937, and the *Survey of International Law* prepared by him for the International Law Commission of the United Nations in 1948. These three pieces are uneven in character; to some extent they are repetitious and to a limited extent complementary. Being written at about ten-year intervals, they are not necessarily consistent, and as they are not arranged chronologically, the evolution of the author's views is not too easy to follow. Nevertheless, it is good to have them here together, and to be able to compare them.

Of the three pieces, the first one, representing the author's final crystallization of his most important contributions to the theory of international law, is certainly the most important. On the other hand, the Hague lectures on the *General Rules of the Law of Peace* are the most readable, as they represent Judge Lauterpacht's ebullient self, not constricted by the rigid structure, positivist approach, and formalistic style of Oppenheim or the neutral aloofness of a United Nations study.

The main impressions one gains from reading these volumes are two: Judge Lauterpacht was a great compromiser, always trying to find a middle road between two opposing points of view; he was also a great humanitarian, putting the need to protect the rights of the individual and to achieve justice close to the center of the picture. A few examples might be useful.

Lauterpacht's predilections are reflected in his definition of international law. It reads as follows:

> International law is the body of rules of conduct,

enforceable by external sanction, which confer rights and impose obligations primarily, though not exclusively, upon sovereign States and which owe their validity both to the consent of States as expressed in custom and treaties and to the fact of the existence of an international community of States and individuals. In that sense international law may be defined, more briefly (though perhaps less usefully), as the law of the international community (p. 9).

While he admits the controversiality of this definition, he includes in it elements reflecting several normally incompatible theories, trying to resolve the differences by qualifying phrases. Lauterpacht endorses the conservative position that international law is based on the consent of States, but at the same time he stresses the equal importance of an international community of both states and individuals. Out of the existence of that community he later derives the supplemental applicability of such more controversial sources of law as "the reason of the thing" and "the law of nature," and their modern equivalent, "the general principles of law" (p. 54). Admitting that the rules derived in that fashion cannot normally override rules based on express consent of states (treaties and custom), he nevertheless insists that it is against the background of these jurisprudential concepts that positive international law is interpreted (pp. 28, 52, 54, 76, 92–94, 267–68). In his characteristic fashion, he refuses to accept the rigid alternatives of "law conceived as will (whose validity is independent of its ethical content) and law conceived as reason rising sovereign over the sovereignty of States" (p. 52).

When Lauterpacht considers the controversial issue of compensation for property taken away from foreigners as part of a program of fundamental social reforms, he emphasizes the need to balance and adjust conflicting claims "by reference not only to such law as is already generally accepted but also by reference to justice and to the needs of the international community." It is not a question of either refusing compensation altogether or awarding full compensation; an international tribunal can choose "a middle course" (pp. 388–89, 524).

The other main theme of these essays is the need to reassess the role of individuals in the international community. This is not merely the question of "subjects" of international law but goes much deeper than that. In his very definition of international law, as was noted above, Lauterpacht postulates that the international community is one not only of states but also of individuals (pp. 9, 30–31). Many rules of international law are based on analogies from private law, on general principles of law first developed in municipal law. This is due not only to the judicial fiction that states are like individuals and enter into legal relations analogous to individuals, but also to the fact that states act through individuals and consist of individual legal beings (p. 34). Consequently, he disagrees with the view that individuals acting on behalf of a state can apply standards of morality less exacting than those obtaining between individuals; this is inconsistent with the basic postulate that "the essential and normal, though not exclusive, substance of all law is morality reduced to rules and principles of a sufficient degree of clarity, precision and enforceability" (pp. 46–49). He also rejects the idea that no punishment can be meted out to sovereign states, as its acceptance would mean that

> individuals, when grouped in the form of a State, acquire a degree of immunity in respect of criminal acts against the life and safety of their neighbours which they do not possess when acting in isolation; it is an immunity covering acts which, because they are collective and assisted by the almost infinite power of the modern State, are potentially of an unlimited destructiveness. . . . There cannot be much hope for international law or morality if the individual acting as the organ of the State can, in violating international law, screen himself effectively behind the impersonal, metaphysical State; and if the State as such can avoid punitive responsibility by invoking the injustice of collective punishment (pp. 391–93).

He concludes that "the true subjects of the duties imposed by international law are not the metaphysical entities of the State but individual human beings"; and that

> the principle that the rights and duties of States are but the rights and duties of man is of importance inasmuch as it lends emphasis to the idea—with which is bound up the progress of international law—that the individual human being is the ultimate unit and end of all law, national and international, and that the effective recognition and protection of "the dignity and worth of the human person" and the development of human personality is the final object of law. In a different sphere, the recognition of the inalienable rights of the individual in relation to the State signifies the recognition of a legal authority, however impersonal, superior to the State itself—a notion which is congenial to the view that international law as a legal system can be understood only by reference to the principle that the State is subordinated to the international legal order. It signifies the establishment of a direct relationship between the individual and international law similar to the manner in which within a Federation the individual benefits directly from and is subject to federal law. To that extent it constitutes an essential stage in the progression of the international community towards the goal of federal integration (pp. 148–49).

Finally, despite a few difficult paragraphs, this is not a dogmatic book, and the author's innate sense of restrained humor makes its appearance from time to time. For instance, in commenting on

the deference of courts to the executive's determination of questions of international law, Lauterpacht notes that "in other spheres the desire not to embarrass the Executive has not been a feature of judicial activity" (p. 162, n. 1). In a discussion of Jellinek's statement that international law "may properly be described as an anarchical law," Lauterpacht ventures the dry comment that "that great lawyer probably realized that anarchical law is as difficult to conceive of as immoral morality" (pp. 210–11). In a similar vein, he deflates Kaufmann's pronouncement that the eternal ideas of law and justice constitute sources of law which are super- or extralegal, which States can invoke extrajudicially and which other States are "obliged, by law, to recognize," with the remark that "some may perhaps find it difficult to understand what are extra-legal rules of law which others are legally bound to recognize" (p. 213).

It is the intention of the editor to publish in the following volumes Judge Lauterpacht's other writings, including some unpublished papers, arranging them in a systematic fashion, as if they constituted a textbook. It may be hoped that they will provide a more detailed treatment of crucial issues of international law, weaving together theory and practice in the same masterly fashion as has been done in this introductory volume.

LOUIS B. SOHN

Harvard Law School

Regional Integration: Theory and Research. Edited by Leon N. Lindberg and Stuart A. Scheingold. (Cambridge: Harvard University Press, 1971. Pp. 427. $12.50.)

This comprehensive collection of articles offers a good summary of regional integration theory, in particular of the "neo-functionalist" school inspired by Professor Ernst B. Haas.

As the editor's preface observes, regional integration is "an increasingly elusive dependent variable" (p. ix). Haas, in the lead article, finds "authority legitimacy transfer" a useful, but insufficient definition. He postulates three heuristic ideal types: the "regional state," which has a centralized supranational authority; the "regional commune," which lacks such a central authority; and "asymetrical regional overlap," a system in which national units link themselves to a variety of functional groupings, not all of them restricted to the region. With these three ideal types as guides, independent variables can be linked into "action paths," which explain the past and predict the future of a particular regional grouping.

Armed with this methodology, Haas exhorts his fellow scholars to move from case studies and reconstruct a general theory. Such a theory must account for new forces entering various stages of the process, be sensitive to the differences between national and regional integration, and broad enough to extend to integration among less-deveoped countries. New attempts at mathematical formulation, especially computer simulation, will compel students to decide relative values for hitherto independent variables, and thus break the current stasis.

Why the hurry? Haas suspects that world peace lies in achieving the third of his ideal types, the asymmetrical regional community. By contrast, new regional states, as national states writ large, will heighten the dangerous confrontation of great powers. Haas, therefore, invites his students and disciples to turn to computer simulation: "Simulation would truly stretch, if not blow, our minds and give us a purchase on the future" (p. 42).

The uncertainties of such an exercise become apparent in Hayward Alker's article, "Integration Logics." Alker demonstrates his usual technical virtuosity, but discourages extravagant expectations. Theories of the integration process, he notes, assume the constancy of a large number of critical variables. Even experienced judgments on the relative significance of independent variables lack adequate data. Nevertheless, Alker tries to illustrate how mathematical models and computer simulation, by providing "metaphorical utopias," can at least make analysts more imaginative.

No doubt. But there is the corresponding danger that, as historical reality continues to prove inhospitable to a general theory, the more tractable medium of mathematics and computer simulation may tempt more and more theorists, like the supranational bureaucrats they study, into some form of "self-encapsulation." It might be as effective, and much cheaper, to enrich current models by a sympathetic study of the great theorists of the national state, in particular Rousseau and Hegel. Greater consideration of Marxist theory about the political consequences of international capitalism might also prove rewarding.

Several other contributors propose refurbished models and theories. Leon N. Lindberg's paradigm is a lengthy catalogue of the properties and subproperties of collective decision making, along with suggestions on how to define and measure them, with voluminous refinements and qualifications drawn from the author's great knowledge of the Common Market and the bibliography of integration.

J. S. Nye produces a sensible and hopeful critique of the neofunctionalist approach, as well as a concise model of his own. Nye is particularly impressive in reformulating "spillover." Automatic integration, stealthily manipulated by a technocratic elite, has not, he notes, proved a

realistic expectation. Instead, the integration process is more likely, in time, to slow down than accelerate—especially in less-developed countries. In general, "most political decisionmakers will opt for the status quo at any level as long as the process forces or popular pressures are not strong enough to make this choice unbearable . . . " (p. 225). But integration may still move forward by "cultivated spillover"—"package deals not only on the basis of technological necessity but on the basis of political and ideological projections and political possibilities" (p. 202). Nye's formulation, by emphasizing political will and varying circumstances, is surely closer to the reality of the European Communities.

Philippe C. Schmitter, who also revises neofunctionalist theory partly on the basis of comparative studies, arrives at somewhat the same conclusions, although by a more circuitous and tendentious route.

Andrzej Korbonski's discussion of Comecon's failures—interesting, but little related to the general theory—at least illustrates the determining role of general economic and political circumstances.

In a beautifully constructed article, Donald J. Puchala discusses the possibilities and shortcomings of transactions analysis. While an excellent case is made for measuring "community formulation," "political amalgamation" seems far more elusive.

Ronald Inglehart's abstract discussion of public opinion and political leadership is unexceptionable, even if obvious and cluttered with meretricious theoretical apparatus. His case study—linking the Common Market's agricultural and constitutional crisis to the French election in 1965—propounds a familiar but unconvincing thesis.

According to Fred M. Hayward, the processes of national and regional systems are not so different, and much can be learned by relating their study. Mr. Hayward's timidity in elaborating his obvious point suggests a fundamental bias in current integration theory. It does seem time for at least this generation of theorists to shake loose from an inherited and thoroughly unscientific ideological prejudice against the nation state. The simple-minded creed that is always ready to believe nationalism wicked and transnational integration (or disintegration) good simply will not do for serious analysis of those great domestic, regional, or international issues gradually coming to a head in Western Europe.

Stuart A. Scheingold's closing article argues that students of European integration should pay more attention to the social and international consequences of the European Communities. He asks the questions which economists have asked since the 'fifties: Does the Common Market genuinely encourage trade and growth, or does it harm other regions, especially the less-developed countries? Impressed by the new socialism in the journal *Agenor*, Scheingold wants to know about integration's effects on the prospects for radical economic, social, and political reform. And, like Haas, he worries about the danger of a new European nationalism which could disrupt world politics.

Haas, of course, fears a European bloc and promotes instead his asymmetrical regional community. Such views are now fashionable, but I would suggest caution. No doubt a strong European grouping will disrupt the present pattern of world affairs. But why not? Is perpetual American military and monetary hegemony over Europe a viable arrangement, likely to promote long-range stability and prosperity? Functionalist theory opposing a European bloc risks becoming a rather sophisticated ideology for American imperialism.

In any event, Scheingold's main point is salutary. Regional integration should not be studied in isolation from the region's broad economic and political context. Is this to affirm the priority of "high" over "low" politics? Despite frequent disclaimers, this distinction has perhaps too easily lent itself to models which emphasize the separation of economics from politics, and postulate a separate logic for each sphere. Thus economic integration, based on rationally calculated greed, is meant to go forward except where interrupted by irrational addicts of grand politics. But as Mr. Nye tells us, "low politics," or the interaction of private economic and bureaucratic interests, does not offer a convincing explanation for economic integration. Instead, without political "cultivation," integration ceases. That cultivation, as I see it, springs from a broad view of national interest, a general will which includes inextricably mingled notions of economic welfare and political and economic self-determination.

Perhaps the high-low distinction should be drawn between public and private interests—or, to be more old-fashioned, between general and private wills. Such a Rousseauan formulation is doubtless alien to most people in this volume, indeed, to anyone predisposed to shrink from concepts like national interest and accustomed to explain national politics almost exclusively as a marketplace for competing private and bureaucratic interests. Such a view of politics, borrowed from the economist's market, tends to have trouble accounting for a general will, which perhaps has more than a little to do with the persisting difficulties of integration theory.

DAVID P. CALLEO
The Johns Hopkins School of Advanced International Studies

History of the Sino-Japanese War, 1937–1945. Compiled by Hsu Long-hsuen and Chang Ming-kai. (Taipei, Taiwan: Chung Wu Publishing Co., 1971. Pp. 642. $15.00.)

Historians of World War II received a windfall of rare proportions when Japanese governmental archives were made public in the wake of the International Military Tribunal for the Far East in the nineteen-forties. Since then, but especially in the decade of the 'sixties, hardly a year went by when fresh archival material was not made available. Still, there has been nothing approaching a definitive study of the Sino-Japanese War segment of World War II. Nor will there be until scholars have a far greater access to Chinese archival resources than has been the case thus far. Books published in Taiwan with titles like *History of the Sino-Japanese War, 1937–1945* arouse the hope that perhaps, at long last, a breakthrough has been achieved. These hopes are quickened when one discovers that the compilers have based their work on a hundred-volume study of the Sino-Japanese War published (in Chinese) by the Military History Bureau of the Ministry of National Defense on Taiwan.

Regrettably, however, the *History of the Sino-Japanese War, 1937–1945*, soon dashes any such hopes. It is so transparently self-serving that one cannot read more than a page without feeling a mixture of disappointment and anger. The Nationalist Chinese propagandists who issue such books would be well-advised to realize that most readers interested in plowing through 600-odd octavo-sized pages of turgidly served data on the Sino-Japanese War probably have at least a passing acquaintance with what was going on in China during those years. Unless they were charter members of the China Lobby, these readers are not likely to swallow the central thesis of this book, relentlessly hammered home, that the battlefield skills, wisdom, compassion, and inspirational leadership of the Generalissimo accounted for all that was good, beautiful, true and victorious that transpired in China in the years under discussion in this book.

The level of analysis in the book is woefully poor. The chapter on the "causes of the war," for example, is slightly less than two pages long and is nothing more than a chronology of events beginning with the "Shoguns' Revolution of 1868" and ending with the Manchurian Incident of 1931. The complex and fascinating Sian Incident in 1936, involving the kidnapping of the Generalissimo by Chang Hsueh-liang, is reduced to a single sentence. The compilers make no mention of the Communists' mediation efforts, which were instrumental in causing Chang Hsueh-liang to release his captive. Instead, we learn that Chang was simply "inspired by" the Generalissimo and thus freed him and returned with him to Nanking (whereupon, though the authors neglect to mention it, Chang was placed under arrest for the next twenty-five years).

The epic struggle between General Joseph Stilwell and the Generalissimo, with all of the rich insights it offers into the wartime styles of the Nationalists versus the Communists, is dismissed in two crisp sentences. "Regrettably, he [Stilwell] lacked a deep understanding of the Chinese Communists. On October 21, 1944, he was relieved and recalled to the United States" (p. 555). Scorched-earth tactics such as the intentional alteration of the course of the Yellow River and the burning of Changsha (both occurring in 1938) which have been the subject of speculation and criticism of the Kuomintang are not discussed. Adversaries of the Generalissimo like Marshal Li Chi-shen are too embarrassing to mention even in passing (and this in a book whose index of names reads like the Hong Kong telephone directory) because their strongly registered complaints in 1944 suggested to American diplomats in China that the Generalissimo was not effectively prosecuting the war against Japan and was in fact denying supplies and munitions to generals who were. Those who collaborated with the Japanese are uniformly dismissed as traitors, although recent studies by both Chinese and American historians argue that many insights into the dynamics of wartime behavior and Sino-Japanese relations in general can be learned if we repudiate such a one-dimensional analysis of collaborators.

The authors ignore uncomfortable facts which do not fit into the picture of the Soviet Union as a relentless foe of the Nationalists, always undermining Nationalist causes and shoring up the sagging Chinese Communist movement. There is no hint, for example, of any aid from the Soviet Union until June 1939; the aid received after that period is dismissed in one grudging sentence. The fact, as documented by Chinese and Westerners alike (for instance, F. F. Liu, *Military History of Modern China* and Arthur N. Young, *China and the Helping Hand*), is that Soviet Russian aid was very large indeed in the early months of the war. It was not until December 1938 that the Western nations gave China anything more than sympathy; by that time the Soviet Union had extended $100 million of credits in the form of munitions, planes, and supplies and had despatched a group of advisers and volunteer pilots who anticipated Chennault's Flying Tigers by three years. Prior to 1940, the total Russian credits authorized to China were more than three times those provided by the Western democracies.

The Japanese are treated with such a heavy hand that the effect becomes comic: "On No-

vember 3 and 22, 1938, the Japanese prime minister issued two absurd statements" (p. 34). In February, 1938, Japanese Prime Minister Konoye issued his ridiculous statement . . . " (p. 35). One might at least expect that the names of the Japanese would be rendered in some recognizable form, but it is unusual for the family name to be given with accuracy and a cause for rejoicing to find both surname and given name accurately rendered. The quality of the English leaves much to be desired. Sometimes you simply have to know beforehand what the authors mean. Thus, when they accuse the Japanese of "attempting to achieve the specialization of the five provinces in North China," it helps to know that Japan was aiming at creating an autonomous North China (p. 2).

Despite all these defects, the book does have a place on the shelves of specialized libraries. There are 47 multicolored maps which are excellent and almost 400 pages of detailed data concerning military operations. If you want to know who commanded the 5th Reserve Division of the New 3rd Corps of the 19th Army Group during the Second Battle of Changsha, this is the book to consult.

JOHN H. BOYLE
California State University, Chico

Colonial Sequence, 1930 to 1949: A Chronological Commentary upon British Colonial Policy, Especially in Africa; and Colonial Sequence, 1949 to 1969: A Chronological Commentary upon British Colonial Policy in Africa, 2 vols. By Margery Perham. (London: Methuen and Co., Vol. I, 1967. Pp. 331. $8.80; Vol. II, 1971. Pp. 377. $12.00.)

African studies in the Commonwealth owe a great deal to women. During the 1930s and 1940s a considerable number of able and determined women carved out careers for themselves in African studies, which at the time was an intellectual frontier. These pioneers included such scholars as Audrey Richards, Lucy Mair, Margaret Green, Monica Hunter, Hilda Kuper, and Dame Lillian Penson, who left their mark on research, teaching, or academic administration. In a similar fashion, Lady Selwyn-Clarke, Margaret Roberts, Rita Hinden, and Marjorie Nicolson attained prominence along that turbulent border linking politics, especially Fabian politics, with the world of learning.

The most eminent of these women was, without a doubt, Dame Margery Perham. A prolific author, she has produced numerous books, the principal one being her biography of Lord Lugard, a massive, two-volume work cast in the "Life and Times" tradition of the Victorian era. She has turned out constitutional and academic inquiries, anthologies, polemics, and even light romantic novels. Within the British university system she wielded formidable authority during her teaching career. To be examined *viva voce* at Oxford for an advanced degree by Dame Margery, magnificently arrayed in a red doctoral gown, was a daunting experience for even the most hardbitten candidate. Dame Margery also played an important part in the training of British colonial officials and in the provision of education, expansion of research, and development of universities within Britain's former African empire. She had, moreover, a voice in colonial policy making at large.

Part of this influence derived from her skill as a journalist writing for *The Times*, *The Observer*, *The Listener*, and *The Spectator*. Her articles and letters reached a wide audience and helped to mold a good deal of educated British opinion about Africa. The present volumes, a collection of ephemera written mainly for British newspapers and prestige journals, show the way in which her ideas progressed from the early 1930s to the late 1960s. They deal with a great variety of topics, primarily African: the theory and practice of "indirect rule"; race relations in Eastern, Central, and Southern Africa; German and Italian colonial claims; dilemmas facing Nigeria, the Sudan, and Ethiopia; problems of decolonization; African socialism; and so forth. These short pieces are almost devoid of footnotes or statistical tables. They lack the rigorous analysis and the specialized terminology that would be expected in a political science course today. The writer does not show any great prescience. During the 1930s, for instance, she shows no sense of the speed with which decolonization would come about. During the 1950s, she is quite unable to foresee the problems of decolonization, including those connected with the creation of military dictatorships, the expulsion of ethnic minorities, or the nationalization of foreign property. Dame Margery, like a stern but just headmistress, instead hands out term reports; these are generally favorable. She acknowledges, for example, that the Sudan on the edge of independence faces serious problems, but notes that "in all the tension and uncertainty, the great majority of the Sudanese have remained amazingly calm and orderly. This is due partly to their own steadiness of character and partly to their faith in their British administrators" (vol. 2, p. 40). In making her points, the author relies on good narrative, on anecdotes, on personal reminiscences. She is also apt to strike a hortatory note, founded on a sense of moral certainty, that today would sound strange in most British lecture rooms. "The evidence suggests," she thus concludes firmly in a

letter to *The Observer* on "The Motives for Man's Cruelties," " . . . that God is more interested in individual than in communal sin" (vol. 2, p. 257).

For a political scientist, these essays will serve above all as a source book. They provide evidence on how the author's thinking developed from a solid belief in imperial trusteeship and "indirect rule" in the 1930s to support of African independence movements after World War II. They also offer an interesting commentary on the evolution in Great Britain of what Ronald Robinson and John Gallagher have called "the official mind of imperialism."

To the average white Kenyan or white Rhodesian, this colonial Establishment gradually became anathema. Insofar as a white colonist was aware of Dame Margery's work, he would regard her as an archenemy, part of that detested academic and bureaucratic gang hell-bent on wrecking the empire. She first acquired prominence during the 1930s through her pointed critique of the Kenyan settlers' demands for political supremacy within Kenya. She continued to follow the British missionary and humanitarian tradition that regarded British governance in Africa as a high moral trust to be exercised on behalf of the African majority. Africans, as she saw it, should be protected against oppression from immigrant settlers, from foreign (that is to say, German and Italian) Fascists, and also from home-grown tyrants. British Africa should be advanced, slowly, gently, inexorably, along the road to self-government and British parliamentary liberties. (It was not for nothing that—in addition to serving as vice chairman of the influential Oxford University Summer School of Colonial Administration, as a member of the Advisory Committee on Education in the Colonies and of the Executive Committee of the Inter-University Council on Higher Education Overseas—she was a president of the Universities Mission to Central Africa, a body distinguished for its traditional High Church links.) Dame Margery thus bitterly opposed all British concessions to the short-lived Federation of Rhodesia and Nyasaland as a surrender to white settlerdom. She was even more uncompromising to Ian Smith's UDI in Rhodesia. While she was able to sympathize with upper-middle-class whites in Rhodesia—men and women who were willing to take a liberal stance in politics and who she believed had sprung "mostly from the families of earlier settlement, sometimes of missionary origin"—she had little patience with "the newly arrived clerk or hairdresser from Britain [who] quickly absorbs a spirit of independence" (vol. 2, pp. 295–96).

Dame Margery has never lost academic credibility. Even during the height of British student unrest during the 1960s, she did not become the target of the defamation campaigns directed against personages like Walter Adams and Terence Miller, both former principals of the University College of Rhodesia and Nyasaland and convicted therefore by left-wing militants of guilt by association with white settlerdom. Yet this redoubtable woman, as these volumes show, in fact had much more in common with a Rhodesian pioneer than with a British progressive. In her Edwardian childhood, she had been drawn to Africa by a romantic interest in hunting stories and the novels of Rider Haggard. She first came to Africa just after the "Mad Mullah" campaign in Somaliland ended. "Here I found all and more than childish dreams had promised—long days on camel or pony; camp-fires at night to keep off the lions; wild-looking but beautiful tribesmen . . . ; and, best of all, perhaps, the word 'unexplored' printed across our very reticent map" (vol. 2, p. 267). Something of these attitudes has remained with Margery Perham all her life. And surprisingly enough, few dared to upbraid her when she continued to glorify Britain's imperial mission in terms that would have done credit to Kipling. Even by the 1930s, she writes, European rule in the remoter parts of Africa had just managed to "stop slave-raiding and inter-tribal war; it had checked ritual murder or the killing of witches and twins, and alleviated famine, sleeping sickness, rinderpest and other dramatic scourges. . . . In south-eastern Nigeria, I read court records of cannibalism, sometimes carried out for magical purposes upon a commercial scale" (vol. 1, p. xv). The British rulers might have been guilty of doing too little and doing it too late; they might not have done enough to check the ambitions of white settlers; but essentially, Dame Margery admired the British administrator—at his worst an honorable if somewhat overcautious gentleman, at his best a statesman of heroic proportions, cast in the mold of Lord Lugard, whom her biography tended to idealize.

At the same time, she found Ian Smith and his friends utterly intolerable. She did not, of course, oppose white farmers as such. As these articles show, she was always willing to admit that European settlers, say, the Kenyan colonists, might justly feel proud "when they survey the statistics of coffee, maize, sisal and wheat exported from their country," and when they consider their role as pioneers of modern civilization (vol. 1, p. 40). But political power for settlers should never be countenanced—in Kenya, Rhodesia, or anywhere else. Raised to the rank of rulers, they would stand in the way of progress. For, essentially, these essays reveal Dame Margery as an optimist by temperament and a reformist by con-

viction. The Empire seemed to her, in Tennyson's words,

> A land of settled government,
> A land of just and old renown,
> Where Freedom slowly broadens down,
> From precedent to precedent.

Let reform do its beneficent work in Central Africa and the future would be bright. To Smith and his followers, this interpretation seemed unrealistic. They saw their world in the mirror image of a black revolutionary. Any major political changes, they were convinced, must inevitably entail a revolutionary transformation, an all-out redistribution of property and the economic liquidation of the white minority. Between these two concepts, there could be no compromise. And only the future can show whether Dame Margery will turn out to have been right.

L. H. GANN
Hoover Institution, Stanford University

The Strategy of Technology: Winning the Decisive War. By Stefan T. Possony and J. E. Pournelle. (New York: Dunellen, 1970. Pp. xxxii, 189. $7.50.)

This is a contentious book. At a time when many writers on defense policy and armaments issues detect a great deal of symmetry in motivation as well as strategic opportunity between the United States and the Soviet Union, Drs. Possony and Pournelle stress asymmetry. The United States, they declare, has no intention of invading Communist territory (p. 152). The Soviets, on the other hand, will launch a surprise attack on the United States whenever a technological breakthrough gives them the prerequisite military superiority. It therefore is crucial that the United States devote more attention and resources to staying superior in such technology, even if this means sacrifices in slum clearance or pollution control, for any resignation to strategic parity runs an unacceptable risk that the Russians might suddenly have a military superiority to exploit.

This model of the Moscow-Washington arms confrontation is not new; it served as the conventional wisdom in the years after 1945 when Americans were shocked by Stalin's behavior, and genuinely assumed that Truman's failure to exploit and maintain the American nuclear monopoly had demonstrated once and for all that the United States was itself not an aggressive power. Possony and Pournelle's special contribution is rather to argue that this model is still appropriate today, when most of us have come to stress the need for détente and the dangers of American unconscious imperialism, when many of us have turned to the opportunities offered by the end of the Communist monolith, or by assured second-strike retaliatory forces—forces which might make it less necessary "to run the race."

The authors of *The Strategy of Technology* make almost no mention of Sino-Soviet conflict or the possibility that such conflict might distract the two Communist powers from hostility to the United States. We must continue to worry about Russian capabilities, Possony and Pournelle warn us, for we can never rely on their intentions. Assumptions about arms races based on Lewis Richardson's theories of mutual provocation are to be rejected. The ABM should be pursued, as should the neutron bomb, and numerous other weapons. Nuclear weapons should have been used in Vietnam (p. 149), although these should have been the neutron-bomb variety which might have been developed long ago but for the naive reasoning of McNamara and others. The authors indeed display a most remarkable open-mindedness and creative ingenuity on thoroughly hostile ideas; why not drop cheap handguns all over Cuba to make life hard for Castro (p. 28), or invest in possibly unrewarding new weapons technologies if only to mislead the Russians into wasting their money in the same technologies (p. 119)? Notions of limited war are misleading, the authors contend, because they imply more cooperation and community of interest with the other side than is warranted. Guerrilla wars should be avoided, but mainly because they sap American energies that must be devoted to the all-important technological race against the Soviet Union.

Possony and Pournelle make no mention of another possible "enemy," the self-serving military officers who might try to sell us weapons we don't need, in the simple empire-building machinations of bureaucratic politics. Rather these are portrayed as far-sighted advocates of national preparedness, trying to overcome the short-sighted naivete of the arms-controllers.

If one boggles at accepting the authors' picture of comparative intentions in the two superpowers, one might more readily, however, accept another of their caveats, against assuming that nature will always be so kind as to make decisive breakthroughs in new weapons technologies impossible. We have too often in the past been told that obnoxious weapons, e.g., the H-bomb, or very accurate missiles, or ABM's, would be impossible. What if anti-submarine warfare (ASW) turns out to be less difficult in the future than is currently predicted; won't we all be in a lot of trouble? Yet the authors hardly mention a much more dangerous technological possibility, the spread of nuclear weapons. Should we really brood about Soviet weapons breakthroughs more than about nuclear weapons in the hands of the IRA or Mafia or FLQ?

Of course, the rest of us can easily have gone

overboard on detente and mutuality of interest. The Possony and Pournelle book may look less antimainstream if the current detente turns sour, perhaps with new Russian provocations in the Middle East, or with new rounds of strategic weapons procurements making impossible any acceptable SALT II. If so, the authors will draw congratulations for having kept the faith when so many others had deserted it. As things stand, however, I do not predict that history will come to Possony and Pournelle's rescue.

GEORGE H. QUESTER
Cornell University

The Persian Gulf: Iran's Role. By Rouhollah K. Ramazani. (Charlottesville, Va.: University Press of Virginia, 1972. Pp. 157. $7.50.)

In the preface to this book the author states that his primary aim is "to clarify the role of Iran in the Persian Gulf by examining the relevant foundations and development of Iranian interests and policy" (p. x). The result is a general and quite useful survey of how Iran came to achieve its current position of local dominance in that area, but readers who seek information about how Iranian foreign policy is made will find little enlightenment within these covers.

The existence of oil and the need for security along its trade routes are, as the author points out, the real reasons why the Shah has displayed such an interest in Gulf affairs over the last decade. What Professor Ramazani does not, however, stress sufficiently is the way in which Teheran was aware of the potential dangers in the Gulf long before either the oil consumers or the other littoral states woke up to the realities of the situation. From the moment when the threat of militant Arab socialism deriving its inspiration from Cairo—and finding concrete expression in the existence of Egyptian troops in Yemen—made itself felt, the Shah took steps to coordinate foreign, economic, and military policies with a view to protecting Iranian interests. This action certainly stemmed from an understandable desire for self-preservation on the part of the Shah, and Professor Ramazani is at pains to make this point. An equally important factor in the situation—and one that receives much less attention in the book—was Iran's realization that her erstwhile allies (particularly the U.S.A.) might not be willing to come to Iran's aid during a moment of crisis. Washington's speedy recognition of the republican regime in the Yemen and the western countries' apparent reluctance to grant economic aid at a time when oil revenues were still low certainly strengthened the Shah's desire for greater Iranian self-sufficiency.

This desire for greater local autonomy gave Teheran a headstart when British withdrawal from the Persian Gulf was announced in January 1968. It was then that many of the problems which had remained more or less dormant during the period of British dominance began to re-emerge as subjects of immediate dispute. Iran could seize the diplomatic initiative in this situation for she had already begun to exercise greater control over her own Gulf provinces. This period of preparation, though not always smoothly successful, was the key to Iran's later achievements; unfortunately, it is not given sufficient prominence in the book.

The perceptive nature of Teheran's policies has found reflection in the Shah's awareness of the dangers posed by the revolt in the Dhofar province of Oman—a subject all but ignored in this book. The existence of Soviet, and later, Chinese assistance for the rebels shows that the significance of this movement was not lost on Moscow and Peking, but such aid is not mentioned by the author.

In discussing the former Trucial States, Professor Ramazani adopts the widely prevalent view that these small states are not really viable political entities. No realistic commentator on Persian Gulf affairs would wish to belittle the dangers which these new states face, but to say baldly that they "lack the human and natural resources necessary for the creation of a powerful union" (pp. 89–90) is misleading. A powerful union could very well be a threat to peace in the Gulf, whereas an adequate but wealthy union, which derives its strength from the minimal nature of its demands for political loyalty—and in an area of old tribal rivalries minimal demands are the ones most likely to be successful—could well continue to live at peace with its larger and more powerful neighbors. This tendency to discount, if not altogether write off, the value of the small states of the Gulf is a common but dangerous error.

For that somewhat rare person, the general reader, however, the book makes several good points: that Iran's role in the Gulf has been a reflection both of the character of her ruler and of her internal political situation; that internal security has helped foster a more self-confident foreign policy; that the historical myth of a continuously active Iranian interest in the Gulf powerfully affects public attitudes in Iran. The point that any regime in Iran, be it royal or otherwise, would have, under current circumstances, to take a growing interest in the Gulf is infrequently made but is very important for a realistic appraisal of the future.

One final but necessary criticism. For an academic press to publish a book in which there are errors in the bibliography, in which the same bank of a river is described as the North bank on page 4 and the East bank on page 19, and in which the

place name spellings on the maps differ misleadingly from those in the text is a disgraceful reflection on present standards of proofreading and book production.

R. M. BURRELL
University of London

The Diffusion of Power, 1957–1972: An Essay in Recent History. By W. W. Rostow. (New York: The Macmillan Co., 1972. 739 pages. $12.50.)

Walt Whitman Rostow inhabits at least three *personae* that contribute to his massive new book on foreign policy. After early government service, he became visible as a Cambridge (Mass.) professor who wrote books and advised princes. Like the original Florentine consultant, his writings and advice were often influential. He then moved into the very center of princemanship, becoming successively Deputy Special Assistant to President Kennedy, Chairman of the State Department's Policy Planning Council, and from 1966–69 Special Assistant for National Security Affairs to President Johnson. The third Rostow *persona* is as theoretician and ideologist of national strategy.

The author calls his book "One man's view of the years 1957–72." The first 129 pages review the postwar world, and in the light of Revisionist distortions it is useful to be reminded again of the Stalinist power-grabbing years and the appropriate U.S. response.

The next 175 pages cover the Kennedy administration, and the 225 pages that follow, the Johnson administration, with chapters on the organization of national security affairs, military policy, the nonproliferation treaty, SALT, development, crises, and of course Vietnam.

The present book is of great value in comprehending the past 16 years as seen through the eyes of a brilliant if controversial economic historian who also enjoyed a rare vantage point inside the policy machine. It is written with clarity and élan, and covers with sweeping strokes a broad spectrum of men, places and events, colored with the always-stimulating Rostovian interpretation.

At the same time, this presents serious problems flowing from its dual nature as history and memoir. Walt Rostow has been singled out for an uncommon amount of abuse because of his role in the Vietnam War, and I do not want to add to that painful controversy. Nevertheless, this book seems to me to labor, perhaps inevitably, under certain deficiencies in terms of objectivity and sensitivity which require recognition along with its positive qualities.

The problem for the reviewer arises when Professor Rostow gives evaluative judgments of the main actors and events of the era. By uncritically praising virtually all that was done, he not only suspends the historian's critical judgment, but also revives in the vulnerable medium of scholarship some of the sharpest edges of the quarrels of the recent past.

Uniformly complimentary toward five presidents, each seen as responding appropriately to challenge, he ensures that none of their underlying premises about the nature of the world or the U.S. role is seriously examined. The book becomes a downright apologia with President Johnson, who "will rank among the greatest" (p. 533). Critics to the contrary, LBJ was "a thorough professional in foreign policy" (p. 366). What many perturbed (and equally loyal) Americans were finally forced to consider a brutalizing, tragic, and eventually demented period in our national history, our Dr. Pangloss assures us was a "creative period" in which Johnson left to Nixon "an agenda potentially more manageable than he or his predecessor had confronted" (p. 312). Lest either Mr. Kennedy or Mr. Johnson might ever consider changing a policy because wrongly conceived, on his own proud testimony there was Professor Rostow at their side to reassure both men of their greatness, and of the wrongness of their "liberal" critics (p. 300).

Playing this role evidently forces suspension not only of critical judgment but of even the ordinary categories of inquiry. For example, the Dominican Republic in 1965 is not worth much discussion because "morally, the question was not difficult" (p. 413). One-time analyst of Soviet affairs Rostow refuses to acknowledge that to intervene militarily in an allied country to prevent an unwanted change in regime is an equally valid description of the Soviet move into Czechoslovakia in 1968. Rostow's discussion of the Dominican Republic deals only with calculations of national interest and the tactics of success, and if it is all any different from the ends-means calculus of a Khrushchev or a Brezhnev, I'd like to have it explained.

On Vietnam the author's vision remains undimmed by nuance or misgiving. The only issue he reports having encountered in his famous 1961 trip with Maxwell Taylor was "Communism or unification under Hanoi" (p. 274), plus an exaggerated linkage with Chinese power and prospects. Professor Rostow acknowledges the existence of arguments about U.S. prestige, power, ideology, human life, the effect on the next generation of Americans, and the other elements of the Indochina tangle. But he easily satisfies himself regarding the "moral issues" concerned, and his only real disappointment was that both Presidents Kennedy and Johnson resisted his advice to invade North Vietnam (p. 513). In the same spirit he adjudges President Kennedy's failure to deal punitively with violations of the Laos accords "the greatest single error in American policy in

the 1960s" (p. 290). Maybe he is right about that, which *did* involve the Soviets and thus the larger equilibrium.

But we really learn nothing new, nor are we helped to a more balanced perspective. The author is at best patronizing to those who disagree, and at worst he is unable to see dissent about foreign policy as anything but hopelessly misguided, glandular, or a fatal disease of the left.

Professor Rostow started out as an economist and, unlike most writers on foreign policy, always has in mind the nonpolitical elements in international relations, which of course were increasingly crucial even as American leaders in the 1950s and '60s became locked into political-military approaches to problems that might have benefited enormously from some of Walt Rostow's own earlier insights concerning the economic, social, cultural, psychological and human components of countries in transition. The heady spirit in which Rostow recounts his wife's *bon mot* that "You're the junior officers of the Second World War come to responsibility" (p. 215), accords with his approving description of the Washington bureaucracy of 1962 as operating "less like a governmental machine than like a regimental command post in combat . . . " (p. 259). But of course no known regimental command post gives much house room to "soft" sociocultural, psychological, and ethical issues.

For us in the center the intellectual dilemmas remain unresolved. (The right and left never had any doubts.) How usable is Rostow's overall map of our times? Does his view of the 1960s expand our understanding? Or is he seeking to make permanent all the conceptions, some of them erroneous, that were activated under savage pressure in that harrassed and ultimately beleaguered command post? Will contemporary history look, in retrospect, like the logical, seamless web Professor Rostow spins for us? Or are there other secular trends that are unavoidably missed by both the committed public servant and the historian *engagé* (just as they are missed by the intellectual *enragé*)?

Who, then, is today's open-minded student of foreign policy to believe? The ivory-tower theorist who pontificates about policy in chronic ignorance of its pressures and imperatives? Or the enthusiastic salesman of official policies who can also write history? The third, and preferred, choice would be a knowledgeable scholar who combines experience and critical judgment. Walt Rostow is a trained historian, a persuasive advocate, and a stimulating intellectual. He could have written his memoirs, and no one would be entitled to criticize them because they were *parti pris*. But he has chosen to write an insider's story with the trappings of both theory and history.

Given the dilemma the author has created for himself, all one can prescribe is a more balanced intellectual diet. We ought to accept with gratitude the generosity to give, and the eagerness to share, of this irrepressible, industrious, committed historian. But we need some roughage from more objective critics, along with a few supplemental vitamins to keep up our strength and enhance our eyesight as we pick our way among the shadows.

LINCOLN P. BLOOMFIELD
Massachusetts Institute of Technology

Small States in International Relations, Nobel Symposium 17. Edited by August Schou and Arne O. Brundtland. (New York: John Wiley & Sons, 1971. Pp. 250. $16.50.)

This book presents papers delivered at a symposium on small states at the Nobel Institute in Oslo, Norway. The participants came from twenty countries, most of them small states. Among the groups of internationally known specialists in that particular subfield were David Vital, Jacques Freymond, Gerald Stourzh, and "the mother of small state studies," Annette Baker Fox.

As one would expect from the participation of such high-powered experts, the book is a valuable contribution to the rapidly growing literature on small states in international relations. The contributions, however, are uneven, with little overall coherence between the chapters. Most of the papers are in fact isolated case studies. Considering the amount of expertise gathered at the conference, it could probably have been put to better use if the editors had presented the paper-givers with a conceptual framework prior to the conference, rather than extracting the structure from the transcript of the discussions after the meetings. If the writers had been asked to respond to specific sets of questions and to arrange their data correspondingly, the scholarly value of the book would have been greatly enhanced.

The symposium was to a large extent carried by three groups of specialists: one with Fox, Vital, Barston and others with a background in theory and teaching; another with representatives of developing countries as J. J. Okumu of Kenya; and a third group, with Erling Bjøl of Denmark, L. G. Jaquet of the Netherlands and S. F. Lemass of Ireland, all of whom had long years of practical experience in international politics before they entered academic life.

The first group devotes much effort to definitions and conceptualization. When writing about small states, how can one avoid including middle-range or even ex-great states in the analysis? None of the theorists comes up with a final answer. With close to a hundred countries potentially on the world's small-state list, it seems

rather futile to look for a common denominator. Population, size, GNP, and other traditional variables are clearly inadequate as analytic criteria. One tends to agree with David Vital who points at the great diversity involved. A small state may be weak in one area, but still surpass even great powers in another. Vital illustrates some of these differences through his three paradigms, represented by Czechoslovakia, Israel and Finland (elaborated in his book, *The Survival of Small States*). Even if we add some recent modifications for Israel, this analysis by subdivisions and special categories seems more fruitful than the traditional search for general definitions and criteria.

The pragmatic approach is further accentuated by the "ex-practitioners." Bjøl, Jaquet and Lemass all emphasize the historical dimension. Denmark and Holland are both unique in the sense that through centuries they have been able to switch from deep involvement in great power politics to almost complete disengagement—while still maintaining an essential influence. In his paper Bjøl points out the importance of linking small state studies to the constant changes in the international system, while Jaquet, in a more policy-oriented approach, stresses the small states' options for influence within systems of alliances. J. J. Okumu, who confines his discussion to African states, makes an important point by showing how small developing states differ from the small industrial ones in elite perceptions, domestic preoccupations, legal concerns, and other major variables which he feels are accentuated by their newness as international actors. Clearly, in certain areas of international activities, the interests of both categories of state may converge. But while most of the small industrialized states struggle to preserve the status quo and hang onto what they have, most developing states see their future as dependent upon change. These two positions call for a fundamental difference in approach.

In terms of theory and generalized observation, the partly transcribed summary of the discussions is a particularly rewarding part of the book. The symposium stressed greatly the importance of functions, a point which, incidentally, has been dealt with in much greater detail by the leading Norwegian expert on small states' problems, Trygve Mathisen, in a recent book, *The Functions of Small States in the Strategies of the Great Powers.*

But there is no need for the small state to try to cover all three levels equally well. Attempting to do so, could do more harm than good in systemic as well as national terms. A crucial point, forcefully argued by Jacques Freymond, is how far a small state should try to extend its functions. Should it engage heavily in international affairs or stick closely to its national environment? Using a systems approach, Freymond's concern is the increasing tendency to refer problems to levels above those on which they occur. Local confrontations often escalate into world problems causing conflicts far out of proportion to the original issues. He feels that the international system can function only on the national, regional and global levels. Therefore, each small state should aim for solving its problems on the national level by its own means. It must, he says: " . . . take care not to become a source of trouble to others."

This proposition appears generally acceptable in principle, but is hard to apply in practice. Dr. Freymond's case country, Switzerland, has managed to live close to such expectations for about two centuries. But how applicable is the Swiss experience to that of other small countries? What is involved here is the whole issue of nonalignment as a policy orientation, which a majority of small states practice more or less successfully. To "stay out of trouble" means in more concrete terms to accept the conditions in the country, more or less as they exist. If they are to be improved, it would have to be done by the "bootstrap method," i.e., by internal, national development. Inviting aid and foreign investments may accelerate progress, but this also carries the risk of neocolonialism and external entanglements which might cause escalation and further trouble.

If one carries Freymond's suggestion a few steps further, one will in fact be asking whether a true neutrality or nonalignment policy is conditional on the acceptance of any status quo, modified slightly by marginal adjustments on the national level, possibly with some carefully measured outside help. However prudent this may sound, it seems certain that most small states will find it unacceptable because the whole meaning of their emergence into statehood is connected with major rather than marginal change.

Quoted out of context Freymond might be reprimanded for suggesting some sort of "neofeudalism" where every nation has its place—and stays there. This is clearly not his intention. The Swiss case, on which he draws, seems to carry his proposition. As it becomes increasingly unique, however, the questions he raises are likely to gain in relevance. While accepting social, economic and political change as the model for behaviour, how can one avoid the corollary of small states' "troubles" spiraling into global engagements, as illustrated by the recent Middle East confrontations. As most of us seem to agree that we cannot afford much more of such, how will the international conflict regulations, which now seem immediately necessary, affect small states' perceptions of change versus status quo as guidelines for their policies?

None of the papers delivered at the Oslo sym-

posium present anything like an answer to this fundamental problem, but even with its blend of triviality and excellence, the book appears to be a useful addition to the literature on problems peculiar to small states.

NILS ØRVIK
Queen's University, Ontario

Aden and British Strategy, 1839–1968. By Harvey Sicherman. (Philadelphia: Foreign Policy Research Institute, Research Monograph Series, No. 12. August, 1972. Pp. 53. $3.50, paper.)

To the student of world politics in a post-imperial age, the recent experience of Aden with both imperialism and independence is more instructive than that country's comparatively low power position might initially suggest.

From 1839 on, the Red Sea port of Aden came to be prized as a vital link—"the Gibraltar of the East" it was called—in the extended chain of colonies and holdings which gave coherence and form to the British Empire. Englishmen found Aden commercially convenient and militarily useful. Faith in impregnable fortresses, the utility of direct rule, and the logic of "preventive possession" only further reinforced this imperial impulse. By the late 1940s, however, such conventional wisdom had been invalidated by events, and Britain began her recessional. The intervening century drained imperialist doctrines of their cogency and Britain of her will to empire.

Consequently, 1967 saw Aden pass quickly from imperial outpost to Middle Eastern variant of a people's republic. But almost a decade of sovereignty has merely substantiated rather than altered her previous condition of dependency. Southern Yemen remains subject to the pressures and insecurities of larger forces, of systemic as well as regional considerations. Made vulnerable internally because of a weak social and political infrastructure, Aden finds her domestic instability is matched by total economic dependence upon the reopening of the Suez Canal as a major avenue of international commerce. In terms of the regional subsystem, Southern Yemen's independence is jeopardized by stronger peninsular actors, such as Saudi Arabia and Yemen; while Soviet Russia and China have each established a presence in Aden since 1967.

As the historical perspective offered by this monograph makes evident, Aden would seem, if anything, to illustrate the enduring salience of spatial location and geopolitical configurations. Witness the effective naval blockade imposed during the 1973 Arab-Israel conflict in waters immediately adjacent to Southern Yemen. Situated at the highly strategic point where Asia and Africa meet, where the Red Sea and Persian Gulf converge and lead on to the Indian Ocean, Aden continues to offer inducements to ambitious great powers, and thus serves as a barometer of global trends.

While the author might have expanded upon the prospects and perils for Adeni independence after 1968 as a result of the British legacy and geopolitical constants, he provides a detailed and useful overview of British policy up to that point. Arguments and pressures governing London's attitude toward Aden are surveyed, unfortunately without recourse to the original British archives, for each of the three phases: acquisition, retention, and release of Aden. Sicherman indirectly also offers a potential documentary case study of decision making and bureaucratic politics by recalling the various schools of thought and ministerial rivalries which prevailed. It suggests that rather than being simply a matter of divide and rule in Aden and the Middle East, Britain, as a decisional unit, came eventually to rule and be divided.

With the British record available to them, Soviet leaders will find it difficult to argue, at some future accounting, that they have acted in Aden and the Arab world in a fit of absent-mindedness.

AARON S. KLIEMAN
Tel Aviv University, Israel

Weak States in a World of Powers. By Marshall R. Singer. (New York: The Free Press, 1972. Pp. xii, 431. $10.95.)

In examining why weak states are weak, Marshall Singer has produced a book on international relations which reverses the usual focus on power. Abandoning the traditional emphasis on great power relations, he looks at the international system from the point of view of countries with little power. He intended to "illuminate the multiple dimensions" of power and to remedy common distortions by including in his analysis interpersonal and intergroup relations. He also meant to concentrate on the noncoercive or "attractive" means by which power is exercised and to mute (even denigrate) military capacity. Following a fairly orthodox discussion of the elements of power which stresses the psychological or perceptual point of view, Professor Singer devotes an important part of the book to a detailed discussion of different kinds of ties between states.

Interesting tables illustrate the existence of perceptual ties (divided into historic, linguistic, religious, racial, class, ideological, and educational identification); of communication ties; economic ties; military ties; and political ties. Many of the tables group the larger powers with smaller states associated with them to tease out patterns of trade, aid, international news service ties, delivery of jet aircraft, voting behavior in the United Nations Assembly, formal political organizations, languages spoken, and the like.

The somewhat prolix discussion of the various ties occasionally produces a noteworthy, or at least novel, point. For example, the "haves" in underdeveloped countries often show important links with the "haves" in advanced countries, but the same is not true for the "have-nots" in the two kinds of countries (pp. 139–41). For another, ideologies of the great powers influence those in smaller states, but not the other way around. Singer stresses the vital influence of education, and in this discussion states that "in 1963 the United States was the country to which almost forty per cent of the independent countries of the world sent most of their overseas students for study" (p. 162).

The author points out the implications that their relatively meager sources of information have for weaker states. When a weak state reacts against a perceptual "mentor," it often turns to another, thus not diminishing its dependency (p. 177), and the same holds true for economic ties (p. 226). In discussing foreign direct investment, Singer states that what began "as a mutually advantageous economic relationship also often takes on, by its very success, highly political overtones" (p. 265). He makes a distinction between "perceived political influence" and "actual political influence," suggesting that decision makers in a weaker state, without basis in fact, behave as though they thought the greater power was almost omnipotent (p. 314).

Professor Singer is from the start concerned about another distinction: between dependence and interdependence. His description of the latter condition, which exists among many of the advanced industrial states, is useful in clarifying many relationships. He comments, for example, that *no* state is interdependent vis-à-vis the Soviet Union (p. 362). At several places in his analysis he points out a mutuality of interest between a great power and weaker states, as in the case of educational exchanges and economic development of the underdeveloped countries.

Although the author has avoided a common and rather fruitless effort to define "weak states," and thus avoids the need to rank states according to their power, his approach leads him into another analytical trap. By generalizing about the behavior of weak states he often fails, in the course of his lengthy discussion, to make the appropriate qualifications, and he often makes simplistic statements or judgments concerning some relationships which more detailed diplomatic investigation would reveal to be highly inaccurate. To take a few examples, British power, he says, prevented the U.S. from absorbing Canada in the 19th century but only stopped "political absorption" (p. 106). Later he refers to Canada as being dominated by the United States (p. 363), a statement which would be surprising to Prime Minister Trudeau or any of his predecessors going back a long way. It is quite untrue that dependent countries "send troops when the U.S. says 'Send troops!'" (p. 364). Or consider the absurdity of his statement that "The British Commonwealth and the French Community, while not specifically military organizations, presumably can function that way" (p. 280). His discussion of the OAS is equally faulty, especially as it avoids mention of the ways in which some of the Latin American members have used it to underscore their diplomatic independence of the U.S. when it suited them. His references to NATO, also, reveal a one-sided perception of its functions.

The book runs down hill after a strong effort to be realistic and analytical. The last part is undisguisedly didactic, as the author lays down his prescription for how strong states and weaker ones should behave. And it ends in a jeremiad warning that neither of them show the appropriate signs that they will do so. In this section he throws away all his careful qualifications of earlier discussions of state behavior and group perceptions, repeats the old simplistic adage, that "Decision makers in all states seek to maximize the power of their state" (p. 380), and makes such generalized statements as that "the weaker states believe they need relatively large military forces . . . " (p. 408). He makes recommendations which are quite inconsistent with his earlier analysis, such as that wealthy states should provide aid to weaker countries regardless of regime (p. 403). In this section, the drawbacks of the personal, conversational style of the book are compounded by a tone of outright advocacy.

ANNETTE BAKER FOX
Columbia University

New Left Diplomatic Histories and Historians: The American Revisionists. By Joseph M. Siracusa. (Port Washington, N.Y., and London: National University Publications, Kennikat Press, 1973. Pp. viii, 138. $6.95.)

Much of the early writing on the Cold War took for granted sole Russian responsibility for the rupture of the World War II coalition. Debates centered instead on the sources of Soviet imperialism: Was its etiology to be found in Stalin's megalomania? In traditional Russian expansionism? In Bolshevik ideology? Critical reactions to this literature include not only more balanced interpretations which avoid dividing the world into the good guys and bad guys, but also a series of books that return to the theme of a single culprit—the United States. Throughout much of the 1960s, a steady outpouring of books by such prolific authors as William Appleman Williams, Gabriel Kolko, Gar Alperovitz, and others por-

trayed America not as a nation that had taken leave of its senses in the disastrous Vietnam debacle, but as one that has always been imperialistic owing to one or another aspect of its capitalist political economy. Emphasis varied from study to study, but most adopted some variant of a common theme. Since the late nineteenth century America has had to choose between one of two courses. It could have restructured its institutions by establishing a socialist political economy that is assumed to be self-sufficient, and is therefore without needs that must be satisfied by external economic ties. The alternative was to embark upon a search abroad for raw materials to sustain the industrial complex, and for markets into which to dispose of agricultural and industrial surpluses—that is, the policy of the "open door," a synonym for imperialism in the lexicon of the New Left. According to the revisionist historians, American leaders at least since William McKinley have repeatedly selected the latter course, partly in order to avoid confronting the implications of domestic reform. Thus, imperialism is, in the words of one revisionist historian, "as American as apple pie." San Juan Hill and Vietnam are separated in time and space, but not in the needs and motives which gave rise to them.

That these revisionist works should have had widespread appeal during the agonizing Vietnam years is understandable; that many were spared searching critical review—even by those that had been sensitive to the Manicheanism of the ultra-right—is less so. This state of affairs now appears to be changing. Joseph Siracusa's study, a revised version of his doctoral dissertation, joins a growing list of books and essays that examine critically the premises and practices of the New Left historians.

Siracusa's small book covers a broad terrain. His opening chapter compares and contrasts the revisionists' work with critiques of American diplomacy by the "realist" school. Where Hans J. Morgenthau, George Kennan, and others find a record often marred by naive idealism and irrationality, the revisionists see only consistency and rationality in assessing the needs of a capitalist political economy, and in pursuing policies to satisfy those requirements. One result is that hitherto lightly regarded figures such as McKinley are transformed into political geniuses who saw with great clarity how empire would resolve the contradictions of capitalism. Chapter 2 focuses on William Appleman Williams, the intellectual dean of the revisionist school. The third chapter examines the New Left reconstruction of American diplomatic history between 1898 and 1941, with emphasis on the Spanish-American War, annexation of the Philippines, the Hay "Open Door" notes, American entry into World War I, the League of Nations, and American involvement in World War II. Historians under scrutiny include Walter LaFeber, Thomas McCormick, Gordon Levin, Arno Meyer, Carl Parrini, Lloyd Gardner, and Robert Freeman Smith. The fourth chapter deals with the origins of the Cold War as viewed by four prominent revisionists—LaFeber, Gardner, Alperovitz, and Kolko. The conclusion reports the results of a survey among the most prominent American diplomatic historians; Thomas A. Bailey, Ernest May, Robert Farrell, Richard Leopold, Julius Pratt, and many others were asked to evaluate the work of the revisionist historians. Not surprisingly, these assessments range from mixed to highly unfavorable.

Although it may serve as an introduction to some key assumptions shared by the New Left revisionists, the Siracusa study is not wholly satisfying. It is long on description and very short on analysis. As a result, this book lacks the analytical insight of Robert Tucker's *The Radical Left and American Foreign Policy*, or the specificity and detail of Robert James Maddox's *The New Left and the Origins of the Cold War*, a brilliant exposé of logical and methodological horrors in the revisionist literature. In contrast, Siracusa's book seems bland and antiseptic. Long quotations from the books under review mark each chapter, as Siracusa appears content to let the revisionists speak for themselves. Certainly *New Left Diplomatic Histories and Historians* falls short of fulfilling the promise on the dust jacket: "Dr. Siracusa's provocative thesis challenges revisionist assumptions, methodology, and interpretations, and offers a new perspective on the past decade of historiographic realignment." The survey of leading diplomatic historians reinforces the reader's feelings that Siracusa is willing to let others analyze and evaluate. This is regrettable because as a young historian without a professional stake in "establishment" interpretations of American diplomatic history (unlike those whose judgments were canvassed for the concluding chapter), Siracusa was in an especially good position to develop a forceful case for the strengths and weaknesses of the revisionist literature.

Some New Left historians have launched a vitriolic campaign against the Princeton University Press for having published the Maddox book. Although Siracusa's concluding sentence reads, "In sum, much of the New Left diplomatic historiography that emerged in the 1960s lacked intellectual validity" (p. 119), one doubts that even the most radical of the revisionists will feel much compulsion to undertake a similar attack against the publisher of *New Left Diplomatic Histories and Historians*.

OLE R. HOLSTI

Duke University

War Coalitions: The Distributions of Payoffs and Losses. By Harvey Starr. (Lexington, Mass.: D.C. Heath, 1972. Pp. 224. $12.50.)

This study tests several hypotheses about the distribution of rewards among war coalition partners. It is based on statistical analyses of data concerning 36 coalitions which participated in 30 international wars since 1821. Some major findings are: (1) A coalition member's percentage share of spoils, in the form of territory or indemnity, is more closely related to the extent of its *participation* in the war, than to its power, or its ideological compatibility with other coalition members. (2) The extent to which a coalition member is *satisfied* with the outcome of a war is not closely related to its share of the spoils, but depends on victory in the war. (3) Generally speaking the equality of the distribution of spoils among coalition members is determined by the size of the war, or the size of the coalition. The larger the war, or the larger the coalition, the more unequal the distribution of spoils will be.

This is a tremendously ambitious project in which Starr shows much promise for the future. He utilizes an impressively wide range of historical materials to test a large number of interesting hypotheses. Furthermore, he makes a valiant attempt to communicate his findings clearly to both the historically oriented, and the more quantitatively oriented reader.

Unfortunately, he does commit some serious logical and methodological errors. There is, for example, a series of levels-of-analysis errors. Starr is interested in two basic questions about coalitions: (1) Who gets how much? (2) How equally is it distributed? These are national level, and coalition level questions, respectively. Starr is insufficiently aware of this distinction. When he performs coalition level analyses, he usually assumes that the N is equal to the number of *states* involved, thus overestimating the statistical significance of his results by a considerable margin. At one point, he correlates the equality of the distributions of battle losses suffered by coalitions with the equality of the distributions of spoils after the war. Since the correlation is positive, he mistakenly concludes that this means that the spoils are being distributed to *states* according to the share of battle losses they suffer (p. 84). This is a fallacious national level conclusion based on a coalition level analysis. Most seriously, this levels-of-analysis confusion leads Starr to formulate and test a large number of self-contradictory hypotheses which involve incorporating variables into national level models which only make sense in coalition level models, and vice versa. For example, he correlates the extent of ideological compatibility in *coalitions* with percentage shares of indemnity received by *states* in the coalitions (p. 92). It is not logically possible, of course, for *every* state in a coalition to get a *larger* percentage share of *anything*, no matter how great the "we-feeling" in the coalition.

Perhaps the most serious error Starr makes involves his choice of indices for the dependent variables. On the national level, Starr is interested in explaining the distribution of shares *within* coalitions. Unfortunately, his analyses compare percentage shares *across* coalitions. This means, first, that virtually all of the national level coefficients Starr reports reflect irrelevant between-coalition covariance, in addition to the theoretically appropriate within-coalition covariance. Second, the percentage shares are not comparable because of the different sizes of the coalitions in Starr's analyses. Finally, several of the national level correlations Starr reports are statistical artifacts brought about at least partially by the fact that both the dependent and the independent variables are functions of the varying sizes of the coalitions. For example, the larger the coalition, the lower, on the average, will be the coalition member's percentage shares of not only spoils, but also power, participation, or gidgets.

There is a similar problem with Starr's coalition level analyses. The upper limit of the Gini index which Starr uses to measure the inequality of distributions is $1-1/N$. Therefore, if a coalition consists of two states (as do several of the coalitions analyzed by Starr), and one state receives 100 per cent of the spoils, the Gini index is only .50. Thus, Starr finds that the size of coalitions is positively related to the inequality of distributions of spoils in coalitions (p. 77) partly because the index of inequality which he uses is a function of the size of coalitions.

Starr also uses stepwise regression in a rather atheoretical manner which was likely to lead him to his conclusion that the largest pot of independent variables is the most productive. Furthermore, there is at times a tenuous connection between the coalition theory which Starr cites and the analyses he performs. For example, he claims that the negative correlation he finds between percentage shares of spoils states receive, and the size of coalitions, (p. 54) yet another statistical artifact, supports Riker's size principle. Finally, several of Starr's "judgmental" variables are so ambiguously defined that a test of intercoder reliability revealed agreement as low as 49 per cent (p. 195).

Despite these problems, I look forward to Starr's future work. We need more scholars who are willing and able to undertake projects of the kind that Starr attempts in *War Coalitions*. And if this project has serious flaws, that is partly because, to repeat, it is so terribly ambitious.

JAMES LEE RAY
SUNY at Fredonia

The American Revolution and the French Alliance. By William C. Stinchcombe. (New York: Syracuse University Press, 1969. Pp. 246. $7.95.)

Until the appearance of this volume the only book that dealt with the origins of the French alliance was Edward Corwin's *French Policy and the American Alliance of 1778* (Princeton, 1916), published well over a half century ago. Corwin relied mainly on edited documents from French archives, wrote from the perspective of European politics, and concentrated on diplomatic problems virtually to the exclusion of other issues.

In contrast, Professor Stinchcombe devotes himself to the American side of the alliance diplomacy. His treatment is also broader. Basing his work on official correspondence, private letters, diaries, newspapers, and relevant secondary literature, he shows the effect of the French alliance on the politics of the American Revolution and analyzes how American leaders used it in formulating foreign and military policy. These are, in effect, his main themes.

In order to accept the alliance and the relationship that went with it the American people had to hold in check a deeply rooted anti-French and anti-Catholic prejudice. The author explains their attitudes, or at least the reactions of special groups of Americans such as the politicians, the military people, the clergy, the business people, and the journalists, to the wartime cooperation with France. He analyzes alignments in the Continental Congress over the alliance and over the politics of the French relationship, the influence of French diplomats on internal as well as external American politics, on military aid, and on diplomacy, and the extent and influence of French propaganda in the United States designed to maintain support for the alliance.

In its main features this story is well known to scholars, but no one has told it as well or in such detail as has Stinchcombe. He sets the record straight, and with no apologetics. For example, he rejects the old textbook chestnut that France aided the United States for the purely selfish reason of seeking revenge on England for the defeat in the Seven Years' War. France's motive, he points out, was neither diabolical nor altruistic. The alliance served the mutual interests of both countries. America's self-interest in the arrangement was as great as that of France, and her need for it was greater. "Americans would have broken the alliance in its first months," the author concludes, if it served their interests to do so—essentially "if they could have ended the war with recognition of United States independence" (p. 23).

Another myth the book refutes is that the American commissioners at the peacemaking in Paris in 1781 were naïvely willing to cooperate fully with their French ally, but did not do so because they had to defend the United States against betrayal and the evils of European diplomacy. Stinchcombe reiterates clearly that the American commissioners, not the French, violated treaty obligations that prohibited either country from signing a separate peace. The Americans unabashedly and self-righteously bypassed the French and concluded a treaty with England on their own.

Despite the difference in approach between Corwin and Stinchcombe, both men reach the same basic conclusion, that without the alliance the United States probably would not have achieved independence, and that with independence in 1783 the alliance had served its purpose and became defunct. This interpretation differs from that of those military historians and others especially imbued with patriotic pride who insist that the alliance was useful but not essential to victory against England, and that the prowess of American fighting men really won independence from England. These writers overlook the changing nature of the war of the American Revolution. Before it ended, it had become a multi-national war of considerable dimension, and as Stinchcombe shows, the United States had become critically dependent on France.

This is a well-researched doctoral dissertation that has been shaped into a gracefully written book. Its interpretations and judgments are usually sound and stem logically from the evidence. It illustrates well principles useful to political scientists, such as how in an alliance in time of war, the weak state becomes virtually the client of the stronger one. It also says much for the skill and *realpolitik* of America's first diplomats. With their eyes on the main chance and with a shrewd awareness of their country's true interest they were able to extract much from both a powerful ally and a strong enemy.

ALEXANDER DECONDE
University of California, Santa Barbara

The United Nations and the News Media. By Alexander Szalai with Margaret Croke and Associates. (New York: United Nations Institute for Training and Research, 1972. Pp. vi, 323. $6.50.)

Several hundred news correspondents file despatches and talk into microphones at United Nations Headquarters during each General Assembly. Any political crisis promptly increases these numbers. The UN Office of Public Information (OPI) grinds out tens of thousands of handouts, and supplements these with radio broadcasts. It offers recordings, photographs and films to any takers. Surely, it might be thought, no one could lack information needed to judge what happens in the global organization. And, in fact, some governments have so much faith in this outpouring that their delegates vote repeatedly for

stronger efforts by OPI to inform the peoples of the world on such subjects as aid for economic development and the opposition to apartheid in South Africa.

Before this study by Professor Alexander Szalai and others at the United Nations Institute for Training and Research, almost all statements about the use of public information in the UN setting were based on pure impression. OPI itself never was equipped to research any aspect of its output, and indeed bureaucratic inertia and journalistic pride suppressed efforts to think scientifically about an $8 million annual program, plus an unknown amount more in associated agencies. Such thinking would obviously have practical consequences and Szalai ends his study with programmatic recommendations that are, however, less novel than the mere act of assembling good data.

The data grew out of observation of the OPI output during three periods in 1968 and out of a simultaneous count of what appeared in the press, radio, and television in 50 countries. The output of 54 correspondents accredited to the United Nations in New York also was monitored. This produced 83,420 information items which, classified and coded, were punched onto 96,094 cards to form a unique data archive. The observation did not go beyond the news media; that is, it excluded opinion journals, specialist publications and other means of information distribution, e.g., films for theaters. The observed material is presented by Szalai in 17 tables and six diagrams keyed to the text and in detailed appendices. A useful bibliography, which incidentally discloses the remarkable paucity of quantitative material on an eminently quantifiable subject, is included.

The observation periods were well chosen to generate data on the UN as it acted in several guises. The period from March 18–31, 1968, included discussions in the Security Council of the still hot Rhodesia case and of a flareup of fighting between Israel and Jordan, and the end of the second UNCTAD Conference in New Delhi. The second period, April 22–May 5, encompassed the International Conference on Human Rights at Teheran, a resumed session on the General Assembly on the nuclear nonproliferation treaty, and a relatively slight disturbance in Jerusalem, which resulted in a Security Council resolution. The final period, November 11–24, fell in the middle of the annual General Assembly, which then was busy with disarmament and Chinese representation. Meantime, away from New York, a UNESCO General Conference took place and the Intergovernmental Committee of the World Food Programme met.

All this activity would appear to furnish an ample supply to the journalistic grist mills. What came out of those mills, however, was both sparse and uneven, as Szalai's able, straightforward analysis of the data clearly shows. He documents the extent to which only crises catch the headlines. Even in the elite press, that handful of self-consciously high-quality newspapers, such as *The New York Times* or *Le Monde*, the accent remains on political events, although they carry material that appeals to the readers with sophisticated knowledge of economics and finance. In the highly developed countries, the press carries fewer and shorter items on economic development and other long-term programs than in the less developed countries, but the latter papers reach far fewer readers, either in numbers or in proportion of the population. Radio concentrates even more on the hot story than the press, which has a broader range of coverage. Television plays the crises, especially the ones that lend themselves to visual images, still harder. Most of the news media in the world depend on the five worldwide wire services for their stories: these services must try to serve clients everywhere and can scarcely concentrate on regional or national material, even though such articles obviously get better local play than do more general treatments. Even the prestige and elite press that maintains accredited correspondents in New York tends to intensify coverage only of crisis, and delivers minuscule amounts of news on slowly maturing activities, such as economic development. The output of OPI, which increases rapidly during the General Assembly, appears to have practically no effect on what goes through the media; a principal function of OPI can be found in informing delegations and the secretariat of the activities of the organization.

Szalai's recommendations have a more tentative and slighter character than his description and analysis. He points out as a starter the vagueness of the instructions given OPI during the first General Assembly and suggests a program that depends on targeting. Nevertheless, much of the study implies that spreading the news really remains outside of the UN bailiwick and that any possible improvements would have a rather slight effect in a world still organized around national governments. He—or perhaps rather observers quite unconnected with the UN system—might well have given more attention to the hypothesis that to try to promote the popularity of the United Nations misses the point: the real task may be to get the appropriate information to the relevant specialized publics at the useful moment.

LEON GORDENKER
Center of International Studies
Princeton University

Greece and the Entente: August 1, 1914–September 25, 1916. By Christos Theodoulou. (Thessaloniki: Institute for Balkan Studies, 1971. Pp. 379. No price indicated.)

Christos Theodoulou focuses in his book on the origins of a controversy which for almost two generations split the Greek nation into two irreconcilable camps. In March 1915, Eleutherios Venizelos resigned as Premier of Greece in a disagreement with King Constantine over the country's entry into the war on the side of the *Entente*—and this set the stage for a disastrous feud. The book is in effect a collection of telegrams, memoranda, ambassadorial reports, and other diplomatic papers. Since the documents are given for the most part in the original language—French or English—a reader must be versed in both languages, otherwise a major part of the book will remain beyond comprehension. I am inclined to think that to many this bilingual presentation will prove a serious handicap. Nonetheless, the assembled documents do throw some new light on the controversy. Venizelos, convinced of Allied victory, was afraid that the war might end too soon, before he had time to join in and share in the fruits of victory. By contrast, Constantine reasoned that should the war end quickly, the Central Powers had a better chance to emerge if not totally victorious at least in a strong bargaining position. Some readers may be surprised to see in the documents that Venizelos was willing to cede Kavala to Bulgaria in exchange for a promise that after the war Greece would be given the Greek-inhabited lands in Asia Minor.

The origins of the feud can be traced to the disagreement between Constantine and Venizelos over Greek participation in the ill-fated operation at Gallipoli. It received fresh impetus when the Allies following their Gallipoli failure asked Venizelos to allow the landing of allied troops in Salonika. The documents quoted by Theodoulou indicate that the King was willing to agree provided the allies furnished adequate military forces (p. 178) and the landing was made to appear as a violation of Greek neutrality for appearances' sake. The affair was mishandled by the British and the landing further strained the relations between Venizelos and Constantine. In the period following Venizelos' resignation in October 1915, and the emergence of his revolutionary government in Salonika in September 1916, the record minutely quoted by the author bears out his general observation that "personal ambition and hatred in the end took the place of patriotism" allowing the *Entente* powers to engage in a "really scandalous treatment of Greece . . ." (p. 190).

Theodoulou has indeed performed a valuable service by bringing together in one volume the original documents which tell the story without partisan distortion or belated embellishments or rationalizations. The author is very frugal with his commentary letting the documents speak for themselves. This is both a blessing and a drawback because the reader must plow through the record with little guidance and must form his own conclusions with little help from the author. Still, the evidence is there for those who are interested in a crucial period of modern Greek history.

D. GEORGE KOUSOULAS
Howard University

Prospects in the Pacific. Edited with an Introduction by Richard L. Walker. (Washington, D.C.: Heldref Publications, 1972. Pp. 212. $8.50.)

The essays collected in the book are based on papers presented at a conference held at the Airlie Institute for International Studies in May, 1970. Participants came from several countries and were drawn from academic, governmental and economic organizations. The editor is Director of the Institute of International Studies, the University of South Carolina, one of the six sponsoring organizations.

The papers in Part One are concerned with problems of power and security. Soedjatmoko, at the time Indonesian ambassador to Washington, weighs the implications of the new multipolar situation. Is there an emerging power balance which can bring peace and a measure of stability to the Asian region? He is guardedly optimistic about the possibilities of neutralization of Southeast Asia. Except for China's concern with her borders, none of the major powers have vital interests in the region, and each is concerned to prevent dominance by any of the others. If the powers exercise sufficient restraint, if Japan does not go nuclear, if an effective regional organization materializes, he feels the balance can be stable.

Owen Harries, University of New South Wales, takes on the liberal critics of containment and American Asian policy. (Fulbright and Schlesinger are his most frequently cited targets.) These critics, he argues, have based their attack on highly dubious propositions: that Southeast Asia is not of major importance to the United States, and that China is not an expansionist power posing any aggressive threat. These and other assumptions he subjects to rigorous scrutiny and finds them wanting in consistency and realism.

Shifts in Australian foreign policy are analyzed by Bruce Grant of the University of Melbourne. The policy of "forward defense" based on alliances with Britain and the United States was designed to provide isolation from Asia. Develop-

ments in Vietnam and shifts in relationships among the powers were producing a reassessment even before the advent of the present Labor Government. The American alliance will remain, he predicts, but under conditions giving Australia greater independence.

The second part takes up problems of political and economic viability. Stephen Hosmer, RAND Corporation, assesses the prospects of guerrilla warfare in the 1970s. He concludes it will neither win nor lose. He expects existing regimes to survive but warns that the strengths of the guerrillas, including the advantages of contiguous sanctuary and external aid, mean they can be contained only by the "continued capacity and willingness to practice protracted defense" (p. 31).

Two important elements of such a capacity are effective administration and economic development. Dr. Hahn-Been Lee, of the East-West Center, finds the bureaucracies of developing Asian states inadequate for the current stage of development in which the socioeconomic problems of transformation predominate. He sees a need for more broadly recruited new talent, capable of operating technical and developmental programs.

Dr. Saburo Okita, well-known Japanese economist, discusses the economic problems of Southeast Asia, where development has lagged behind that of East Asia. He recommends policies designed to provide more employment in agriculture, reduced disparities in income, increased exports of manufactures to developed nations, increased foreign aid and private investment. His warning of the possible reaction to the growing weight of Japanese economic power appears prophetic in the light of Prime Minister Tanaka's recent stormy reception in Bangkok and Djakarta.

The major portion of the third part is an essay by Mochtar Lubis, Indonesian editor and publisher, which is primarily concerned with regional cooperation. Among the obstacles he cites, UN agencies, especially ECAFE which he would abolish, come in for the sharpest criticism. Functional organizations, and especially, informal and unofficial channels are more effective instrumentalities in his view.

Readers approach collections of conference papers with justifiable apprehension. The material is frequently diffuse, the quality, uneven. The present volume does not entirely surmount the difficulties. Shifts in perspective from bird's eye to worm's eye can produce a disjointed view of the landscape. Substance and organization are not of uniform quality. (For the hurried reader, this reviewer would recommend Part One plus the Okita article.) Yet, the editor is correct in saying that there are certain recurring questions which provide an element of continuity, and there is a remarkable amount of agreement on major points: prospects for the neutralization of Southeast Asia are reasonably encouraging; China poses some dangers, but even the wary eye of Mr. Harries foresees no Armageddon; Japan's economic strength is increasing her influence, but for her to develop a nuclear force would be a serious mistake; regional cooperation still has a long way to go before becoming more than a "beautiful slogan" (Lubis, p. 187). One concern that is voiced again and again: will America's reassessment of her global interests lead to disengagement from Asia in such a fashion as to upset the emerging balance and compound the region's instabilities? Is the American deterrent still credible? A continuing commitment is assumed, but merely to pose the question is to suggest a growing doubt.

The book deserves one final word of praise: it has stood the test of time remarkably well. The events in the interval since the conference, e.g., the Peking visit, the Vietnam cease-fire, have underlined the enduring nature of the concerns raised, and strengthened rather than diminished the force of many of the arguments.

WILLARD H. ELSBREE
Ohio University

World War II: Books in English, 1945–65. Compiled by Janet Ziegler. (Stanford, Calif.: Hoover Institution Press, 1971. Pp. 223. $10.50.)

This bibliography distributes its 4,519 titles across eight major categories, each with two to three further sublevels. This yields the first nearly comprehensive checklist of its subject, if one discounts the many editing errors, debatable categorizations, exclusion of all 1939–1944 publications, and spotty inclusion of major memoirs (Lady Norwich in; her husband, Duff Cooper, out).

Ziegler's introduction and 179-item list of specialized bibliographies and other reference books constitute a valuable guide to the literature. However, denied annotation, the user is unwarned about abridgments (of translations and reprints) or even outright forgeries (e.g., Krylov).

Given its fine tradition of bibliographic and documentary publication, Stanford's Hoover Institution should have required higher standards in lending its imprimatur to this thin by-product of UCLA's Graduate School of Library Service. Even brief annotation and, say, library call-numbers would have made this work of much wider practical value to the scholars it claims to serve.

BARTON WHALEY
Boston University

INDEX TO VOLUME LXVIII

Abbott, Simon, ed. *The Prevention of Racial Discrimination in Britain.* Reviewed by Richard P. Longaker. 1783

Abendroth, Wolfgang. *A Short History of the European Working Class.* Nicholas Jacobs, Brian Trench, and Joris de Bres, translators. Reviewed by Graham Wootton. 797

The ABM Issue in the Senate, 1968–1970: The Importance of Ideology. Robert A. Bernstein and William W. Anthony. 1198

Abrams, Charles. *The Language of Cities: A Glossary of Terms.* Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696

Abramson, Paul R. *Generational Change in American Electoral Behavior.* 93

Achen, Christopher H. *Communication* on Richard S. Katz, "The Attribution of Variance in Electoral Returns: An Alternative Measurement Technique" and Donald E. Stokes, "Comment" (September, 1973). 1272

Adamany, David. Review of Harold W. Chase, *Federal Judges: The Appointing Process.* 275

Adamec, Ludwig. Review of Richard S. Newell, *The Politics of Afghanistan.* 834

Adam, Heribert, ed. *South Africa: Sociological Perspectives.* Reviewed by Edwin S. Munger. . 1785

Adams, Arthur E. and Jan S. Adams. *Men Versus Systems: Agriculture in the USSR, Poland, and Czechoslovakia.* Reviewed by Sidney Ploss. 798

Aden and British Strategy, 1839–1968. Harvey Sicherman. Reviewed by Aaron S. Klieman. . 1877

Adloff, Richard and Virginia Thompson. *The French Pacific Islands: French Polynesia and New Caledonia.* Reviewed by Francis West. . 1843

Administration and Development in Malaysia: Institution Building and Reform in a Plural Society. Milton Esman. Reviewed by Lucian W. Pye. 1352

Administration: The Word and the Science. Andrew Dunsire. Reviewed by Robert T. Golembiewski. 1303

Adorno, Theodor W. and Max Horkheimer. *Dialectic of Enlightenment.* John Cumming, trans. Reviewed by Hiram Caton. 1307

Aerial Piracy and International Law. Edward McWhinney. Reviewed by D. H. N. Johnson . 355

Afanasyev, V. G. *The Scientific Management of Society.* Reviewed by Arthur Trueger. 253

Aging and Society, 3 vols. Matilda White Riley et al. Reviewed by Frederick R. Eisele. 262

Agor, Weston H. Review of Regis Debray, *The Chilean Revolution: Conversations with Allende.* 812

Agrarian Revolt in a Mexican Village. Paul Friedrich. Reviewed by Lola Romanucci-Ross. 1805

Aguilar, Luis E. *Cuba 1933, Prologue to Revolution.* Reviewed by Manuela Semidei. 335

Aid to Russia, 1941–1946: Strategy, Diplomacy, the Origins of the Cold War. George C. Herring, Jr. Reviewed by Richard H. Heindel. 867

Aitkin, Don. *The Country Party in New South Wales: A Study of Organisation and Survival.* Reviewed by D. W. Rawson. 1786

Alfange, Dean Jr. Review of George Anastaplo, *The Constitutionalist: Notes on the First Amendment.* 774

Alexander, Robert J. Review of James M. Malloy, *Bolivia: The Uncompleted Revolution.* 1825

Allen, Ken. *Communication* on Peter Eisinger, "The Conditions of Protest Behavior in American Cities" (March, 1973). 1273

Allerbeck, Klaus R. and Leopold Rosenmayr. *Aufstand der Jugend? Neue Aspekte der Jugendsoziologie.* Reviewed by Peter H. Merkl. 1788

The All-Volunteer Army: An Analysis of Demand and Supply. K. H. Kim, Susan Farrell, and Ewan Clague. Reviewed by Ryan C. Amacher and Robert D. Tollison. 787

The Almanac of World Military Power. T. N. Dupuy and Wendell Blanchard, eds. Reviewed by James A. Huston. 862

Amacher, Ryan C. and Robert D. Tollison. Review of K. H. Kim, Susan Farrell, and Ewan Clague, *The All-Volunteer Army: An Analysis of Demand and Supply;* and Harry A. Marmion, *The Case Against a Volunteer Army.* .. 787

The American Bibliography of Russian and East European Studies for 1966. Fritz T. Epstein, ed.; Penelope H. Carson and Michael E. Shaw, associate eds. Reviewed by John A. Armstrong. 299

American Criminal Justice: The Defendant's Perspective. Jonathan D. Casper. Reviewed by Ruth G. Weintraub. 1324

The American Mail: Enlarger of the Common Life. Wayne E. Fuller. Reviewed by Paul T. David. 278

An American Philosophy of Social Security: Evaluation and Issues. J. Douglas Brown. Reviewed by Louis L. Friedland. 777

The American Political Dictionary, 3rd ed. Jack C. Plano and Milton Greenberg. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696

American Religious Groups View Foreign Policy: Trends in Rank-and-File Opinion, 1937–1969. Alfred O. Hero, Jr. Reviewed by Kenneth W. Thompson. 1863

The American Revolution and the French Alliance. William C. Stinchcombe. Reviewed by Alexander DeConde. 1881

American Roulette: The History and Dilemma of the Vice Presidency, rev. ed. Donald Young. Reviewed by Robert S. Ross. 796
Americans and Chinese Communists, 1927–1945: A Persuading Encounter. Kenneth E. Shewmaker. Reviewed by Allen S. Whiting. 1853
America's Outward Thrust: Approaches To Foreign Affairs, 1865–1890. Milton Plesur. Reviewed by Donald M. Borock. 875
America and the World Political Economy. David P. Calleo and Benjamin M. Rowland. Reviewed by Ole R. Holsti. 1385
Anarchism Today. David E. Apter and James Joll, eds. Reviewed by Stanley Rothman. ... 1346
Anastaplo, George. *The Constitutionalist: Notes on the First Amendment.* Reviewed by Dean Alfange, Jr. 774
The Anatomy of Influence: Decision-Making in International Organization. Robert W. Cox, Harold K. Jacobson, Gerard and Victoria Curzon, Joseph S. Nye, Lawrence Scheinman, James P. Sewell, and Susan Strange. Reviewed by H. Field Haviland. 1389
Anderson, Stanley V. and John E. Moore, eds. *Establishing Ombudsman Offices: Recent Experience in the United States.* Transcript of the Ombudsman WORKSHOP, Honolulu, Hawaii, May 5–7, 1971. Reviewed by William B. Gwyn. 1752
Andriot, John L. *Guide to U.S. Publications.* Reviewed by John Brown Mason. 1753
Angell, Alan. *Politics and the Labour Movement in Chile.* Reviewed by Federico G. Gil. 800
Review of James Petras and Hugo Zemelman Merino, *Peasants in Revolt: A Chilean Case Study, 1965–1971.* 1832
Annotated Bibliography of Bibliographies on Selected Government Publications and Supplementary Guides to the Superintendent of Documents Classification System. Alexander C. Body. Reviewed by John Brown Mason. ... 1753
Another Look at the Twentieth-Century South. George E. Mowry. Reviewed by Charles S. Bullock, III. 1334
Ansel, Walter. *Hitler and The Middle Sea.* Reviewed by Paul L. Savage. 802
An, Tai Sung. *Mao Tse-tung's Cultural Revolution.* Reviewed by C. P. FitzGerald. 799
Anthony, William W. and Robert A. Bernstein. *The ABM Issue in the Senate, 1968–1970: The Importance of Ideology.* 1198
Antonio Gramsci: Life of a Revolutionary. Giuseppe Fiori. Reviewed by Dante Germino. 1305
Antoun, Richard and Iliya Harik, eds. *Rural Politics and Social Change in the Middle East.* Reviewed by Douglas E. Ashford. 1345
APSA Presidential Address, 1973: *Culture and the Comparative Study of Politics, or The Constipated Dialectic.* Robert E. Ward. 190
APSA Presidential Address, 1973, Introduction to. Lucian W. Pye. 188
Apter, David E. and James Joll, eds. *Anarchism Today.* Reviewed by Stanley Rothman. 1346
Arab Contemporaries: The Role of Personalities in Politics. Majid Khadduri. Reviewed by Iliya Harik. 1818
The Arabian Peninsula: Society and Politics. Derek Hopwood, ed. Reviewed by John G. Merriam. 1359
Aranson, Peter H., Melvin J. Hinich, and Peter C. Ordeshook. *Election Goals and Strategies: Equivalent and Nonequivalent Candidate Objectives.* 135
Arkes, Hadley. *Bureaucracy, the Marshall Plan, and the National Interest.* Reviewed by Thomas R. Wolanin. 1848
Armed and Alone: The American Security Dilemma. Dean William Rudoy. Reviewed by Thomas J. Volgy. 360
The Armed Services and Society, J. N. Wolfe and John Erickson, eds. Reviewed by William F. Levantrosser. 1758
Armer, Michael. Review of William M. O'Barr, David H. Spain, and Mark A. Tessler, eds., *Survey Research in Africa: Its Applications and Limits.* 835
Armstrong, John A. Review of Fritz T. Epstein, ed., *The American Bibliography of Russian and East European Studies for 1966.* Penelope H. Carson and Michael E. Shaw, associate eds. . 299
Review of Richard H. Marshall, Jr., ed., *Aspects of Religion in the Soviet Union, 1917–1967.* Thomas E. Bird and Andrew Q. Blane, assoc. eds. 1827
Arms, Yen and Power: The Japanese Dilemma. John K. Emmerson. Reviewed by Paul F. Langer. 863
Arnold, Joseph L. *The New Deal in the Suburbs: A History of the Greenbelt Towns.* Reviewed by Hugh A. Bone. 776
Arraes, Miguel. *Brazil: The People and the Power.* Lancelot Sheppard, trans. With an introduction by Yves Goussault. Reviewed by Ronald H. Chilcote. 1789
The Art and Practice of Diplomacy: A Selected and Annotated Guide. Robert B. Harmon. Reviewed by Elmer Plischke. 347
Ashcraft, Richard. Review of Irving Louis Horowitz, *Foundations of Political Sociology.* . 761
Asher, Robert E. Review of Gerald M. Meier, ed., *International Economic Reform: Collected Papers of Emile Despres.* 1403
Ashford, Douglas E. Review of Richard Antoun and Iliya Harik, eds., *Rural Politics and Social Change in the Middle East.* 1345
Asian Development: Problems and Prognosis. John Badgley. Reviewed by Khalid B. Sayeed. 1347
Aspects of Religion in the Soviet Union, 1917–

1967. Richard H. Marshall, Jr., ed. Thomas E. Bird and Andrew Q. Blane, assoc. eds. Reviewed by John A. Armstrong. 1827
Atkins, G. Pope. Review of Malcolm T. Walker, *Politics and the Power Structure: A Rural Community in the Dominican Republic.* 1846
Aufstand der Jugend? Neue Aspekte der Jugendsoziologie. Klaus R. Allerbeck and Leopold Rosenmayr. Reviewed by Peter H. Merkl. .. 1788
Australia in New Guinea, 2nd ed. L. B. Mair. Reviewed by C. D. Rowley. 828
Australia in the Nuclear Age: National Defence and National Development. Ian Bellany. Reviewed by Harry G. Gelber. 1792
Australia and Papua New Guinea. W. J. Hudson, ed. Reviewed by Francis West. 308
Austrian Neutrality in Postwar Europe: The Domestic Roots of a Foreign Policy. Thomas O. Schlesinger. Reviewed by Konrad Ginther. . 881
Aveni, Adrian F., D. Woods Thomas, Harry R. Potter, and William L. Miller, eds. *Institution Building: A Model for Applied Social Change.* Reviewed by Seymour B. Sarason. 268
Axelrod, Robert. *Communication* on the 1972 presidential election. 717
Ayres, Robert L. Review of Robert R. Kaufman, *The Politics of Land Reform in Chile.* 313

Bacchus, William I. *Diplomacy for the 70s: An Afterview and Appraisal.* Review of U.S. Department of State, *Diplomacy for the 70s: A Program of Management Reform for the Department of State.* 736
Bacciocco, Edward J., Jr. Review of Kirkpatrick Sale, *S.D.S.* 1338
Bachman, Jerald G. et al. *Youth in Transition:* Jerald G. Bachman, Robert L. Kahn, Martha T. Mednick, Terrence N. Davidson, and Lloyd D. Johnston. Vol. I: *Blueprint for a Longitudinal Study of Adolescent Boys;* Jerald G. Bachman. Vol. II: *The Impact of Family Background and Intelligence on Tenth-Grade Boys;* Jerome Johnston and Jerald G. Bachman. Vol. V: *Young Men and Military Service.* Jerald G. Bachman and Elizabeth Van Duinen. *Youth Looks at National Problems: A Special Report from the Youth in Transition Project.* Reviewed by Jarol B. Manheim. ... 1296
Bachman, Jerald G. and Jerome Johnston. *Young Men Look at Military Service: A Preliminary Report.* Reviewed by James M. Gerhardt. 1768
Badgley, John. *Asian Development: Problems and Prognosis.* Reviewed by Khalid B. Sayeed. 1347
Baehr, Peter. *The Role of a National Delegation in the General Assembly.* Reviewed by John O. Lindell. 1849
Bagehot, Walter. *The English Constitution.* With an introduction by R. H. S. Crossman, M.P. Reviewed by Samuel Beer in *Tradition and Nationality: A Classic Revisited.* 1292
Bailey, F. G., ed. *Gifts and Poison: The Politics of Reputation.* Reviewed by Lewis A. Dexter. . 254
Baines, John M. *Revolution in Peru: Mariátegui and the Myth.* Reviewed by Harry Kantor. 1348
Baker, John H. Review of Frederick M. Wirt, Benjamin Walter, Francine Rabinovitz, and Deborah Hensler, *On the City's Rim: Politics and Policy in Suburbia.* 291
Balassa, Bela. Review of Gustav Ranis, ed., *Government and Economic Development.* 1834
Baloyra, Enrique. Review of Gerrit Huizer, *The Revolutionary Potential of Peasants in Latin America.* 1812
Banfield, Edward C. and James Q. Wilson. *Communication* in reply to criticism of their article "Political Ethos Revisited" (December, 1971). 1271
Banks, Arthur S., assembler. *Cross-Polity Time-Series Data.* Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* 243
Banning, Lance. Review of Pauline Maier, *From Resistance to Revolution: Colonial Radicals and the Development of American Opposition to Britain, 1765–1776.* 284
Banton, Michael. *Racial Minorities.* Reviewed by Jonathan Pool. 1791
Barber, James Alden Jr. *Social Mobility and Voting Behavior.* Reviewed by Jerrold G. Rusk. 1754
Barkun, Michael and Wesley L. Gould. *Social Science Literature: A Bibliography for International Law.* Reviewed by Anthony D'Amato. 1394
Barnett, Correlli. *The Collapse of British Power.* Reviewed by F. S. Northedge. 1349
Barnett, Marguerite Ross. Review of Richard G. Fox, *Kin, Clan, Raja and Rule: State-Hinterland Relations in Pre-Industrial India.* 817
Barriers to Progress in South Vietnam: The United States Experience. Edward Reynolds Wright. Reviewed by William W. Boyer. ... 1411
Barry, Brian. Review of Thomas Hobbes, *Man and Citizen.* With an introduction by Bernard Gert, ed. 759
Bartholomew, Paul C. *The Irish Judiciary.* Reviewed by John H. Whyte. 803
Bases of Budgetary Incrementalism. John Wanat. 1221
The Battle Stalin Lost: Memoirs of Yugoslavia, 1948–1953. Vladimir Dedijer. Reviewed by Milan J. Reban. 298
Bauman, Zygmunt. Review of Paul Hollander, *Soviet and American Society: A Comparison.* . 1811
Becker, Theodore L. *Comparative Judicial Politics.* Reviewed by Herbert Jacob. 292
Beck, Paul Allen. *Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior.* 1229

Bedeski, Robert E. Review of Chün-tu Hsüeh, ed., *Revolutionary Leaders of Modern China*.. 307
Beer, Samuel. *Tradition and Nationality: A Classic Revisited.* Review of Walter Bagehot, *The English Constitution.* With an introduction by R. H. S. Crossman, M.P. 1292
Behavioural Worlds: The Study of Single Cases. P. G. Herbst. Reviewed by Steven Doehrman. 758
Behrman, Jack N. *U.S. International Business and Governments;* and *National Interests and the Multinational Enterprise; Tensions Among the North Atlantic Countries.* Reviewed by Vincent J. Fuccillo. 855
Beitzinger, A. J. Review of Leonard Gilhooley, *Contradiction and Dilemma: Orestes Brownson and the American Idea.* 1735
Belaief, Gail. *Spinoza's Philosophy of Law.* Reviewed by Douglas Sturm. 1724
Bellany, Ian. *Australia in the Nuclear Age: National Defence and National Development.* Reviewed by Harry G. Gelber. 1792
Bell Sidney. *Righteous Conquest: Woodrow Wilson and the Evolution of the New Diplomacy.* Reviewed by Milton Plesur. 1755
Bell, Wendell. Review of A. N. R. Robinson, *The Mechanics of Independence: Patterns of Political and Economic Transformation in Trinidad and Tobago.* 326
Beloff, Max. Review of George Alexander Lensen, *The Strange Neutrality: Soviet-Japanese Relations during the Second World War, 1941–1945.* 354
Bender, Gerald J. Review of Ronald H. Chilcote, *Emerging Nationalism in Portuguese Africa: Documents.* 296
Bennett, Gordon A. Review of Scott A. Boorman, *The Protracted Game: A Wei-ch'i Interpretation of Maoist Revolutionary Strategy.* 805
Bennett, Stephen E. and Abraham H. Miller. *Communication* on James Q. Wilson and Edward C. Banfield, "Political Ethos Revisited" (December, 1971). 1265
The Berlin Crisis of 1961: Soviet-American Relations and the Struggle for Power in the Kremlin, June-November 1961. Robert M. Slusser. Reviewed by Werner J. Feld. 1410
Berman, Marshall. *The Politics of Authenticity: Radical Individualism and the Emergence of Modern Society.* Reviewed by Allan Bloom. . 1297
Bernholz, Peter. *Communication* on William H. Riker and Steven J. Brams, "The Paradox of Vote Trading" (December, 1973). 1689
Bernstein, Robert A. and William W. Anthony. *The ABM Issue in the Senate, 1968–1970: The Importance of Ideology.* 1198
Berque, Jacques. *Egypt: Imperialism and Revolution.* Reviewed by John P. Entelis. 293
Beth, Richard S. *Communication* on Peter J. Taylor, "A New Shape Measure for Evaluating Electoral District Patterns" (September, 1973). 1275
A Bibliography of German Studies, 1945–1971. Gisela Hersch. Reviewed by Manfred Henningsen. 305
Bickel, Alexander M. *Reform and Continuity: The Electoral College, the Convention, and the Party System.* Reviewed by Charles Press. .. 1756
Biddle, W. F. *Weapons Technology and Arms Control.* Reviewed by Bernard Brodie. 1382
Bill, James Alban. *The Politics of Iran: Groups, Classes and Modernization.* Reviewed by Sepehr Zabih. 1793
Bindman, Geoffrey and Anthony Lester. *Race and Law in Great Britain.* Reviewed by Richard P. Longaker. 1783
Birnbaum, Norman. *Toward a Critical Sociology.* Reviewed by Guenther Roth. 1724
The Birth of Mass Political Parties: Michigan, 1827–1861. Ronald P. Formisano. Reviewed by Roger E. Wyman. 782
Bismarck and the French Nation, 1848–1890. Allan Mitchell. Reviewed by Otto Pflanze. .. 874
Black Africa: A Comparative Handbook. Donald G. Morrison, Robert C. Mitchell, John N. Paden, Hugh M. Stevenson, and others. Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* 243
Black, Gordon S. *Conflict in the Community: A Theory of the Effects of Community Size.* ... 1245
Black Political Parties: An Historical and Political Analysis. Hanes Walton, Jr. Reviewed by William R. Keech. 1341
Blanchard, Wendell and T. N. Dupuy, eds. *The Almanac of World Military Power.* Reviewed by James A. Huston. 862
Blank, Stephen. *Industry and Government in Britain: The Federation of British Industries in Politics, 1945–65.* Reviewed by Graham Wootton. 1794
Blechman, Barry M. and Alton H. Quanbeck. *Strategic Forces: Issues for the Mid-Seventies.* Reviewed by Herbert Scoville, Jr. 1405
Blechman, Barry M. Review of Stockholm International Peace Research Institute, *Resources Devoted to Military Research and Development, An International Comparison.* 1841
Blissett, Marlen. *Politics in Science.* Reviewed by Gene M. Lyons. 1726
Bloom, Allan. Review of Marshall Berman, *The Politics of Authenticity: Radical Individualism and the Emergence of Modern Society.* 1297
Bloomfield, Lincoln P. Review of W. W. Rostow, *The Diffusion of Power, 1957–1972: An Essay in Recent History.* 1874
Boaden, Noel. Review of Donald Foley, *Governing the London Region: Reorganization and Planning in the 1960s.* 816

Bober, Arie, ed. *The Other Israel: The Radical Case Against Zionism.* Reviewed by Allan E. Shapiro. 804
Body, Alexander C. *Annotated Bibliography of Bibliographies on Selected Government Publications and Supplementary Guides to the Superintendent of Documents Classification System.* Reviewed by John Brown Mason. 1753
Bogart, Leo, ed. *Social Research and the Desegregation of the U.S. Army.* Reviewed by William F. Levantrosser. 1758
Bognetti, Giovanni. Review of Denis Mack Smith, *Victor Emanuel, Cavour, and the Risorgimento.* 330
Böhme, Helmut, ed. *The Foundation of the German Empire: Select Documents.* Agatha Ramm, trans. Reviewed by Otto Pflanze. ... 295
Böhning, W. R. *The Migration of Workers in the United Kingdom and the European Community.* Reviewed by Dennis Brooks. 1851
Bolivia: *The Uncompleted Revolution.* James M. Malloy. Reviewed by Robert J. Alexander. .. 1825
Bolton, G. C. Review of J. A. La Nauze, *The Making of the Australian Constitution.* 315
Bonacich, Edna and Robert F. Goodman. *Deadlock in School Desegregation: A Case Study of Inglewood, California.* Reviewed by Leslie Lenkowsky. 272
Bone, Hugh A. Review of Joseph L. Arnold, *The New Deal in the Suburbs: A History of the Greenbelt Towns.* 776
Bonjean, Charles and Louis Schneider, eds. *The Idea of Culture in the Social Sciences.* Reviewed by Harry C. Triandis. 1318
Book Publishing in the USSR: *Reports of the Delegations of U.S. Book Publishers Visiting the USSR, October 21–November 4, 1970, August 20–September 17, 1962,* 2nd ed., enlarged. Reviewed by Patricia K. Grimsted. .. 1379
Boorman, Scott A. *The Protracted Game: A Wei-ch'i Interpretation of Maoist Revolutionary Strategy.* Reviewed by Gordon A. Bennett. 805
Borock, Donald M. Review of Milton Plesur, *America's Outward Thrust: Approaches to Foreign Affairs, 1865–1890.* 875
Bose, Tarun Chandra. *The Superpowers and the Middle East.* Reviewed by O. M. Smolansky. . 1382
Boskey, Bennett and Mason Willrich, eds. *Nuclear Proliferation: Prospects for Control.* Reviewed by Robert L. Rothstein. 336
Bourguina, Anna. *Russian Social Democracy; The Menshevik Movement: A Bibliography.* Reviewed by Stanley W. Page. 806
Bower, Robert T. *Television and the Public.* Reviewed by Paul Conway. 1322
Bowie, Norman E. *Towards a New Theory of Distributive Justice.* Reviewed by Edward J. Erler. 749
Boyce, D. G. *Englishmen and Irish Troubles: British Public Opinion and the Making of Irish Policy, 1918–1922.* Reviewed by Paul A. Pfretzschner. 295
Boyer, William W. Review of Edward Reynolds Wright, *Barriers to Progress in South Vietnam: The United States Experience.* 1411
Boyle, John H. Review of Hsu Long-hsuen and Chang Ming-kai, comps., *History of the Sino-Japanese War, 1937–1945.* 1869
Review of Robert Jungk, Ernan McMullin, Joseph Needham, Joan Robinson, Stuart Schram, William Sewell, and Bernard Towers, *China and the West: Mankind Evolving.* 1399
Bozeman, Adda B. *The Future of Law in a Multicultural World.* Reviewed by Wesley L. Gould. 338
The Bracero Program: Interest Groups and Foreign Policy. Richard B. Craig. Reviewed by Charles Ornelas. 858
Bradshaw, Kenneth and David Pring. *Parliament and Congress.* Reviewed by Anthony King. 807
Brady, David W. *Congressional Voting in a Partisan Era: A Study of the McKinley Houses and a Comparison to the Modern House of Representatives.* Reviewed by Charles M. Tidmarch. 1323
Brams, Steven J. and Morton D. Davis. *The 3/2's Rule in Presidential Campaigning.* 113
Brams, Steven J. and William H. Riker. *Communications* in reply to comments on their article "The Paradox of Vote Trading" (December, 1973). 1688, 1692
Brandon, Henry. *The Retreat of American Power.* Reviewed by Frank Burd. 1383
Brazil: The People and the Power. Miguel Arraes. Lancelot Sheppard, trans. With an introduction by Yves Goussault. Reviewed by Ronald H. Chilcote. 1789
British and Soviet Politics: Legitimacy and Convergence. Jerome M. Gilison. Reviewed by A. H. Brown. 301
The British Year Book of International Law, 1970, Vol. 44. Sir Humphrey Waldock and R. Y. Jennings, eds. Reviewed by Oliver J. Lissitzyn. 887
Brock, William R. *The Evolution of American Democracy.* Reviewed by Richard P. McCormick. 273
Brodie, Bernard. Review of W. F. Biddle, *Weapons Technology and Arms Control.* 1382
Brogan, Hugh. *Tocqueville.* Reviewed by J. P. Mayer. 1299
Bromke, Adam. Review of Robin Alison Remington, *The Warsaw Pact: Case Studies in Communist Conflict Resolution.* 876
Brooks, Dennis. Review of W. R. Böhning, *The Migration of Workers in the United Kingdom and the European Community.* 1851

Brown, A. H. Review of Jerome M. Gilison, *British and Soviet Politics: Legitimacy and Convergence*. ... 301
Brown, Frederic J. *Chemical Warfare: A Study in Restraints*. Reviewed by George H. Quester in *Chemical and Biological Warfare*. ... 1285
Brown, J. Douglas. *An American Philosophy of Social Security: Evaluation and Issues*. Reviewed by Louis L. Friedland. ... 777
Brown, Stuart Gerry. *The Presidency on Trial: Robert Kennedy's 1968 Campaign and Afterwards*. Reviewed by Fred I. Greenstein. ... 779
Brucan, Silviu. *The Dissolution of Power: A Sociology of International Relations and Politics*. Reviewed by Robert L. Farlow. ... 339
Bruell, Christopher. *Communication* in reply to comment on his article "Thucydides' View of Athenian Imperialism" (March, 1974). ... 1680
Thucydides' View of Athenian Imperialism. ... 11
Brundtland, Arne O. and August Schou, eds. *Small States in International Relations, Nobel Symposium 17*. Reviewed by Nils Ørvik. ... 1875
Bruun, H. H. *Science, Values and Politics in Max Weber's Methodology*. Reviewed by Jerome Gittleman. ... 1727
Buchan, Alastair. *Power and Equilibrium in the 1970s*. Reviewed by Robert L. Rothstein. ... 1852
Buchanan, A. Russell, ed. *The United States and World War II: Military and Diplomatic Documents*. Reviewed by Manfred Jonas. ... 780
Buell, Emmett H. Jr. Review of John C. Weistart, ed., *Community Economic Development*. . 288
Buel, Richard, Jr. *Securing the Revolution: Ideology in American Politics, 1789–1815*. Reviewed by Richard H. Kohn. ... 274
Bueno de Mesquita, Bruce. *Need for Achievement and Competitiveness as Determinants of Political Party Success in Elections and Coalitions*. ... 1207
Buhr, Walter. Review of Barbara Ward, J. D. Runnalls, and Lenore D'Anjou, eds., *The Widening Gap: Development in the 1970s*. ... 367
Bull, Hedley. Review of John Burton, *World Society*. ... 857
Review of *Problems of War and Peace: A Critical Analysis*. (Authors not given.) Bryan Bean, trans. ... 1850
Bullock, Charles S., III. Review of George E. Mowry, *Another Look at the Twentieth-Century South*. ... 1334
Bunker, Gerald E. *The Peace Conspiracy: Wang Ching-wei and the China War, 1937–1941*. Reviewed by James E. Sheridan. ... 1384
Bunyan, James. *The Origin of Forced Labor in the Soviet State, 1917–1921: Documents and Materials*. Reviewed by Leonard B. Schapiro. 296
Burd, Frank A. Review of Henry Brandon, *The Retreat of American Power*. ... 1383
Review of Randolph C. Downes, *The Rise of Warren Gamaliel Harding, 1865–1920*. ... 277
Bureaucracy, the Marshall Plan, and the National Interest. Hadley Arkes. Reviewed by Thomas R. Wolanin. ... 1848
Bureaucracy and Representative Government. William A. Niskanen, Jr. Reviewed by William C. Mitchell. ... 1775
Burma and Pakistan: A Comparative Study of Development. Mya Maung. Reviewed by Frank N. Trager. ... 1828
Burnham, Walter Dean. *Communication* on Edward Tufte, "The Relationship Between Seats and Votes in Two Party Systems" (June, 1973). ... 207
Rejoinder to "Comments" by Philip Converse and Jerrold Rusk. ... 1050
Theory and Voting Research: Some Reflections on Converse's "Change in the American Electorate." ... 1002
Burns, A. L. Review of J. R. Seeley, *The Expansion of England*. With an introduction by John Gross, ed. ... 844
Burns, R. M., ed. *One Country or Two?* Reviewed by Peter Regenstreif. ... 808
Burrell, R. M. Review of Rouhollah K. Ramazani, *The Persian Gulf: Iran's Role*. ... 1873
Burton, John. *World Society*. Reviewed by Hedley Bull. ... 857
Butler, W. E. Review of Samuel Kucherov, *The Organs of Soviet Administration of Justice: Their History and Operation*. ... 827
Butterworth, Charles E. Review of Ralph Lerner and Muhsin Mahdi, eds., *Medieval Political Philosophy*. ... 765
Review of Robert L. Hoffman, *Revolutionary Justice: The Social and Political Theory of P.-J. Proudhon;* and P.-J. Proudhon, *General Idea of the Revolution in the 19th Century*. .. 1737
Butterworth, Robert Lyle. *Comment on Shepsle's "On the Size of Winning Coalitions."* ... 519

The Caciques: Oligarchical Politics and the System of Caciquismo in the Luso-Hispanic World. Robert Kern, ed. Reviewed by Douglas A. Chalmers. ... 1362
Cahn, Anne H. *Communication* in reply to criticism of her review of Roy E. Licklider, *The Private Nuclear Strategists* (December, 1973). 1274
Cahnmann, Werner J. and Rudolf Heberle, eds. *Ferdinand Toennies on Sociology: Pure, Applied and Empirical. Selected Writings*. Reviewed by Donald A. Nielsen. ... 1728
Caldwell, Oliver J. *A Secret War: Americans in China, 1944–1945*. Reviewed by Allen S. Whiting. ... 1853
Calleo, David P. and Benjamin M. Rowland. *America and the World Political Economy*. Reviewed by Ole R. Holsti. ... 1385

Calleo, David P. Review of Leon N. Lindberg and Stuart A. Scheingold, eds., *Regional Integration: Theory and Research.* 1867
Campaign Craftsmanship. Edward Schwartzman. Reviewed by Max M. Kampelman. 784
Campbell, Blair. *Helvétius and the Roots of the "Closed" Society.* 153
Campbell, Ernest Q., ed. *Racial Tensions and National Identity. Proceedings of the Second Annual Vanderbilt Sociology Conference, November 4–6, 1970.* Reviewed by Jonathan Pool. 1791
Caplan, Lionel. *Land and Social Change in East Nepal: A Study of Hindu-Tribal Relations.* Reviewed by Louis Dupree. 809
Cappelletti, Mauro and Denis Tallon, eds. *Fundamental Guarantees of the Parties in Civil Litigation: Studies in National, International and Comparative Law.* Reviewed by Richard Pierre Claude. 1386
Carmines, Edward G. *The Mediating Influence of State Legislatures on the Linkage Between Interparty Competition and Welfare Policies.* . 1118
Carnegie Endowment for International Peace. *The Control of Chemical and Biological Weapons.* Reviewed by George H. Quester in *Chemical and Biological Warfare.* 1285
Carney, Thomas F. *Content Analysis: A Technique for Systematic Inference from Communications.* Reviewed by Dina A. Zinnes. 750
Carson, Penelope H. and Michael E. Shaw, associate eds. *The American Bibliography of Russian and East European Studies for 1966.* Fritz T. Epstein, ed. Reviewed by John A. Armstrong. 299
The Case Against a Volunteer Army. Harry A. Marmion. Reviewed by Ryan C. Amacher and Robert D. Tollison. 787
Casper, Jonathan D. *American Criminal Justice: The Defendant's Perspective.* Reviewed by Ruth G. Weintraub. 1324
Caton, Hiram. Review of Max Horkheimer and Theodor W. Adorno, *Dialectic of Enlightenment.* 1307
Cavala, William. *Changing the Rules Changes the Game: Party Reform and the 1972 California Delegation to the Democratic National Convention.* 27
CBW. Steven Rose, ed. Reviewed by George H. Quester in *Chemical and Biological Warfare.* . 1285
Chace, James. *A World Elsewhere: The New American Foreign Policy.* Reviewed by Hilliard A. Gardiner. 1387
Chadwick-Brown, David. Review of Otis H. Stephens, Jr., *The Supreme Court and Confessions of Guilt.* 1780
Chairman or Chief? The Role of Taoiseach in Irish Government. Brian Farrell. Reviewed by John H. Whyte. 803
Chalmers, Douglas A. Review of Robert Conrad, *The Destruction of Brazilian Slavery, 1850–1888.* 1799
Review of Robert Kern, ed., *The Caciques: Oligarchical Politics and the System of Caciquismo in the Luso-Hispanic World.* 1362
Chamberlin, John. *Provision of Collective Goods As a Function of Group Size.* 707
The Changing Balance of Power in the Persian Gulf: An International Seminar Report. Elizabeth Monroe. Reviewed by John G. Merriam. 1359
The Changing Brahmans: Associations and Elites among the Kanya-Kubjas of North India. R. S. Khare. Reviewed by Roderick Church. 1819
The Changing Politics of the South. William C. Havard, ed. Reviewed by Alexander Heard. . 278
Changing the Rules Changes the Game: Party Reform and the 1972 California Delegation to the Democratic National Convention. William Cavala. 27
Chang Ming-kai and Hsu Long-hsuen, comps. *History of the Sino-Japanese War, 1937–1945.* Reviewed by John H. Boyle. 1869
Chapman, John W. and J. Roland Pennock, eds. *Political and Legal Obligation, Nomos XII, Yearbook of the American Society for Political and Legal Philosophy.* Reviewed by M. R. Kadish. 768
Chase, Harold W. *Federal Judges: The Appointing Process.* Reviewed by David Adamany. .. 275
Chemical and Biological Warfare. George H. Quester. Review of Frederic J. Brown, *Chemical Warfare: A Study in Restraints;* Carnegie Endowment for International Peace, *The Control of Chemical and Biological Weapons;* John Cookson and Judith Nottingham, *A Survey of Chemical and Biological Warfare;* J. B. Neilands, G. H. Orians, E. W. Pfeiffer, Alje Vennema, and Arthur H. Westing, *Harvest of Death;* Steven Rose, ed., *CBW;* Stockholm International Peace Research Institute, *The Problem of Chemical and Biological Warfare,* 6 vols. 1285
Chemical Warfare: A Study in Restraints. Frederic J. Brown. Reviewed by George H. Quester in *Chemical and Biological Warfare.* . 1285
Chesneaux, Jean, ed. *Popular Movements and Secret Societies in China, 1840–1950.* Reviewed by Daniel L. Overmyer. 1795
Chilcote, Ronald H. *Emerging Nationalism in Portuguese Africa: Documents.* Reviewed by Gerald J. Bender. 296
Review of Miguel Arraes, *Brazil: The People and the Power.* Lancelot Sheppard, trans. With an introduction by Yves Goussault. ... 1789
The Chilean Revolution: Conversations with Allende. Regis Debray. Reviewed by Weston H. Agor. 812

China and Africa, 1949–1970: The Foreign Policy of the People's Republic of China. Bruce D. Larkin. Reviewed by Harold C. Hinton. 353
China and Ourselves: Explorations and Revisions by a New Generation. Bruce Douglass and Ross Terrill, eds. Reviewed by Oliver M. Lee. 814
China and the Overseas Chinese: A Study of Peking's Changing Policy, 1949–1970. Stephen Fitzgerald. Reviewed by Robert O. Tilman. . 866
China and the West: Mankind Evolving. Robert Jungk, Ernan McMullin, Joseph Needham, Joan Robinson, Stuart Schram, William Sewell, and Bernard Towers. Reviewed by John H. Boyle. 1399
Chinese Education and Society: A Bibliographic Guide. Stewart E. Fraser and Kuang-liang Hsu. Reviewed by Merle Goldman. 1804
Chi, Wen-shun, comp. and ed. *Readings in the Chinese Communist Cultural Revolution.* Reviewed by Arthur Huck. 810
Cho, Chang-Hyun and Se-Jin Kim, eds. *Government and Politics of Korea.* Reviewed by Gregory Henderson. 825
Chodak, Szymon. Review of David Lane and George Kolankiewicz, eds., *Social Groups in Polish Society.* 1364
Chodorow, Stanley. *Christian Political Theory and Church Politics in the Mid-Twelfth Century: The Ecclesiology of Gratian's Decretum.* Reviewed by Ernest L. Fortin. 1300
Choose! Pierre Vallières. Reviewed by Peter Regenstreif. 808
Chrimes, S. B., C. D. Ross, and R. A. Griffiths, eds. *Fifteenth Century England, 1399–1509: Studies in Politics and Society.* Reviewed by Arthur Joseph Slavin. 1797
Christensen, Cheryl. Review of Alvin Z. Rubinstein and George Ginsburg, eds., *Soviet and American Policies in the United Nations.* 878
Christian Political Theory and Church Politics in the Mid-Twelfth Century: The Ecclesiology of Gratian's Decretum. Stanley Chodorow. Reviewed by Ernest L. Fortin. 1300
Christoph, James B. Review of R. D. Coates, *Teachers' Unions and Interest Group Politics: A Study in the Behaviour of Organised Teachers in England and Wales.* 1798
Chrypinski, V. C. Review of Joseph R. Fiszman, *Revolution and Tradition in People's Poland: Education and Socialization.* 1801
Chuang, Richard Y. *The International Air Transport Association.* Reviewed by Robert L. Thornton. 857
The Church as a Political Factor in Latin America: With Special Reference to Colombia and Chile. David E. Mutchler. Reviewed by Ivan Vallier. 832
The Churches and Politics in Germany. Frederic Spotts, Reviewed by Guenter Lewy. 1376
Church, Roderick. Review of R. S. Khare, *The Changing Brahmans: Associations and Elites among the Kanya-Kubjas of North India.* 1819
Citrin, Jack. *Comment: The Political Relevance of Trust in Government.* 973
City Politics and the Press: Journalists and the Governing of Merseyside. Harvey Cox and David Morgan. Reviewed by Morris Davis. . 1350
Civil Aviation Development: A Policy and Operating Analysis. Robert C. Fraser, Alan D. Donheiser, and Thomas G. Miller, Jr. Prepared for Arthur D. Little, Inc. Reviewed by Robert L. Thornton. 857
Clague, Ewan, K. H. Kim, and Susan Farrell. *The All-Volunteer Army: An Analysis of Demand and Supply.* Reviewed by Ryan C. Amacher and Robert D. Tollison. 787
Clarke, James W. and Henry C. Kenski. *Communication* on Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, "The Structuring Principle: Political Socialization and Belief Systems" (June, 1973). 722
Clark, R. H. Review of Virginia Van Der Veer Hamilton, *Hugo Black: The Alabama Years.* . 1767
Claude, Richard Pierre. Review of Mauro Cappelletti and Denis Tallon, eds., *Fundamental Guarantees of the Parties in Civil Litigation: Studies in National, International and Comparative Law.* 1386
Clayton, Elizabeth. Review of William J. Conyngham, *Industrial Management in the Soviet Union.* 1799
Clientelist Politics in the Philippines: Integration or Instability? Thomas C. Nowak and Kay A. Snyder. 1147
Coates, R. D. *Teachers' Unions and Interest Group Politics: A Study in the Behaviour of Organised Teachers in England and Wales.* Reviewed by James B. Christoph. 1798
Coffey, Joseph I. *Strategic Power and National Security.* Reviewed by Jerome H. Kahan. ... 340
Cohan, Al. *The Irish Political Elite.* Reviewed by John H. Whyte. 803
Cohen, Bernard C. Review of Robert L. Rothstein, *Planning Prediction, and Policymaking in Foreign Affairs.* 1407
Cohen, Harris S. Review of Walter A. Rosenbaum, *The Politics of Environmental Concern.* 1337
Cohen, Michael. Review of John C. Leggett, *Race, Class, and Political Consciousness.* 1770
Cohen, Warren I. Review of Akira Iriye, *Pacific Estrangement: Japanese and American Expansion, 1897–1911.* 350
Cohen, Wilbur J. and Milton Friedman. *Social Security: Universal or Selective?* Reviewed by Louis L. Friedland. 777
Cohn, Helen D. *Soviet Policy toward Black Africa: The Focus on National Integration.* Reviewed by Vernon McKay. 1388

Cohn, Jules. *The Conscience of the Corporations: Business and Urban Affairs, 1967–1970.* Reviewed by Joseph Smucker. 1760
Colcord, Frank, Alan Lupo, and Edmund P. Fowler. *Rites of Way: The Politics of Transportation in Boston and the U.S. City.* Reviewed by G. Ross Stephens. 282
Cole, David C. and Princeton N. Lyman. *Korean Development: The Interplay of Politics and Economics.* Reviewed by Gregory Henderson. 297
The Collapse of British Power. Correlli Barnett. Reviewed by F. S. Northedge. 1349
Collective Violence. J. F. Short, Jr. and M. E. Wolfgang, eds. Reviewed by N. Lucas. 1839
Colon, Frank T. Review of Harry L. Hopkins, *Spending to Save: The Complete Story of Relief.* 786
Colonial Sequence, 1930 to 1949: A Chronological Commentary upon British Colonial Policy, Especially in Africa; and *Colonial Sequence, 1949 to 1969: A Chronological Commentary upon British Colonial Policy in Africa,* 2 vols. Margery Perham. Reviewed by L. H. Gann. . 1870
Colton, Kenneth E. Review of Chalmers E. Johnson, *Conspiracy at Matsukawa.* 312
Comecon and the Politics of Integration. Henry W. Schaefer. Reviewed by Andrzej Korbonski. 880
Comment: The American Electoral Universe: Speculation and Evidence. Jerrold G. Rusk. .. 1028
Comment on Burnham's "Theory and Voting Research." Philip E. Converse. 1024
Comment on "Changing the Rules Changes the Game." Austin Ranney. 43
Comment: The Political Relevance of Trust in Government. Jack Citrin. 973
Comment on Shepsle's "On the Size of Winning Coalitions." Robert Lyle Butterworth. 519
Comment: The Status of Nonattitudes. Philip E. Converse. 650
Communication by Achen, Christopher H. on Richard S. Katz, "The Attribution of Variance in Electoral Returns: An Alternative Measurement Technique" and Donald E. Stokes, "Comment" (September, 1973). 1272
Communication by Allen, Ken on Peter Eisinger, "The Conditions of Protest Behavior in American Cities" (March, 1973). 1273
Communication by Axelrod, Robert on the 1972 presidential election. 717
Communication by Banfield, Edward C. and James Q. Wilson in reply to criticism of their article "Political Ethos Revisited" (December, 1971). 1271
Communication by Bennett, Stephen E. and Abraham H. Miller on James Q. Wilson and Edward C. Banfield, "Political Ethos Revisited" (December, 1971). 1265
Communication by Bernholz, Peter on William H. Riker and Steven J. Brams, "The Paradox of Vote Trading" (December, 1973). 1689
Communication by Beth, Richard S. on Peter J. Taylor, "A New Shape Measure for Evaluating Electoral District Patterns?" (September, 1973). 1275
Communications by Brams, Steven J. and William H. Riker in reply to comments on their article "The Paradox of Vote Trading" (December, 1973). 1688, 1692
Communication by Bruell, Christopher in reply to comment on his article "Thucydides' View of Athenian Imperialism" (March, 1974). ... 1680
Communication by Burnham, Walter Dean on Edward Tufte, "The Relationship Between Seats and Votes in Two Party Systems" (June, 1973). 207
Communication by Cahn, Anne H. in reply to criticism of her review of Roy E. Licklider, *The Private Nuclear Strategists* (December, 1973). 1274
Communication by Clarke, James W. and Henry C. Kenski on Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, "The Structuring Principle: Political Socialization and Belief Systems" (June, 1973). 722
Communication by Corsi, Jerome R. on Christopher Bruell, "Thucydides' View of Athenian Imperialism" (March, 1974). 1679
Communication by Deber, Raisa B. on Virginia Gray, "Innovation in the States: A Diffusion Study" (December, 1973). 1681
Communication by Diggins, John P. on review of his book *Mussolini and Fascism: The View From America* in A. James Gregor, "On Understanding Fascism: A Review of Some Contemporary Literature" (December, 1973). 1683
Communication by Domhoff, G. William on review of his book *The Higher Circles: The Governing Class in America* (March, 1972). . 1277
Communication by Dye, Thomas R. on Douglas D. Rose, "National and Local Forces in State Politics" (December, 1973). 1264
Communication by Eisinger, Peter K. in reply to criticism of his article "The Conditions of Protest Behavior in American Cities" (March, 1973). 1273
Communication by Gray, Virginia in reply to comment on her article "Innovation in the States: A Diffusion Study" (December, 1973). 1682
Communication by Greenstein, Fred I. on Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, "The Structuring Principle: Political Socialization and Belief Systems" (June, 1973). 720
Communication by Gregor, A. James in reply to criticism of his review essay "On Understanding Fascism: A Review of Some Contemporary Literature" (December, 1973). ... 1686

Communication by Harsanyi, John C. in reply to comment on his review of Nigel Howard, *Paradoxes of Rationality: Theory of Metagames and Political Behavior* (June, 1973). .. 730
Communication in reply to criticism of his further discussion of his review of Nigel Howard, *Paradoxes of Rationality: Theory of Metagames and Political Behavior* (June, 1974). 1694
Communication by Howard, Nigel on review of his book *Paradoxes of Rationality: Theory of Metagames and Political Behavior* (June, 1973). 729
Communication in reply to further discussion of his book *Paradoxes of Rationality: Theory of Metagames and Political Behavior* (June, 1974). 1692
Communication by Kaplan, Morton A. on review of his book *On Historical and Political Knowing* (September, 1973). 731
Communication by Katz, Richard S. in reply to comment on his article "The Attribution of Variance in Electoral Returns: An Alternative Measurement Technique" (September, 1973). 1273
Communication by Kenski, Henry C. and James W. Clarke on Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, "The Structuring Principle: Political Socialization and Belief Systems" (June, 1973). 722
Communication by Kort, Fred in reply to a comment on his article "Regression Analysis and Discriminant Analysis: An Application of R. A. Fisher's Theorem to Data in Political Science" (June, 1973). 203
Communication by Latham, Earl in reply to criticism of his review of G. William Domhoff, *The Higher Circles: The Governing Class in America* (March, 1972). 1278
Communication by Lemieux, Peter H. on Fred Kort, "Regression Analysis and Discrimant Analysis: an Application of R. A. Fisher's Theorem to Data in Political Science" (June, 1973). 202
Communication by Licklider, Roy E. on review of his book *The Private Nuclear Strategists* (December, 1973). 1274
Communication by Miller, Abraham H. and Stephen E. Bennett on James Q. Wilson and Edward C. Banfield, "Political Ethos Revisited" (December, 1971). 1265
Communication by Pfotenhauer, David on Philip Stewart et al., "Political Mobility and the Soviet Political Process: A Partial Test of Two Models" (December, 1972). 205
Communications by Riker, William H. and Steven J. Brams in reply to comments on their article "The Paradox of Vote Trading" (December, 1973). 1688, 1692
Communication by Rose, Douglas D. in reply to criticism of his article "National and Local Forces in State Politics" (December, (1973). . 1265
Communication by Schwartz, Joel J. and Donald D, Searing in reply to criticism of their and Alden E. Lind's article "The Structuring Principle: Political Socialization and Belief Systems" (June, 1973). 725
Communication by Searing, Donald D. and Joel J. Schwartz in reply to criticism of their and Alden E. Lind's article "The Structuring Principle: Political Socialization and Belief Systems" (June, 1973). 725
Communication by Stewart, Philip D. in reply to criticism of the article "Political Mobility and the Soviet Political Process: A Partial Test of Two Models" by Philip D. Stewart, Robert L. Arnett, William Ebert, Raymond E. McPhail, Terrence L. Rich, and Craig E. Schopmeyer (December, 1972). .....
Communication by Stinchcombe, Arthur L. on systems analyses. 1262
Communication by Taylor, Peter J. in reply to comments on his article "A New Shape Measure for evaluating Electoral District Patterns" (September, 1973). 1276
Communication by Tufte, Edward R. in reply to comments on his article "The Relationship Between Seats and Votes in Two Party Systems" (June, 1973). 207
Communication by Tullock, Gordon on William H. Riker and Steven J. Brams, "The Paradox of Vote Trading" (December, 1973). 1687
Communication by Wilson, James Q. and Edward C. Banfield in reply to criticism of their article "Political Ethos Revisited" (December, 1971). 1271
Communication by Wolinetz, Steven B. on his review of S. J. R. Noel, *Politics in Newfoundland* (September, 1973). 731
Community Action and the Poor: Influence vs. Social Control in a New York City Community. Kenneth J. Pollinger and Annette C. Pollinger. Reviewed by Kenneth R. Greene. ... 286
Community and Control: Critical Democratic Theory in the Progressive Period. David E. Price. 1663
Community Economic Development. John C. Weistart, ed. Reviewed by Emmett H. Buell, Jr. 288
The Community of Oil Exporting Countries; A Study in Governmental Cooperation. Zuhayr Mikdashi. Reviewed by J. S. Nye. 873
Comparative Judicial Politics. Theodore L. Becker. Reviewed by Herbert Jacob. 292
Competition and the Corporate Society: British Conservatives, the State, and Industry, 1945–1964. Nigel Harris. Reviewed by S. C. Ghosh. 1357
Computer Simulations of Voting Behavior. Wil-

liam R. Shaffer. Reviewed by Samuel L. Popkin. 1747
The Concept of Aggression in International Law. Ann Van Wynen Thomas and A. J. Thomas, Jr. Reviewed by Norman V. Walbek. 365
Conflict in the Community: A Theory of the Effects of Community Size. Gordon S. Black. .. 1245
Conflict and Harmony in Human Affairs: A Study of Cross-Pressures and Political Behavior. Peter W. Sperlich. Reviewed by Ada W. Finifter. 1749
Conflict, Power and Relative Deprivation. Walter Korpi. 1569
Confrontation With Pakistan. B. M. Kaul. Reviewed by Steven A. Hoffmann. 351
Congressional Voting in a Partisan Era: A Study of the McKinley Houses and A Comparison to The Modern House of Representatives. David W. Brady. Reviewed by Charles M. Tidmarch. 1323
Connell, R. W. Review of Mary Ellen Goodman, *The Culture of Childhood: Child's-Eye Views of Society and Culture.* 302
Connor, Walker. Review of K. R. Minogue, *Nationalism.* 320
Conrad, Robert. *The Destruction of Brazilian Slavery, 1850–1888.* Reviewed by Douglas A. Chalmers. 1799
The Conscience of the Corporations: Business and Urban Affairs, 1967–1970. Jules Cohn. Reviewed by Joseph Smucker. 1760
Conspiracy at Matsukawa. Chalmers E. Johnson. Reviewed by Kenneth E. Colton. 312
The Constitutionalist: Notes on the First Amendment. George Anastaplo. Reviewed by Dean Alfange, Jr. 774
Content Analysis: A Technique for Systematic Inference from Communications. Thomas F. Carney. Reviewed by Dina A. Zinnes. 750
The Contexts of Social Mobility: Ideology and Theory. Anselm L. Strauss. Reviewed by Edward O. Laumann. 1781
Contradiction and Dilemma: Orestes Brownson and the American Idea. Leonard Gilhooley. Reviewed by A. J. Beitzinger. 1735
The Control of Chemical and Biological Weapons. Carnegie Endowment for International Peace. Reviewed by George H. Quester in *Chemical and Biological Warfare.* 1285
The Convention Problem: Issues in Reform of Presidential Nominating Procedures. Judith H. Parris. Reviewed by Charles Press. 1756
Converse, Philip E. *Comment on Burnham's "Theory and Voting Research."* 1024
Comment: The Status of Nonattitudes. 650
Conway, Paul. Review of Robert T. Bower, *Television and the Public.* 1322
Conyngham, William J. *Industrial Management in the Soviet Union.* Reviewed by Elizabeth Clayton. 1799
Cookson, John and Judith Nottingham. *A Survey of Chemical and Biological Warfare.* Reviewed by George H. Quester in *Chemical and Biological Warfare.* 1285
Corbett, Edward M. Review of Paul Painchaud, director, *Francophonie, Bibliographie 1960–1969.* 359
Corbett, P. E. Review of John Norton Moore, *Law and the Indo-China War.* 357
Cordell, Arthur J. Review of Bart S. Fisher, *The International Coffee Agreement: A Study in Coffee Diplomacy.* 865
Cordier, Andrew W. and Wilder Foote, eds. *Public Papers of the Secretaries-General of the United Nations,* 3 vols. Reviewed by Philip C. Jessup. 1855
Corkran, Herbert, Jr. *Patterns of International Cooperation in the Caribbean, 1942–1969.* Reviewed by Thomas Mathews. 341
Cornelius, Wayne A. *Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities.* 1125
Corsi, Jerome R. *Communication* on Christopher Bruell, "Thucydides' View of Athenian Imperialism" (March, 1974). 1679
Corwin, Ronald G. and Saad Z. Nagi, eds. *The Social Contexts of Research.* Reviewed by Arnold Thackray. 1314
Costigliola, Frank. Review of Justus D. Doenecke, *The Literature of Isolationism: A Guide to Non-Interventionist Scholarship, 1930–1972.* 860
Coufoudakis, Van. Review of John O. Iatrides, *Revolt in Athens: The Greek Communist "Second Round," 1944–1945.* 309
The Country Party in New South Wales: A Study of Organisation and Survival. Don Aitkin. Reviewed by D. W. Rawson. 1786
Cowan, L. Gray. Review of Nigel Heseltine, *Madagascar.* 822
Cowling, Maurice. *The Impact of Labour, 1920–1924.* Reviewed by Martin Harrison. 810
Cox, Harvey and David Morgan. *City Politics and the Press: Journalists and the Governing of Merseyside.* Reviewed by Morris Davis. 1350
Cox, Robert W., Harold K. Jacobson, Gerard and Victoria Curzon, Joseph S. Nye, Lawrence Scheinman, James P. Sewell, and Susan Strange. *The Anatomy of Influence: Decision-Making in International Organization.* Reviewed by H. Field Haviland. 1389
Craig, Richard B. *The Bracero Program: Interest Groups and Foreign Policy.* Reviewed by Charles Ornelas. 858
Cranston, Maurice and Richard S. Peters, eds. *Hobbes and Rousseau: A Collection of Critical Essays.* Reviewed by C. B. Macpherson. 1730
Cranston, Maurice and Sanford A. Lakoff, eds. *A Glossary of Political Ideas.* Reviewed by

Clement E. Vose in *Political Dictionaries: A Bibliographical Essay*. 1696
Crazy States: A Counterconventional Strategic Problem. Yehezkel Dror. Reviewed by Donald L. Hafner. 342
Creating a New World Politics: From Conflict to Co-operation. David V. Edwards. Reviewed by J. L. Richardson. 1392
Crick, Bernard. *In Defence of Politics*, 2nd ed. Reviewed by James H. Nichols, Jr. 1731
Crime, Dissent, and the Attorney General: The Justice Department in the 1960s. John T. Elliff. Reviewed by Ruth G. Weintraub. 1324
Crime and Justice in American Society. Jack D. Douglas, ed. Reviewed by Ruth G. Weintraub. 1324
Cronin, Thomas E. Review of *Who's Who in Government, 1972–1973*, 1st ed. 290
Crosby, Richard. Review of Stephen G. Kurtz and James H. Hutson, eds., *Essays on the American Revolution*. 1332
Cross-Polity Time-Series Data. Arthur S. Banks, assembler. Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science*. 243
Cuba 1933, Prologue to Revolution. Luis E. Aguilar. Reviewed by Manuela Semidei. 335
Cuff, Robert D. *The War Industries Board: Business-Government Relations During World War I*. Reviewed by Robert A. Kagan. 1325
Cultural Engineering and Nation-Building in East Africa. Ali A. Mazrui. Reviewed by John Nellis. 831
The Culture of Childhood: Child's-Eye Views of Society and Culture. Mary Ellen Goodman. Reviewed by R. W. Connell. 302
Culture and the Comparative Study of Politics, or the Constipated Dialectic. APSA Presidential Address, 1973. Robert E. Ward. 190
Curzon, Gerard and Victoria, Robert W. Cox, Harold K. Jacobson, Joseph S. Nye, Lawrence Scheinman, James P. Sewell, and Susan Strange. *The Anatomy of Influence: Decision-Making in International Organization*. Reviewed by H. Field Haviland. 1389
Czechoslovakia and the Absolute Monopoly of Power: A Study of Political Power in a Communist System. Barbara Wolfe Jancar. Reviewed by Galia Golan. 1813

Dahl, Robert A. and Edward R. Tufte. *Size and Democracy*. Reviewed by Heinz Eulau. 1301
Dalfiume, Richard M. *Desegregation of the U.S. Armed Forces: Fighting on Two Fronts, 1939–1953*. Reviewed by William F. Levantrosser. 1758
D'Amato, Anthony. Review of Wesley L. Gould and Michael Barkun, *Social Science Literature: A Bibliography for International Law*. 1394
D'Anjou, Lenore, Barbara Ward, and J. D. Runnalls, eds. *The Widening Gap: Development in the 1970s*. Reviewed by Walter Buhr. . 367
David Davies Memorial Institute of International Studies. *International Disputes: The Legal Aspects*. Reviewed by A. J. R. Groom. . 1857
David, Paul T. Review of Wayne E. Fuller, *The American Mail: Enlarger of the Common Life*. 278
Davidson, Robert D. and Allen Reitman. *The Election Process, Voting Laws and Procedures*. Reviewed by Max M. Kampelman. 784
Davidson, Terrence N., Jerald G. Bachman, Robert L. Kahn, Martha T. Mednick, and Lloyd D. Johnston. *Youth in Transition*, Vol. I: *Blueprint for a Longitudinal Study of Adolescent Boys*. Reviewed by Jarol B. Manheim. . 1296
Davies, John Paton, Jr. *Dragon by the Tail*. Reviewed by Allen S. Whiting. 1853
Davis, David Howard. *How the Bureaucracy Makes Foreign Policy: An Exchange Analysis*. Reviewed by Fred A. Kramer. 781
Davis, Morris. Review of Harvey Cox and David Morgan, *City Politics and the Press: Journalists and the Governing of Merseyside*. . 1350
Davis, Morton D. and Steven J. Brams. *The 3/2's Rule in Presidential Campaigning*. 113
Dawson, Philip. *Provincial Magistrates and Revolutionary Politics in France, 1789–1795*. Reviewed by Robert Forster. 811
Review of Robert Forster and Jack P. Greene, eds., *Preconditions of Revolution in Early Modern Europe*. The Johns Hopkins Symposia in History. 1803
Deadlock in School Desegregation: A Case Study of Inglewood, California. Edna Bonacich and Robert F. Goodman. Reviewed by Leslie Lenkowsky. 272
Deber, Raisa B. *Communication* on Virginia Gray, "Innovation in the States: A Diffusion Study" (December, 1973). 1681
Debray, Regis. *The Chilean Revolution: Conversations with Allende*. Reviewed by Weston H. Agor. 812
Decentralization and Self-Government in Russia, 1830–1870. S. Frederick Starr. Reviewed by Marc Raeff. 332
DeConde, Alexander. Review of William C. Stinchcombe, *The American Revolution and the French Alliance*. 1881
Dedijer, Vladimir. *The Battle Stalin Lost: Memoirs of Yugoslavia, 1948–1953*. Reviewed by Milan J. Reban. 298
Deferential-Participant Politics: The Early Republic's Political Culture, 1789–1840. Ronald P. Formisano. 473
Delyusin, L., Y. Zhukov, A. Iskenderov, and L. Stepanov. *The Third World: Problems and Prospects*. Reviewed by Edward Taborsky. .. 889
Democracy in Crisis: New Challenges to Consti-

tutional Democracy in the Atlantic Area. E. A. Goerner, ed. Reviewed by Minton F. Goldman. 1807
Denny, Brewster C. Review of Rutherford M. Poats, *Technology for Developing Nations: New Directions for U.S. Technical Assistance.* 1371
Derthick, Martha. Review of Jeffrey L. Pressman and Aaron Wildavsky, *Implementation: How Great Expectations in Washington are Dashed in Oakland; Or, Why It's Amazing that Federal Programs Work at All, This Being a Saga of the Economic Development Administration as Told by Two Sympathetic Observers Who Seek to Build Morals on a Foundation of Ruined Hopes.* 1336
Desegregation of the U.S. Armed Forces: Fighting on Two Fronts, 1939–1953. Richard M. Dalfiume. Reviewed by William F. Levantrosser. 1758
The Desirability of a Correctional Ombudsman. Timothy L. Fitzharris. Reviewed by Karl A. Friedmann. 1326
The Destruction of Brazilian Slavery, 1850–1888. Robert Conrad. Reviewed by Douglas A. Chalmers. 1799
Destutt de Tracy, Antoine Louis Claude. *A Treatise on Political Economy.* Reviewed by G. David Garson. 750
Détente in Europe: Real or Imaginary? Josef Korbel. Reviewed by Kendall Myers. 871
Developing Democracy. William A. Douglas. Reviewed by Gordon P. Means. 813
The Development of the Marxian Dialectic. Dick Howard. Reviewed by Z. A. Jordan. 1308
Development Policy II: The Pakistan Experience. Walter P. Falcon and Gustav F. Papanek, eds. Reviewed by Rowland Egger. 1800
Dexter, Lewis Anthony. *Elite and Specialized Interviewing.* Reviewed by Keith M. Henderson. 752
Review of F. G. Bailey, ed., *Gifts and Poison: The Politics of Reputation.* 254
Dialectic of Enlightment. Max Horkheimer and Theodor W. Adorno. John Cumming, trans. Reviewed by Hiram Caton. 1307
Dictionary of American Politics, 2nd ed. Edward C. Smith and Arnold J. Zurcher. Reviewed by Clement E. Voss in *Political Dictionaries: A Bibliographical Essay.* 1696
A Dictionary of Modern War. Edward Luttwak. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
A Dictionary of Political Analysis. Geoffrey K. Roberts. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
Dictionary of Political Science. Joseph Dunner, ed. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
A Dictionary of Politics, 6th ed. Florence Elliott. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
A Dictionary of Politics. Walter Laqueur, ed. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
Dietze, Gottfried. *Two Concepts of the Rule of Law.* Reviewed by Stephen L. Wasby. 1303
The Diffusion of Power, 1957–1972: An Essay in Recent History. W. W. Rostow. Reviewed by Lincoln P. Bloomfield. 1874
Diggins, John P. *Communication* on review of his book *Mussolini and Fascism: The View From America* in A. James Gregor, "On Understanding Fascism: A Review of Some Contemporary Literature" (December, 1973). 1683
The Dimensions of Nations. R. J. Rummel. Reviewed by Gordon Hilton. 1408
Dion, Lèon. *Société et Politique: La vie des Groupes. Tome premier: Fondements de la Société Libérale. Tome Second: Dynamique de la Société Libérale.* Reviewed by Henry W. Ehrmann. 1733
Diplomacy for the 70s: An Afterview and Appraisal. William I. Bacchus. Review of U.S. Department of State, *Diplomacy for the 70s: A Program of Management Reform for the Department of State.* 736
Diplomacy for the 70s: A Program of Management Reform for the Department of State. U.S. Department of State. Reviewed by William I. Bacchus in *Diplomacy for the 70s: An Afterview and Appraisal.* 736
Directory of Selected Scientific Institutions in Mainland China. Surveys and Research Corporation. Reviewed by Susan Beth Rifkin. 333
The Dissolution of Power: A Sociology of International Relations and Politics. Silviu Brucan. Reviewed by Robert L. Farlow. 339
Divine, Robert A. *Roosevelt and World War II.* Reviewed by Arnold A. Offner. 1858
The Divisive Primary Revisited: Party Activists in Iowa. Donald Bruce Johnson and James R. Gibson. 67
Dodd, Lawrence C. *Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis.* 1093
Doehrman, Steven. Review of P. G. Herbst, *Behavioural Worlds: The Study of Single Cases.* 758
Doenecke, Justus D. *The Literature of Isolationism: A Guide to Non-Interventionist Scholarship, 1930–1972.* Reviewed by Frank Costigliola. 860
Domar, Evsey D. Review of Joseph T. Fuhrmann, *The Origins of Capitalism in Russia: Industry and Progress in the Sixteenth and Seventeenth Centuries.* 818
Domhoff, G. William. *Communication* on review of his book *The Higher Circles: The Governing Class in America* (March, 1972). . 1277

Dominguez, Jorge I. Review of John Friedmann, *Urbanization, Planning, and National Development.* 1354
The Dominican Republic: Rebellion and Repression. Carlos Maria Gutierrez. Reviewed by Howard J. Wiarda. 1809
Donheiser, Alan D., Robert C. Fraser, and Thomas G. Miller, Jr. *Civil Aviation Development: A Policy and Operating Analysis.* Prepared for Arthur D. Little, Inc. Reviewed by Robert L. Thornton. 857
Doolin, Dennis J., Charles Price Ridley, and Paul H. B. Godwin. *The Making of A Model Citizen in Communist China.* Reviewed by Franklin W. Houn. 1835
Doran, Charles. *The Politics of Assimilation: Hegemony and Its Aftermath.* Reviewed by Richard Rosecrance. 860
Dorsey, John M. *Psychology of Political Science With Special Consideration for the Political Acumen of Destutt de Tracy.* Reviewed by G. David Garson. 750
Douglas, Jack D., ed. *Crime and Justice in American Society.* Reviewed by Ruth G. Weintraub. 1324
Douglas, William A. *Developing Democracy.* Reviewed by Gordon P. Means. 813
Douglas, William O. *Holocaust or Hemispheric Co-op: Cross Currents in Latin America.* Reviewed by Edward S. Milenky. 861
Douglass, Bruce and Ross Terrill, eds. *China and Ourselves: Explorations and Revisions by a New Generation.* Reviewed by Oliver M. Lee. 814
Downes, Randolph C. *The Rise of Warren Gamaliel Harding, 1865–1920.* Reviewed by Frank A. Burd. 277
Drache, Daniel, ed. *Quebec—Only the Beginning: The Manifestoes of the Common Front.* Reviewed by Peter Regenstreif. 808
Dragnich, Alex N. Review of Walter R. Roberts, *Tito, Mihailović and the Allies, 1941–1945.* .. 1372
Dragon by the Tail. John Paton Davies, Jr. Reviewed by Allen S. Whiting. 1853
Dreijmanis, John. Review of Dirk Hartung, Reinhard Nuthmann, and Dietrich Winterhager, *Politologen im Beruf. Zur Aufnahme und Durchsetzung neuer Qualifikationem im Beschäftigungssystem.* 304
Dresang, Dennis L. *Ethnic Politics, Representative Bureaucracy and Development Administration: The Zambian Case.* 1605
Dror, Yehezkel. *Crazy States: A Counterconventional Strategic Problem.* Reviewed by Donald L. Hafner. 342
Drug Use in America: Problem in Perspective. National Commission on Marihuana and Drug Abuse. Reviewed by Joseph L. Zentner. 1329
Duggan, William R. *A Socioeconomic Profile of South Africa.* Reviewed by Thomas Karis. .. 1391
Duncan, W. Raymond. Review of Sven Lindqvist, *The Shadow: Latin America Faces the Seventies.* Keith Bradfield, trans. 1821
Dunn, Edgar S., Jr. *Economic and Social Development: A Process of Social Learning.* Reviewed by Roy E. Feldman. 255
Dunner, Joseph, ed. *Dictionary of Political Science.* Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* ... 1696
Dunn, John. *Modern Revolution: An Introduction to the Analysis of a Political Phenomenon.* Reviewed by Rex Mortimer. 1351
Review of David H. Pinkney, *The French Revolution of 1830.* 1833
Dunsire, Andrew. *Administration: The Word and the Science.* Reviewed by Robert T. Golembiewski. 1303
Dupree, Louis. Review of Lionel Caplan, *Land and Social Change in East Nepal: A Study of Hindu-Tribal Relations.* 809
Dupuy, T. N. and Wendell Blanchard, eds. *The Almanac of World Military Power.* Reviewed by James A. Huston. 862
Dye, Thomas R. *Communication* on Douglas D. Rose, "National and Local Forces in State Politics" (December, 1973). 1264

East Indians in Trinidad: A Study in Minority Politics. Yogendra K. Malik. Reviewed by H. Hoetink. 1825
Eastin, Roy B. and Laurence F. Schmeckebier, *Government Publications and Their Use,* 2nd rev. ed. Reviewed by John Brown Mason. .. 1753
East and West Germany: A Comparative Economic Analysis. Martin Schnitzer. Reviewed by Gerald G. Watson. 1373
Economic and Social Development: A Process of Social Learning. Edgar S. Dunn, Jr. Reviewed by Roy E. Feldman. 255
Edelman, Murray. *Politics as Symbolic Action: Mass Arousal and Quiescence.* Reviewed by Marc Howard Ross. 257
Education and State Politics. Michael D. Usdan, David W. Minar, and Emanuel Hurwitz, Jr. Reviewed by Carl Grafton. 794
Education, Training, and the Urban Ghetto. Bennett Harrison. Reviewed by JeDon A. Emenhiser. 1328
Edwards, David V. *Creating a New World Politics: From Conflict to Co-operation.* Reviewed by J. L. Richardson. 1392
Egbert, Donald Drew. *Social Radicalism and the Arts: Western Europe.* Reviewed by Louis Wasserman. 753
Egger, Rowland. Review of Walter P. Falcon and Gustav F. Papanek, eds., *Development Policy II: The Pakistan Experience.* 1800

Egypt: Imperialism and Revolution. Jacques Berque. Reviewed by John P. Entelis. 293
Ehrensweig, Albert A. *Psychoanalytic Jurisprudence: On Ethics, Aesthetics, and Law—On Crime, Tort, and Procedure.* Reviewed by G. Edward White. 754
Ehrmann, Henry W. Review of Lèon Dion, *Société et Politique: La vie des Groupes. Tome premier: Fondements de la Société Libérale. Tome second: Dynamique de la Société Libérale;* Franz Nuscheler and Winfried Steffani, eds., *Pluralismus: Konzeptionen und Kontroversen;* Rainer Eisfeld, *Pluralismus Zwischen Liberalismus und Sozialismus.* 1733
Eisele, Frederick R. Review of Matilda White Riley et al., *Aging and Society*, 3 vols. 262
Eisfeld, Rainer. *Pluralismus zwischen Liberalismus und Sozialismus.* Reviewed by Henry W. Ehrmann. 1733
Eisinger, Peter K. *Communication* in reply to criticism of his article "The Conditions of Protest Behavior in American Cities" (March, 1973). 1273
Racial Differences in Protest Participation. .. 592
Elazar, Daniel J. *The Politics of Belleville: A Profile of the Civil Community.* Reviewed by Phillip Monypenny. 1761
The Election Game and How To Win It. Joseph Napolitan. Reviewed by Max M. Kampelman. 784
Election Goals and Strategies: Equivalent and Nonequivalent Candidate Objectives. Peter H. Aranson, Melvin J. Hinich, and Peter C. Ordeshook. 135
The Election Men. David Lee Rosenbloom. Reviewed by Max M. Kampelman. 784
The Election Process, Voting Laws and Procedures. Allen Reitman and Robert D. Davidson. Reviewed by Max M. Kampleman. 784
Elections in South Vietnam. Howard Penniman. Reviewed by Allan E. Goodman. 1370
Elite and Specialized Interviewing. Lewis Anthony Dexter. Reviewed by Keith M. Henderson. 752
Elkins, David J. *The Measurement of Party Competition.* 682
Elliff, John T. *Crime, Dissent, and the Attorney General: The Justice Department in the 1960s.* Reviewed by Ruth G. Weintraub. 1324
Elliott, Florence. *A Dictionary of Politics*, 6th ed. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay*..... 1696
Elsbree, Willard H. Review of Donald C. Hellmann, *Japan and East Asia: The New International Order.* 1395
Review of Richard L. Walker, ed., *Prospects in the Pacific.* With an introduction by the editor. 1883
Emblem of Liberty: The Image of Lafayette in the American Mind. Anne C. Loveland. Reviewed by Michael McGiffert. 282
Emenhiser, JeDon A. Review of Bennett Harrison, *Education, Training, and the Urban Ghetto.* 1328
The Emergence of the Indian National Congress. S. R. Mehrotra. Reviewed by Paul Wallace... 1367
Emerging Nationalism in Portuguese Africa: Documents. Ronald H. Chilcote. Reviewed by Gerald J. Bender. 296
Emmerson, John K. *Arms, Yen and Power: The Japanese Dilemma.* Reviewed by Paul F. Langer. 863
The English Constitution. Walter Bagehot. With an introduction by R. H. S. Crossman, M. P. Reviewed by Samuel Beer in *Tradition and Nationality: A Classic Revisited.* 1292
Englishmen and Irish Troubles: British Public Opinion and the Making of Irish Policy, 1918–1922. D. G. Boyce. Reviewed by Paul A. Pfretzschner. 295
Enteen, George. Review of Anatole G. Mazour, *The Writing of History in the Soviet Union.* .. 319
Entelis, John P. Review of Jacques Berque, *Egypt: Imperialism and Revolution.* 293
Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior. Paul Allen Beck. 1229
Epstein, Fritz T., ed. Penelope H. Carson and Michael E. Shaw, associate eds. *The American Bibliography of Russian and East European Studies for 1966.* Reviewed by John A. Armstrong. 299
The Era of Trujillo: Dominican Dictator. Jesús de Galíndez. Reviewed by Howard J. Wiarda. 818
Erickson, John and J. N. Wolfe, eds. *The Armed Services and Society.* Reviewed by William F. Levantrosser. 1758
Erler, Edward J. Review of Norman E. Bowie, *Towards a New Theory of Distributive Justice.* 749
Esman, Milton. *Administration and Development in Malaysia: Institution Building and Reform in a Plural Society.* Reviewed by Lucian W. Pye. 1352
Essays on the American Revolution. Stephen G. Kurtz and James H. Hutson, eds. Reviewed by Richard Crosby. 1332
Establishing Ombudsman Offices: Recent Experience in the United States. Transcript of the Ombudsman WORKSHOP, Honolulu, Hawaii, May 5–7, 1971. Stanley V. Anderson and John E. Moore, eds. Reviewed by William B. Gwyn. 1752
Ethnic Politics, Representative Bureaucracy and Development Administration: The Zambian Case. Dennis L. Dresang. 1605
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Eulau, Heinz. Review of Robert A. Dahl and Edward R. Tufte, *Size and Democracy*. 1301
The European Community in the 1970s. Steven Joshua Warnecke, ed. Reviewed by Robert L. Pfaltzgraff, Jr. 369
Europe Since World War II: The Big Change. Norman Luxenburg. Reviewed by Douglas W. Simon. 872
Europe in the Twentieth Century. George Lichtheim. Reviewed by George L. Mosse. . 1402
The Evolution of American Democracy. William R. Brock. Reviewed by Richard P. McCormick. 273
The Evolving United Nations: A Prospect for Peace? Kenneth J. Twitchett, ed. Reviewed by Harold K. Jacobson. 884
The Expansion of England. J. R. Seeley. With an introduction by John Gross, ed. Reviewed by A. L. Burns. 844
L'Explication Politique: Une Introduction a l'Analyse Comparative. Alfred Grosser. Reviewed by Roy Macridis. 1736

Les Facteurs locaux de la vie politique nationale. Institut d'Études Politiques de Bordeaux, under the direction of Albert Mabileau. Reviewed by William R. Schonfeld. 310
Fairbank, John King. *The United States and China*, 3rd ed. Reviewed by C. P. FitzGerald. 344
Falcon, Walter P. and Gustav F. Papanek, eds. *Development Policy II: The Pakistan Experience*. Reviewed by Rowland Egger. 1800
Falk, Richard A., ed. *The International Law of Civil War*. Reviewed by Oliver J. Lissitzyn. .. 345
Fallows, James M. *The Water Lords*. Reviewed by Dean E. Mann. 1762
Farlow, Robert L. Review of Silviu Brucan, *The Dissolution of Power: A Sociology of International Relations and Politics*. 339
Farrell, Brian. *Chairman or Chief? The Role of Taoiseach in Irish Government;* and *The Founding of Dáil Éireann: Parliament and Nation-Building*. Reviewed by John H. Whyte. . 803
Review of Robert Kee, *The Green Flag: The Turbulent History of the Irish National Movement*. 824
Farrell, Susan, K. H. Kim, and Ewan Clague. *The All-Volunteer Army: An Analysis of Demand and Supply*. Reviewed by Ryan C. Amacher and Robert D. Tollison. 787
Fedder, Edwin, ed. *NATO in the Seventies*. Reviewed by Robert S. Jordan. 864
Federal Judges: The Appointing Process. Harold W. Chase. Reviewed by David Adamany. .. 275
Fein, Leonard J. Review of S. Clement Leslie, *The Rift in Israel: Religious Authority and Secular Democracy*. 1820
Feldman, Roy E. Review of Edgar S. Dunn, Jr., *Economic and Social Development: A Process of Social Learning*. 255
Feld, Werner J. Review of Robert M. Slusser, *The Berlin Crisis of 1961: Soviet-American Relations and the Struggle for Power in the Kremlin, June–November 1961*. 1410
Fellman, David. Review of Stuart S. Nagel, ed., *The Rights of the Accused*. 1774
Ferdinand Toennies on Sociology: Pure, Applied and Empirical. Selected Writings. Werner J. Cahnmann and Rudolf Heberle, eds. Reviewed by Donald A. Nielsen. 1728
Ferejohn, John A. and Morris P. Fiorina. *The Paradox of Not Voting: A Decision Theoretic Analysis*. 525
Ferguson, Anthony W. Review of Peter J. Seybolt, *Revolutionary Education in China: Documents and Commentary*. 1375
Ferkiss, Victor. Review of Raphael G. Kaspar, ed., *Technology Assessment: Understanding the Social Consequences of Technological Applications*. 1310
Fifteenth Century England, 1399–1509: Studies in Politics and Society. S. B. Chrimes, C. D. Ross, and R. A. Griffiths, eds. Reviewed by Arthur Joseph Slavin. 1797
The Fight for Competitive Advantage: A Study of the United States Domestic Air Trunk Carriers. William E. Fruhan. Reviewed by Robert L. Thornton. 1327
Finger, Nachum. *The Impact of Government Subsidies on Industrial Management: The Israeli Experiment*. Reviewed by Amos Perlmutter. . 815
Finifter, Ada W. *The Friendship Group as a Protective Environment for Political Deviants*. ... 607
Review of Peter W. Sperlich, *Conflict and Harmony in Human Affairs: A Study of Cross-Pressures and Political Behavior*. 1749
Fiori, Giuseppe. *Antonio Gramsci: Life of a Revolutionary*. Reviewed by Dante Germino. 1305
Fiorina, Morris P. and John A. Ferejohn. *The Paradox of Not Voting: A Decision Theoretic Analysis*. 525
Fiscal Federalism. Wallace E. Oates. Reviewed by Deil S. Wright. 1777
Fischer, George, ed. *The Revival of American Socialism: Selected Papers of the Socialist Scholars Conference*. Reviewed by Gary D. Glenn. 1764
Fishburn, Peter C. *Paradoxes of Voting*. 537
Fisher, Bart S. *The International Coffee Agreement: A Study in Coffee Diplomacy*. Reviewed by Arthur J. Cordell. 865
Fisher, Glen H. *Public Diplomacy and the Behavioral Sciences*. Reviewed by Richard A. Rettig. 1859
Fiszman, Joseph R. *Revolution and Tradition in People's Poland: Education and Socialization*. Reviewed by V. C. Chrypinski. 1801

FitzGerald, C. P. Review of John King Fairbank, *The United States and China*, 3rd ed. .. 344
Review of Tai Sung An, *Mao Tse-tung's Cultural Revolution*. 799
The Southern Expansion of the Chinese People. Reviewed by D. G. E. Hall. 1802
Fitzgerald, Stephen. *China and the Overseas Chinese: A Study of Peking's Changing Policy, 1949–1970*. Reviewed by Robert O. Tilman. . 866
Fitzharris, Timothy L. *The Desirability of a Correctional Ombudsman*. Reviewed by Karl A. Friedmann. 1326
Flathman, Richard E. *Political Obligation*. Reviewed by Leslie J. Macfarlane. 1305
Foley, Donald. *Governing the London Region: Reorganization and Planning in the 1960s*. Reviewed by Noel Boaden. 816
Foltz, William J. Review of Nicholas S. Hopkins, *Popular Government in an African Town: Kita, Mali*. 1358
Foner, Anne and Matilda White Riley. *Aging and Society*, Vol. I: *An Inventory of Research Findings;* Anne Foner, Matilda White Riley, and Marilyn Johnson. *Aging and Society*, Vol. III: *A Sociology of Age Stratification*. Reviewed by Frederick R. Eisele. 262
Foote, Wilder and Andrew W. Cordier, eds. *Public Papers of the Secretaries-General of the United Nations*, 3 vols. Reviewed by Philip C. Jessup. 1855
Foreign Policy and Empirical Democratic Theory. David W. Moore. 1192
Formisano, Ronald P. *The Birth of Mass Political Parties: Michigan, 1827–1861*. Reviewed by Roger E. Wyman. 782
Deferential-Participant Politics: The Early Republic's Political Culture, 1789–1840. 473
Forster, Robert and Jack P. Greene, eds. *Preconditions of Revolution in Early Modern Europe*. The Johns Hopkins Symposia in History. Reviewed by Philip Dawson. 1803
Forster, Robert. Review of Philip Dawson, *Provincial Magistrates and Revolutionary Politics in France, 1789–1795*. 811
Förster, Winfried. *Thomas Hobbes und der Puritanismus*. Reviewed by Herbert W. Schneider. 755
Fortin, Ernest L. Review of Stanley Chodorow, *Christian Political Theory and Church Politics in the Mid-Twelfth Century: The Ecclesiology of Gratian's Decretum*. 1300
The Fortunes of the West: The Future of the Atlantic Nations. Theodore Geiger. Reviewed by Annette Baker Fox. 1393
The Foundation of the German Empire: Select Documents. Helmut Böhme, ed. Agatha Ramm, trans. Reviewed by Otto Pflanze. ... 295
Foundations of Political Sociology. Irving Louis Horowitz. Reviewed by Richard Ashcraft. .. 761
The Founding of Dáil Éireann: Parliament and Nation-Building. Brian Farrell. Reviewed by John H. Whyte. 803
The Fourth World: Victims of Group Oppression. Ben Whitaker, ed. Reviewed by Richard Sandbrook. 1381
Fowler, Edmund P., Alan Lupo, and Frank Colcord. *Rites of Way: The Politics of Transportation in Boston and the U.S. City*. Reviewed by G. Ross Stephens. 282
Fox, Annette Baker. Review of Marshall R. Singer, *Weak States in a World of Powers*. .. 1877
Review of Theodore Geiger, *The Fortunes of the West: The Future of the Atlantic Nations*. . 1393
Fox, Richard G. *Kin, Clan, Raja and Rule: State-Hinterland Relations in Pre-industrial India*. Reviewed by Marguerite Ross Barnett. 817
France, 1848–1945. Volume I: Ambition, Love, and Politics. Theodore Zeldin. Reviewed by Eugen Weber. 1847
Francophonie, Bibliographie 1960–1969. Paul Painchaud, director. Reviewed by Edward M. Corbett. 359
Frank, Joseph. Review of Ellis Sandoz, *Political Apocalypse: A Study of Dostoevsky's Grand Inquisitor*. 1747
Franklin, Julian H. *Jean Bodin and the Rise of Absolutist Theory*. Reviewed by Randal H. Ihara. 1306
Frankrikes författningar. Ragnar Simonsson. Reviewed by M. Donald Hancock. 845
Frase, Robert W. and Charles McKinley. *Launching Social Security: A Capture-and-Record Account, 1935–1937*. Reviewed by Louis L. Friedland. 777
Fraser, Robert C. Alan D. Donheiser, and Thomas G. Miller, Jr. *Civil Aviation Development: A Policy and Operating Analysis*. Prepared for Arthur D. Little, Inc. Reviewed by Robert L. Thornton. 857
Fraser, Stewart E. and Kuang-liang Hsu. *Chinese Education and Society: A Bibliographic Guide*. Reviewed by Merle Goldman. 1804
Freedman, Robert Owen. Review of Peter Mansfield, ed., *The Middle East: A Political and Economic Survey*, 4th ed. 1365
The French Colonial Myth and Constitution-Making in the Fourth Republic. D. Bruce Marshall. Reviewed by Peter Alexis Gourevitch. 1366
The French Pacific Islands: French Polynesia and New Caledonia. Virginia Thompson and Richard Adloff. Reviewed by Francis West. . 1843
The French Revolution of 1830. David H. Pinkney. Reviewed by John Dunn. 1833
Fried, Albert, ed. *Socialism in America: From the Shakers to the Third International*. Reviewed by Gary D. Glenn. 1764
Friedland, Louis L. Review of J. Douglas

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Friedman, Julian R. Review of Ian Mackler, *Pattern for Profit in Southern Africa.* 1824
Friedman, Milton and Wilbur J. Cohen. *Social Security: Universal or Selective?* Reviewed by Louis L. Friedland. 777
Friedmann, John. *Urbanization, Planning and National Development.* Reviewed by Jorge I. Domínguez. 1354
Friedmann, Karl A. Review of Timothy L. Fitzharris, *The Desirability of a Correctional Ombudsman.* 1326
Friedrich, Paul. *Agrarian Revolt in a Mexican Village.* Reviewed by Lola Romanucci-Ross. 1805
The Friendship Group as a Protective Environment for Political Deviants. Ada W. Finifter. . 607
Friesema, H. Paul. *Metropolitan Political Structure: Intergovernmental Relations and Political Integration in the Quad-Cities.* Reviewed by Francis M. Wilhoit. 1765
From Resistance to Revolution: Colonial Radicals and the Development of American Opposition to Britain, 1765–1776. Pauline Maier. Reviewed by Lance Banning. 284
Fruhan, William E. *The Fight for Competitive Advantage: A Study of the United States Domestic Air Trunk Carriers.* Reviewed by Robert L. Thornton. 1327
Fuccillo, Vincent J. Review of Jack N. Behrman, *U.S. International Business and Governments;* and *National Interests and the Multinational Enterprise: Tensions Among the North Atlantic Countries.* 855
Fuchs, Lawrence H. Review of Michael Novak, *The Rise of the Unmeltable Ethnics: The New Political Force of the Seventies.* 790
Fuhrmann, Joseph T. *The Origins of Capitalism in Russia: Industry and Progress in the Sixteenth and Seventeenth Centuries.* Reviewed by Evsey D. Domar. 818
Fuller, Wayne E. *The American Mail: Enlarger of the Common Life.* Reviewed by Paul T. David. 278
Fundamental Guarantees of the Parties in Civil Litigation: Studies in National, International and Comparative Law. Mauro Cappelletti and Denis Tallon, eds. Reviewed by Richard Pierre Claude. 1386
The Future of Law in a Multicultural World. Adda B. Bozeman. Reviewed by Wesley L. Gould. 338

Gabor, Dennis. *The Mature Society.* Reviewed by Kenneth W. Thompson. 756
Galíndez, Jesús de. *The Era of Trujillo: Dominican Dictator.* Reviewed by Howard J. Wiarda. 818
Gallo, Max. *The Night of Long Knives.* Lily Emmet, trans. Reviewed by C. F. Latour. .. 300
Gamer, Robert E. *The Politics of Urban Development in Singapore.* Reviewed by Lynn T. White, III. 1355
Gann, L. H. Review of Margery Perham, *Colonial Sequence, 1930 to 1949: A Chronological Commentary upon British Foreign Policy, Especially in Africa;* and *Colonial Sequence, 1949 to 1969: A Chronological Commentary upon British Colonial Policy in Africa,* 2 vols. 1870
Gardiner, Hilliard A. Review of James Chace, *A World Elsewhere: The New American Foreign Policy.* 1387
Garson, G. David. *On the Origins of Interest-Group Theory: A Critique of a Process.* 1505
Review of Antoine Louis Claude Destutt de Tracy, *A Treatise on Political Economy;* and John M. Dorsey, *Psychology of Political Science With Special Consideration for the Political Acumen of Destutt de Tracy.* 750
Gavre, Mark. *Hobbes and His Audience: The Dynamics of Theorizing.* 1542
Geiger, Theodore. *The Fortunes of the West: The Future of the Atlantic Nations.* Reviewed by Annette Baker Fox. 1393
Gelber, Harry G. Review of Ian Bellany, *Australia in the Nuclear Age: National Defence and National Development.* 1792
General Idea of the Revolution in the 19th Century. P.-J. Proudhon. John Beverly Robinson, trans. Reviewed by Charles E. Butterworth. .. 1737
General Stilwell in China, 1942–1944: The Full Story. Chin-tung Liang. Reviewed by Allen S. Whitling. 1853
Generational Change in American Electoral Behavior. Paul R. Abramson. 93
The Geography of Modernization in Kenya: A Spatial Analysis of Social, Economic, and Political Change. Edward W. Soja. Reviewed by Jay E. Hakes. 331
Gerhardt, James M. Review of Jerome Johnston, and Jerald G. Bachman, *Young Men Look at Military Service: A Preliminary Report;* Charles C. Moskos, Jr., ed., *Public Opinion and the Military Establishment.* 1768
Gerhardt, Paul. Review of Davendra Kaushik, *Soviet Relations with India and Pakistan.* 1400
German Resistance to Hitler: Count von Moltke and the Kreisau Circle. Ger van Roon. Peter Ludlow, trans. Reviewed by Walter Struve. .. 1844
Germany in Our Time: A Political History of the Postwar Years. Alfred Grosser. Reviewed by Catherine McArdle Kelleher. 1808
Germino, Dante. Review of Gaetano Mosca, *The Ruling Class: Elementi di scienza politica.* 261
Review of Giuseppe Fiori, *Antonio Gramsci: Life of a Revolutionary.* 1305

Gert, Bernard, ed. *Man and Citizen.* Thomas Hobbes. Reviewed by Brian Barry. 759
Ghosh, S. C. Review of Nigel Harris, *Competition and the Corporate Society: British Conservatives, the State, and Industry, 1945–1964.* 1357
Gibson, James R. and Donald Bruce Johnson. *The Divisive Primary Revisited: Party Activists in Iowa.* 67
Gifts and Poison: The Politics of Reputation. F. G. Bailey, ed. Reviewed by Lewis A. Dexter. 254
Gil, Federico G. Review of Alan Angell, *Politics and the Labour Movement in Chile.* 800
Gilhooley, Leonard. *Contradiction and Dilemma: Orestes Brownson and the American Idea.* Reviewed by A. J. Beitzinger. 1735
Gilison, Jerome M. *British and Soviet Politics: Legitimacy and Convergence.* Reviewed by A. H. Brown. 301
Ginsburg, George and Alvin Z. Rubinstein, eds. *Soviet and American Policies in the United Nations.* Reviewed by Cheryl Christensen. 878
Ginther, Konrad. Review of Thomas O. Schlesinger, *Austrian Neutrality in Postwar Europe: The Domestic Roots of a Foreign Policy.* 881
Gitelmann, Z. Y. *Jewish Nationality and Soviet Politics.* Reviewed by Lionel Kochan. 1805
Gittleman, Jerome. Review of H. H. Bruun, *Science, Values and Politics in Max Weber's Methodology.* 1727
Glenn, Gary D. Review of George Fischer, ed., *The Revival of American Socialism: Selected Papers of the Socialist Scholars Conference;* and Albert Fried, ed., *Socialism in America: From the Shakers to the Third International.* .. 1764
A Glossary of Political Ideas. Maurice Cranston and Sanford A. Lakoff, eds. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
Godschalk, David R. *Participation, Planning, and Exchange in Old and New Communities: A Collaborative Paradigm.* Reviewed by Judith V. May. 1766
Godwin, Paul H. B., Charles Price Ridley, and Dennis J. Doolin. *The Making of A Model Citizen in Communist China.* Reviewed by Franklin W. Houn. 1835
Goerner, E. A., ed. *Democracy in Crisis: New Challenges to Constitutional Democracy in the Atlantic Area.* Reviewed by Minton F. Goldman. 1807
Golan, Galia. Review of Barbara Wolfe Jancar, *Czechoslovakia and the Absolute Monopoly of Power: A Study of Political Power in a Communist System.* 1813
Goldman, Merle. Review of Stewart E. Fraser and Kuang-liang Hsu, *Chinese Education and Society: A Bibliographic Guide.* 1804
Goldman, Minton F. Review of E. A. Goerner, ed., *Democracy in Crisis: New Challenges to Constitutional Democracy in the Atlantic Area.* 1807
Goldman, Perry M. and James S. Young, eds. *U.S. Congressional Directories, 1798–1840.* Reviewed by Nelson W. Polsby. 1328
Golembiewski, Robert T. Review of Andrew Dunsire, *Administration: The Word and the Science.* 1303
Goodall, Merrill R. Review of Leo Rose, *Nepal: Strategy for Survival.* 878
Goodman, Allan E. Review of Howard Penniman, *Elections in South Vietnam.* 1370
Goodman, Mary Ellen. *The Culture of Childhood: Child's-Eye Views of Society and Culture.* Reviewed by R. W. Connell. 302
Goodman, Robert F. and Edna Bonacich. *Deadlock in School Desegregation: A Case Study of Inglewood, California.* Reviewed by Leslie Lenkowsky. 272
Gordenker, Leon. Review of Alexander Szalai, *The United Nations and the News Media.* With Margaret Croke and Associates. 1881
Gordon, Harold J. Jr. *Hitler and the Beer Hall Putsch.* Reviewed by C. F. Latour. 300
Gouinlock, James. *John Dewey's Philosophy of Value.* Reviewed by Edward B. Portis. 1735
Gould, James W. *The United States and Malaysia.* Reviewed by Robert O. Tilman. 1860
Gould, Wesley L. and Michael Barkun. *Social Science Literature: A Bibliography for International Law.* Reviewed by Anthony D'Amato. 1394
Gould, Wesley L. Review of Adda B. Bozeman, *The Future of Law in a Multicultural World.* .. 338
Goure, Leon. *The Military Indoctrination of Soviet Youth.* Reviewed by Jaan Pennar. 1808
Review of Michael Parrish, *The Soviet Armed Forces, Books in English, 1950–1967.* Hoover Institution Bibliographical Series: XLVIII. .. 1370
Gourevitch, Peter Alexis. Review of D. Bruce Marshall, *The French Colonial Myth and Constitution-Making in the Fourth Republic.* 1366
Governing the London Region: Reorganization and Planning in the 1960s. Donald Foley. Reviewed by Noel Boaden. 816
Government and Economic Development. Gustav Ranis, ed. Reviewed by Bela Balassa. 1834
Government Management Internships and Executive Development. Thomas P. Murphy, ed. Reviewed by Raymond Pomerleau. 1772
Government and Politics of Korea. Se-Jin Kim and Chang-Hyun Cho, eds. Reviewed by Gregory Henderson. 825
Government Publications and Their Use, 2nd rev. ed. Laurence F. Schmeckebier and Roy B. Eastin. Reviewed by John Brown Mason. 1753
Grafton, Carl. Review of Michael D. Usdan, David W. Minar, and Emanuel Hurwitz, Jr., *Education and State Politics.* 794
Graham, Lawrence S. Review of Roger D. Hansen, *The Politics of Mexican Development.* 820
Graham, Otis L. Jr., *The Great Campaigns: Reform and War in America, 1900–1928.* Re-

viewed by Richard Weiss. 783
Gray, Virginia. *Communication* in reply to comment on her article "Innovation in the States: A Diffusion Study" (December, 1973). 1682
The Great Campaigns: Reform and War in America, 1900–1928. Otis L. Graham, Jr. Reviewed by Richard Weiss. 783
Greece and the Entente: August 1, 1914-September 25, 1916. Christos Theodoulou. Reviewed by D. George Kousoulas. 1883
Greek and Roman Voting and Elections. E. S. Staveley. Reviewed by Charles D. Hamilton. 847
Greenberg, Milton and Jack C. Plano. *The American Political Dictionary*, 3rd ed. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
Greene, Jack P. and Robert Forster, eds. *Preconditions of Revolution in Early Modern Europe.* The Johns Hopkins Symposia in History. Reviewed by Philip Dawson. 1803
Greene, Jack P. Review of Michael Kammen, *People of Paradox: An Inquiry Concerning the Origins of American Civilization.* 1331
Greene, Kenneth R. Review of Kenneth J. Pollinger and Annette C. Pollinger, *Community Action and the Poor: Influence vs. Social Control in a New York City Community.* 286
The Green Flag: The Turbulent History of the Irish National Movement. Robert Kee. Reviewed by Brian Farrell. 824
Greenfield, Sidney M. and Arnold Strickon, eds. *Structure and Process in Latin America: Patronage, Clientage, and Power System.* Reviewed by Steffen Schmidt. 1377
Greenstein, Fred I. *Communication* on Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, "The Structuring Principle: Political Socialization and Belief Systems" (June, 1973). 720
Review of Stuart Gerry Brown, *The Presidency on Trial: Robert Kennedy's 1968 Campaign and Afterwards.* 779
Greer, Thomas H. Review of Rexford G. Tugwell, *In Search of Roosevelt.* 793
Gregor, A. James. *Communication* in reply to criticism of his review essay "On Understanding Fascism: A Review of Some Contemporary Literature" (December, 1973). 1686
Review of Walter C. Langer, *The Mind of Adolf Hitler;* and Robert Payne, *The Life and Death of Adolf Hitler.* 317
Griffiths, R. A., S. B. Chrimes, and C. D. Ross, eds. *Fifteenth Century England, 1399–1509: Studies in Politics and Society.* Reviewed by Arthur Joseph Slavin. 1797
Grimsted, Patricia K. Review of *Book Publishing in the USSR: Reports of the Delegations of U.S. Book Publishers Visiting the USSR, October 21–November 4, 1970, August 20–September 17, 1962,* 2nd ed., enlarged. 1379
Grofman, Bernard. Review of John A. Sohnquist, *Multivariate Model Building: The Validation of a Search Strategy.* 1749
Groom, A. J. R. Review of David Davies Memorial Institute of International Studies, *International Disputes: The Legal Aspects.* 1857
Grosser, Alfred. *L'Explication Politique: Une Introduction à l'Analyse Comparative.* Reviewed by Roy Macridis. 1736
Germany in Our Time: A Political History of the Postwar Years. Reviewed by Catherine McArdle Kelleher. 1808
Gross, John, ed. *The Expansion of England.* J. R. Seeley. Reviewed by A. L. Burns. 844
The Guarantee Clause of the U.S. Constitution. William M. Wiecek. Reviewed by Robert J. Morgan. 1344
Guide to U.S. Publications. John L. Andriot. Reviewed by John Brown Mason. 1753
Gukiina, Peter M. *Uganda: A Case Study in African Political Development.* Reviewed by James H. Mittelman. 303
Gurr, Ted Robert. *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* Review of Arthur S. Banks, assembler, *Cross-Polity Time-Series Data;* Donald G. Morrison, Robert C. Mitchell, John N. Paden, Hugh M. Stevenson, and others, *Black Africa: A Comparative Handbook;* J. David Singer and Melvin Small, *The Wages of War, 1816–1965: A Statistical Handbook;* Charles Lewis Taylor, Michael C. Hudson, and others, *World Handbook of Political and Social Indicators,* 2nd ed. 243
Persistence and Change in Political Systems, 1800–1971. 1482
Gutiérrez, Carlos María. *The Dominican Republic: Rebellion and Repression.* Reviewed by Howard J. Wiarda. 1809
Gwyn, William B. Review of Stanley V. Anderson and John E. Moore, eds. *Establishing Ombudsman Offices: Recent Experience in the United States.* Transcript of the Ombudsman WORKSHOP, Honolulu, Hawaii, May 5–7, 1971. 1752

Haas, Ernst B. Review of Ghita Ionescu, ed., *The New Politics of European Integration.* 349
Hackforth, R., trans. *Plato's Phaedo, Plato's Phaedrus, Plato's Philebus* (3 vols. with commentary). Reviewed by Thomas L. Pangle. .. 258
Hafner, Donald L. Review of Yehezkel Dror, *Crazy States: A Counterconventional Strategic Problem.* 342
Hagan, Charles B. Review of Paul F. Kress, *Social Science and the Idea of Process: The Ambiguous Legacy of Arthur F. Bentley.* 764
Hakes, Jay E. Review of Edward W. Soja, *The Geography of Modernization in Kenya: A*

Spatial Analysis of Social, Economic, and Political Change. 331
Hall, D. G. E. Review of C. P. FitzGerald, *The Southern Expansion of the Chinese People.* ... 1802
Hamilton, Charles D. Review of E. S. Staveley, *Greek and Roman Voting and Elections.* 847
Hamilton, Virginia Van Der Veer. *Hugo Black: The Alabama Years.* Reviewed by R. H. Clark. 1767
Hancock, M. Donald. Review of Ragnar Simonsson, *Frankrikes författningar.* 845
Handbook of Organizational Measurement. James L. Price. Reviewed by Lawrence B. Mohr. 1746
Hansen, Roger D. *The Politics of Mexican Development.* Reviewed by Lawrence S. Graham. 820
Han, Sungjoo. Review of Hesung Chun Koh and Joan Steffens, eds., *Korea: An Analytical Guide to Bibliographies.* 827
Harik, Iliya. Review of Majid Khadduri, *Arab Contemporaries: The Role of Personalities in Politics*, and *Political Trends in the Arab World: The Role of Ideas and Ideals in Politics.* 1818
Harik, Iliya and Richard Antoun, eds. *Rural Politics and Social Change in the Middle East.* Reviewed by Douglas E. Ashford. 1345
Harmon, Robert B. *The Arts and Practice of Diplomacy: A Selected and Annotated Guide.* Reviewed by Elmer Plischke. 347
Harris, George S. *Troubled Alliance: Turkish-American Problems in Historical Perspective, 1945–1971.* Reviewed by Richard H. Pfaff. .. 348
Harris, Nigel. *Competition and the Corporate Society: British Conservatives, the State, and Industry, 1945–1964.* Reviewed by S. C. Ghosh. 1357
Harrison, Bennett. *Education, Training, and the Urban Ghetto.* Reviewed by JeDon A. Emenhiser. 1328
Harrison, James Pinckney. *The Long March to Power: A History of the Chinese Communist Party, 1921–72.* Reviewed by Ezra F. Vogel. 1810
Harrison, Martin. Review of Maurice Cowling, *The Impact of Labour, 1920–1924.* 810
Harsanyi, John C. *Communication* in reply to comment on his review of Nigel Howard, *Paradoxes of Rationality: Theory of Metagames and Political Behavior* (June, 1973). .. 730
Communication in reply to criticism of his further discussion of his review of Nigel Howard, *Paradoxes of Rationality: Theory of Metagames and Political Behavior* (June, 1974). 1694
Hartman, Robert W. and Robert D. Reischauer, with the assistance of Daniel J. Sullivan. *Reforming School Finance.* Reviewed by David Stern. 1778
Hartung, Dirk, Reinhard Nuthmann, and Dietrich Winterhager. *Politologen im Beruf. Zur Aufnahme und Durchsetzung neuer Qualifikationen im Beschäftigungssystem.* Reviewed by John Dreijmanis. 304
Harvard, William C., ed. *The Changing Politics of the South.* Reviewed by Alexander Heard. 278
Harvest of Death. J. B. Neilands, G. H. Orians, E. W. Pfeiffer, Alje Vennema, and Arthur H. Westing. Reviewed by George H. Quester in *Chemical and Biological Warfare.* 1285
Haviland, H. Field. Review of Robert W. Cox, Harold K. Jacobson, Gerard and Victoria Curzon, Joseph S. Nye, Lawrence Scheinman, James P. Sewell, and Susan Strange, *The Anatomy of Influence: Decision-Making in International Organization.* 1389
Hayes, Margaret Daly. Review of Raouf Kahil, *Inflation and Economic Development in Brazil, 1946–1963.* 1815
Hayward, Fred M. Review of Alvin Rabushka and Kenneth A. Shepsle, *Politics in Plural Societies: A Theory of Democratic Instability.* 841
Head, Kendra B., John P. Robinson, and Jerrold G. Rusk, eds. *Measures of Political Attitudes.* Reviewed by David E. RePass. 771
Heard, Alexander. Review of William C. Havard, ed., *The Changing Politics of the South.* 278
Heath, Shirley Brice. *Telling Tongues: Language Policy in Mexico, Colony to Nation.* Reviewed by Martin C. Needler. 821
Heberle, Rudolf and Werner J. Cahnmann, eds. *Ferdinand Toennies on Sociology: Pure, Applied and Empirical. Selecting Writings.* Reviewed by Donald A. Nielsen. 1728
Hegel. Raymond Plant. Reviewed by Judith N. Shklar. 1744
Hegel's Critique of Liberal Theories of Rights. Peter G. Stillman. 1086
Heindel, Richard H. Review of George C. Herring, Jr., *Aid to Russia, 1941–1946: Strategy, Diplomacy, the Origins of the Cold War.* 867
Heineman, Benjamin W., Jr. *The Politics of the Powerless: A Study of the Campaign Against Discrimination.* Reviewed by Richard P. Longaker. 1783
Hellmann, Donald C. *Japan and East Asia: The New International Order.* Reviewed by Willard H. Elsbree. 1395
Helvétius and the Roots of the "Closed" Society. Blair Campbell. 153
Henderson, Gregory. Review of David C. Cole and Princeton N. Lyman, *Korean Development: The Interplay of Politics and Economics.* 297
Review of Se-Jin Kim and Chang-Hyun Cho, eds., *Government and Politics of Korea.* 825
Henderson, Keith M. Review of Lewis Anthony Dexter, *Elite and Specialized Interviewing.* ... 752
Henningsen, Manfred. Review of Gisela Hersch, *A Bibliography of German Studies, 1945–1971.* 305
Henshaw, Edmund L., Jr. and Paul A. Theis, eds. *Who's Who in American Politics: 1973–1974,*

4th ed. Reviewed by Charles O. Jones. 1339
Hensler, Deborah, Frederick M. Wirt, Benjamin Walter, and Francine Rabinovitz. *On the City's Rim: Politics and Policy in Suburbia.* Reviewed by John H. Baker. 291
Herbst, Jurgen. Review of Goldwyn Smith, compiler, *The Professor and the Public: The Role of the Scholar in the Modern World.* Essays by Goldwin Smith, A. L. Rowse, and J. H. Hexter. 267
Herbst, P. G. *Behavioural Worlds: The Study of Single Cases.* Reviewed by Steven Doehrman. 758
Hermann, Charles F. *International Crises: Insights from Behavioral Research.* Reviewed by P. Terrence Hopmann. 1861
Hermet, Guy. *La Politique dans L'Espagne Franquiste.* Reviewed by Raphael Zariski. ... 822
Hero, Alfred O., Jr. *American Religious Groups View Foreign Policy: Trends in Rank-and-File Opinion, 1937–1969.* Reviewed by Kenneth W. Thompson. 1863
Herring, George C., Jr. *Aid to Russia, 1941–1946: Strategy, Diplomacy, the Origins of the Cold War.* Reviewed by Richard H. Heindel. 867
Hersch, Gisela. *A Bibliography of German Studies, 1945–1971.* Reviewed by Manfred Henningsen. 305
Herz, John H. Review of Roger Morgan, ed., *The Study of International Affairs: Essays in Honour of Kenneth Younger.* 1404
Heseltine, Nigel. *Madagascar.* Reviewed by L. Gray Cowan. 822
Hexter, J. H., Goldwin Smith, and A. L. Rowse. *The Professor and the Public: The Role of the Scholar in the Modern World.* Goldwyn Smith, compiler. Reviewed by Jurgen Herbst. 267
Hiebert, Ray, Robert Jones, John Lorenz, and Ernest Lotito, eds. *The Political Image Merchants.* Reviewed by Max M. Kampelman. .. 784
Hill, Larry B. *Institutionalization, the Ombudsman, and Bureaucracy.* 1075
Hill, Richard Child. *Separate and Unequal: Governmental Inequality in the Metropolis.* 1557
Hilton, Gordon. Review of R. J. Rummel, *The Dimensions of Nations.* 1408
Hinckley, Barbara. *Stability and Change in Congress.* Reviewed by Randall B. Ripley. 280
Hinich, Melvin J., Peter H. Aranson, and Peter C. Ordeshook. *Election Goals and Strategies: Equivalent and Nonequivalent Candidate Objectives.* 135
Hinton, Harold C. Review of Bruce D. Larkin, *China and Africa, 1949–1970: The Foreign Policy of the People's Republic of China.* 353
History of the Sino-Japanese War, 1937–1945. Hsu Long-hsuen and Chang Ming-kai, comps. Reviewed by John H. Boyle. 1869
Hitler and the Beer Hall Putsch. Harold J. Gordon, Jr. Reviewed by C. F. Latour. 300
Hitler: The Man and the Military Leader. Percy Ernst Schramm. Reviewed by Gerhard L. Weinberg. 306
Hitler and The Middle Sea. Walter Ansel. Reviewed by Paul L. Savage. 802
Hitler's War Aims: Ideology, the Nazi State, and the Course of Expansion. Norman Rich. Reviewed by A. J. P. Taylor. 1406
Hitler's Weltanschauung: A Blueprint for Power. Eberhard Jäckel. Reviewed by Gerhard L. Weinberg. 306
Hobbes-Forschungen. Reinhart Koselleck and Roman Schnur, eds. Reviewed by Herbert W. Schneider. 755
Hobbes and His Audience: The Dynamics of Theorizing. Mark Gavre. 1542
Hobbes and Rousseau: A Collection of Critical Essays. Maurice Cranston and Richard S. Peters, eds. Reviewed by C. B. Macpherson. 1730
Hobbes, Thomas. *Man and Citizen.* With an introduction by Bernard Gert, ed. Reviewed by Brian Barry. 759
Hoetink, H. Review of Yogendra K. Malik, *East Indians in Trinidad: A Study in Minority Politics.* 1825
Hoffmann, Steven A. Review of B. M. Kaul, *Confrontation With Pakistan.* 351
Hoffman, Robert L. *Revolutionary Justice: The Social and Political Theory of P.-J. Proudhon.* Reviewed by Charles E. Butterworth. 1737
Holborn, Hajo. *Republic to Reich: The Making of the Nazi Revolution.* Reviewed by Gerhard L. Weinberg. 306
Holbrik, Karel and Philip L. Swan. *Trade and Industrialization in the Central American Common Market, the First Decade.* Reviewed by Melville E. Osborne. 824
Hollander, Paul. *Soviet and American Society: A Comparison.* Reviewed by Zygmunt Bauman. 1811
Holocaust or Hemispheric Co-Op: Cross Currents in Latin America. William O. Douglas. Reviewed by Edward S. Milenky. 861
Holsti, Ole R. Review of David P. Calleo and Benjamin M. Rowland, *America and the World Political Economy.* 1385
Review of Joseph M. Siracusa, *New Left Diplomatic Histories and Historians: The American Revisionists.* 1878
The Study of International Politics Makes Strange Bedfellows: Theories of the Radical Right and the Radical Left. 217
Hopkins, Harry L. *Spending to Save: The Complete Story of Relief.* Reviewed by Frank T. Colon. 786
Hopkins, Nicholas S. *Popular Government in an African Town: Kita, Mali.* Reviewed by William J. Foltz. 1358
Hopmann, P. Terrence. Review of Charles F.

Hermann, *International Crises: Insights from Behavioral Research*. ... 1861
Hopwood, Derek, ed. *The Arabian Peninsula: Society and Politics*. Reviewed by John G. Merriam. ... 1359
Horkheimer, Max and Theodor W. Adorno. *Dialectic of Enlightenment*. John Cumming, trans. Reviewed by Hiram Caton. ... 1307
Horowitz, Irving Louis. *Foundations of Political Sociology*. Reviewed by Richard Ashcraft. ... 761
Houn, Franklin W. Review of Charles Price Ridley, Paul H. B. Godwin, and Dennis J. Doolin, *The Making of a Model Citizen in Communist China*. ... 1835
Howard, Dick. *The Development of the Marxian Dialectic*. Reviewed by Z. A. Jordan. ... 1308
Howard, Dick and Karl Klare, eds. *The Unknown Dimension: European Marxism Since Lenin*. Reviewed by James R. Ozinga. ... 762
Howard, Nigel. *Communication* on review of his book *Paradoxes of Rationality: Theory of Metagames and Political Behavior* (June, 1973). 729
Communication in reply to further discussion of his book *Paradoxes of Rationality: Theory* of Metagames and Political Behavior (June, 1974). ... 1692
How the Bureaucracy Makes Foreign Policy: An Exchange Analysis. David Howard Davis. Reviewed by Fred A. Kramer. ... 781
Hsüeh, Chün-tu, ed. *Revolutionary Leaders of Modern China*. Reviewed by Robert E. Bedeski. ... 307
Hsu, Kuang-liang and Stewart E. Fraser. *Chinese Education and Society: A Bibliographic Guide*. Reviewed by Merle Goldman. ... 1804
Hsu Long-hsuen and Chang Ming-kai, comps. *History of the Sino-Japanese War, 1937–1945*. Review by John H. Boyle. ... 1869
Huck, Arthur. Review of Wen-shun Chi, comp. and ed., *Readings in the Chinese Communist Cultural Revolution*. ... 810
Hudson, Michael C., Charles Lewis Taylor, and others. *World Handbook of Political and Social Indicators*, 2nd ed. Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science*. ... 243
Hudson, W. J., ed. *Australia and Papua New Guinea*. Reviewed by Francis West. ... 308
Hugo Black: The Alabama Years. Virginia Van Der Veer Hamilton. Reviewed by R. H. Clark. 1767
Huizer, Gerrit. *The Revolutionary Potential of Peasants in Latin America*. Reviewed by Enrique Baloyra. ... 1812
The Human Basis of the Polity: A Psychological Study of Political Men. Jeanne E. Knutson. Reviewed by Ira Rohter. ... 260
Hungary and the Superpowers: The 1956 Revolution and Realpolitik. János Radványi. Reviewed by Kurt L. London. ... 360
Hunter, Robert. *Security in Europe*. Reviewed by F. S. Northedge. ... 1396
Hurwitz, Emanuel, Jr., Michael D. Usdan, and David W. Minar. *Education and State Politics*. Reviewed by Carl Grafton. ... 794
Huston, James A. Review of T. N. Dupuy and Wendell Blanchard, eds., *The Almanac of World Military Power*. ... 862
Hutson, James H. and Stephen G. Kurtz, eds. *Essays on the American Revolution*. Reviewed by Richard Crosby. ... 1332
Hveem, Helge. *International Relations and World Images: A Study of Norwegian Foreign Policy Elites*. Reviewed by Nils Ørvik. ... 1397
Hyman, Sidney. *Youth in Politics: Expectations and Realities*. Reviewed by Everett Carll Ladd, Jr. ... 281

Iatrides, John O. *Revolt in Athens: The Greek Communist "Second Round," 1944–1945*. Reviewed by Van Coufoudakis. ... 309
The Idea of Culture in the Social Sciences. Louis Schneider and Charles Bonjean, eds. Reviewed by Harry C. Triandis. ... 1318
Ihara, Randal H. Review of Julian H. Franklin, *Jean Bodin and the Rise of Absolutist Theory*. 1306
The Impact of Government Subsidies on Industrial Management: The Israeli Experiment. Nachum Finger. Reviewed by Amos Perlmutter. ... 815
The Impact of Labour, 1920–1924. Maurice Cowling. Reviewed by Martin Harrison. ... 810
Implementation: How Great Expectations in Washington are Dashed in Oakland; Or, Why It's Amazing that Federal Programs Work at All, This Being a Saga of the Economic Development Administration as Told by Two Sympathetic Observers Who Seek to Build Morals on a Foundation of Ruined Hopes. Jeffrey L. Pressman and Aaron Wildavsky. Reviewed by Martha Derthick. ... 1336
In Defence of Politics, 2nd ed. Bernard Crick. Reviewed by James H. Nichols, Jr. ... 1731
Individual and Contextual Variables in British Voting Behavior: An Exploratory Note. C. Neal Tate. ... 1656
Industrial Management in the Soviet Union. William J. Conyngham. Reviewed by Elizabeth Clayton. ... 1799
Industry and Government in Britain: The Federation of British Industries in Politics, 1945–65. Stephen Blank. Reviewed by Graham Wootton. ... 1794
Inflation and Economic Development in Brazil, 1946–1963. Raouf Kahil. Reviewed by Margaret Daly Hayes. ... 1815
The Influence of Parents on the Political Attitudes of Adolescents. Kent L. Tedin. ... 1579
Innovation In New Communities. Brown Miller,

Neil J. Pinney, and William S. Saslow. Reviewed by Thomas P. Murphy. 788
In Search of Roosevelt. Rexford G. Tugwell. Reviewed by Thomas H. Greer. 793
Institut d'Études Politiques de Bordeaux, under the direction of Albert Mabileau. *Les Facteurs locaux de la vie politique nationale.* Reviewed by William R. Schonfeld. 310
Institutionalization, the Ombudsman, and Bureaucracy. Larry B. Hill. 1075
Institution Building: A Model for Applied Social Change. D. Woods Thomas, Harry R. Potter, William L. Miller, and Adrian F. Aveni, eds. Reviewed by Seymour B. Sarason. 268
Institutions and Social Exchange: The Sociologies of Talcott Parsons and C. Homans. Herman Turk and Richard L. Simpson, eds. Reviewed by Gianfranco Poggi. 773
Integration of the Negro in the U.S. Armed Forces. Richard J. Stillman, II. Reviewed by William F. Levantrosser. 1758
The International Air Transport Association. Richard Y. Chuang. Reviewed by Robert L. Thornton. 857
The International Coffee Agreement: A Study in Coffee Diplomacy. Bart S. Fisher. Reviewed by Arthur J. Cordell. 865
International Crises: Insights from Behavioral Research. Charles F. Hermann. Reviewed by P. Terrence Hopmann. 1861
International Disputes: The Legal Aspects. David Davies Memorial Institute of International Studies. Reviewed by A. J. R. Groom. 1857
International Economic Reform: Collected Papers of Emile Despres. Gerald M. Meier, ed. Reviewed by Robert E. Asher. 1403
The International Law of Civil War. Richard A. Falk, ed. Reviewed by Oliver J. Lissitzyn. ... 345
International Law: Collected Papers of Hersch Lauterpacht. Vol. I: General Works. E. Lauterpacht, ed. Reviewed by Louis B. Sohn. 1865
International Relations and World Images: A Study of Norwegian Foreign Policy Elites. Helge Hveem. Reviewed by Nils Ørvik. 1397
Intervention and Negotiation: The United States and the Dominican Revolution. Jerome Slater. Reviewed by Michael J. Kryzanek. 362
The Introduction of Socialism into China. Li Yuning. Reviewed by Harold Z. Schiffrin..... 854
Ionescu, Ghita, ed. *The New Politics of European Integration.* Reviewed by Ernst B. Haas. 349
The Irish Judiciary. Paul C. Bartholomew. Reviewed by John H. Whyte. 803
The Irish Political Elite. Al Cohan. Reviewed by John H. Whyte. 803
Irish Political Parties: An Introduction. Maurice Manning. Reviewed by John H. Whyte. 803
Iriye, Akira. *Pacific Estrangement: Japanese and American Expansion, 1897–1911.* Reviewed by Warren I. Cohen. 350
Iskenderov, A., Y. Zhukov, L. Delyusin, and L. Stepanov. *The Third World: Problems and Prospects.* Reviewed by Edward Taborsky. .. 889
Jäckel, Eberhard. *Hitler's Weltanschauung: A Blueprint for Power.* Reviewed by Gerhard L. Weinberg. 306
Jacob, Herbert. Review of Theodore L. Becker, *Comparative Judicial Politics;* and Laura Nader, ed., *Law in Culture and Society.* 292
Jacobson, Harold K. Review of Kenneth J. Twitchett, ed., *The Evolving United Nations: A Prospect for Peace.* 884
Jacobson, Harold K., Robert W. Cox, Gerard and Victoria Curzon, Joseph S. Nye, Lawrence Scheinman, James P. Sewell, and Susan Strange. *The Anatomy of Influence: Decision-Making in International Organization.* Reviewed by H. Field Haviland. 1389
Jahan, Rounaq. *Pakistan: Failure in National Integration.* Reviewed by Lawrence Ziring. .. 311
Jancar, Barbara Wolfe. *Czechoslovakia and the Absolute Monopoly of Power: A Study of Political Power in a Communist System.* Reviewed by Galia Golan. 1813
Japan and East Asia: The New International Order. Donald C. Hellmann. Reviewed by Willard H. Elsbree. 1395
Jean Bodin and the Rise of Absolutist Theory. Julian H. Franklin. Reviewed by Randal H. Ihara. 1306
Jennings, R. Y. and Sir Humphrey Waldock, eds. *The British Year Book of International Law, 1970, Vol. 44.* Reviewed by Oliver J. Lissitzyn. 887
Jensen, Lloyd. Review of Raymond Tanter and Richard H. Ullman, eds., *Theory and Policy in International Relations.* 363
Jessup, Philip C. Review of Andrew W. Cordier and Wilder Foote, eds., *Public Papers of the Secretaries-General of the United Nations,* 3 vols. 1855
Jewish Nationality and Soviet Politics. Z. Y. Gitelmann. Reviewed by Lionel Kochan. ... 1805
Jewish Reactions to German Anti-Semitism, 1870–1914. Ismar Schorsch. Reviewed by Fritz Nova. 843
Jha, Shashishekhar. *Political Elite in Bihar.* Reviewed by Douglas C. Smyth. 1361
John Dewey's Philosophy of Value. James Gouinlock. Reviewed by Edward B. Portis. 1735
Johnson, Bruce D. *Marihuana Users and Drug Subcultures.* Reviewed by Joseph L. Zentner. 1329
Johnson, Chalmers E. *Conspiracy at Matsukawa.* Reviewed by Kenneth E. Colton. 312
Johnson, Donald Bruce and James R. Gibson. *The Divisive Primary Revisited: Party Activists in Iowa.* 67
Johnson, D. H. N. Review of Edward McWhinney, *Aerial Piracy and International Law.* 355

hnson, Marilyn, Matilda White Riley, and John W. Riley, Jr. *Aging and Society*, Vol. II: *Aging and the Professions;* Marilyn Johnson, Matilda White Riley and Anne Foner. *Aging and Society*, Vol. III: *A Sociology of Age Stratification*. Reviewed by Frederick R. Eisele. 262
hns, Sheridan W., III. Review of Christian P. Potholm, *Swaziland: The Dynamics of Political Modernization*. 1833
hnston, Jerome and Jerald G. Bachman. *Young Men Look at Military Service: A Preliminary Report*. Reviewed by James M. Gerhardt. 1768
Youth in Transition, Vol. V: *Young Men and Military Service*. Reviewed by Jarol B. Manheim. 1296
hnston, Lloyd D., Jerald G. Bachman, Robert L. Kahn, Martha T. Mednick, and Terrence N. Davidson. *Youth in Transition*, Vol. I: *Blueprint for a Longitudinal Study of Adolescent Boys*. Reviewed by Jarol B. Manheim. .. 1296
ll, James and David E. Apter, eds. *Anarchism Today*. Reviewed by Stanley Rothman. 1346
nas, Manfred. Review of A. Russell Buchanan, ed., *The United States and World War II: Military and Diplomatic Documents*. 780
nes, Charles O. Review of Paul A. Theis and Edmund L. Henshaw, Jr., eds., *Who's Who in American Politics: 1973–1974*, 4th ed. 1339
nes, Robert, Ray Hiebert, John Lorenz, and Ernest Lotito, eds. *The Political Image Merchants*. Reviewed by Max M. Kampelman. .. 784
rdan, Robert S. Review of Edwin Fedder, ed., *NATO in the Seventies*. 864
rdan, Z. A. Review of Dick Howard, *The Development of the Marxian Dialectic*. 1308
Review of R. W. K. Paterson, *The Nihilistic Egoist: Max Stirner*. 766
Review of Saul K. Padover, ed. and arranger, *Karl Marx on Revolution*. 1743
urney to America. Alexis de Tocqueville. George Lawrence, trans. J. P. Mayer, ed. Reviewed by Aldo Tassi. 254
witt, Kenneth. *An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems*. 1171
gendliche und Parteien. Peter Menacher. Reviewed by Peter H. Merkl. 1788
ng, Hwa Yol. Review of Alfred Schutz and Thomas Luckmann, *The Structures of the Life-World*. Richard M. Zaner and H. Tristram Engelhardt, Jr., trans. 1319
ngk, Robert, Ernan McMullin, Joseph Needham, Joan Robinson, Stuart Schram, William Sewell, and Bernard Towers. *China and the West: Mankind Evolving*. Reviewed by John H. Boyle. 1399

idish, M. R. Review of J. Roland Pennock and John W. Chapman, eds., *Political and Legal Obligations, Nomos XII, Yearbook of the American Society for Political and Legal Philosophy*. 768
Kagan, Robert A. Review of Robert D. Cuff, *The War Industries Board: Business-Government Relations During World War I*. 1325
Kahan, Jerome H. Review of Joseph I. Coffey, *Strategic Power and National Security*. 340
Kahil, Raouf. *Inflation and Economic Development in Brazil, 1946–1963*. Reviewed by Margaret Daly Hayes. 1815
Kahin, George Mc.T. Review of David G. Marr, *Vietnamese Anticolonialism: 1885–1925*. 829
Kahn, Robert L., Jerald G. Bachman, Martha T. Mednick, Terrence N. Davidson, and Lloyd D. Johnston. *Youth in Transition*, Vol. I: *Blueprint for a Longitudinal Study of Adolescent Boys*. Reviewed by Jarol B. Manheim. .. 1296
Kammen, Michael. *People of Paradox: An Inquiry Concerning the Origins of American Civilization*. Reviewed by Jack P. Greene. 1331
Kampelman, Max V. Review of Ray Hiebert, Robert Jones, John Lorenz, and Ernest Lotito, eds., *The Political Image Merchants;* William C. Mitchell, *Why Vote?;* Joseph Napolitan, *The Election Game and How to Win It;* Allen Reitman and Robert D. Davidson, *The Election Process, Voting Laws and Procedures;* David Lee Rosenbloom, *The Election Men;* Edward Schwartzman, *Campaign Craftsmanship;* Stephen C. Shadegg, *The New How To Win An Election;* and Robert C. Wood, *The Necessary Majority*. 784
Kanet, Roger E. and Ivan Volgyes, eds. *On the Road to Communism: Essays on Soviet Domestic and Foreign Politics*. Reviewed by Teresa Rakowska-Harmstone. 1816
Kanet, Roger E. Review of Bernard P. Kiernan, *The United States, Communism, and the Emergent World*. 1401
Kantor, Harry. Review of John M. Baines, *Revolution in Peru: Mariátegui and the Myth*. 1348
Kaplan, Morton A. *Communication* on review of his book *On Historical and Political Knowing* (September, 1973). 731
Kapur, Harish. *The Soviet Union and the Emerging Nations: A Case Study of Soviet Policy Towards India*. Reviewed by Bernard S. Morris. 858
Karcz, G. F., ed. *The Private Sector in Soviet Agriculture*, 2nd rev. ed. of *Privatproduzenten in der sowjetischen Landwirtschaft*. Karl-Eugen Wädekin. K. Bush, trans. Reviewed by James R. Millar. 1379
Karis, Thomas. Review of William R. Duggan, *A Socioeconomic Profile of South Africa*. 1391
Karl Marx on Revolution. Saul K. Padover, ed. and arranger. With an introduction and new

translations by the editor. Reviewed by Z. A. Jordan. 1743
Kaspar, Raphael G., ed. *Technology Assessment: Understanding the Social Consequences of Technological Applications.* Reviewed by Victor Ferkiss. 1310
Katz, Abraham. *The Politics of Economic Reform in the Soviet Union.* Reviewed by Frederic L. Pryor. 1817
Katz, Richard S. *Communication* in reply to comment on his article "The Attribution of Variance in Electoral Returns: An Alternative Measurement Technique" (September, 1973). 1273
Kaufman, Robert R. *The Politics of Land Reform in Chile.* Reviewed by Robert L. Ayres..... 313
Kaul, B. M. *Confrontation With Pakistan.* Reviewed by Steven A. Hoffmann. 351
Kaushik, Davendra. *Soviet Relations with India and Pakistan.* Reviewed by Paul Gerhardt. .. 1400
Keddie, Nikki R. *Sayyid Jamal ad-Din "al-Afghani": A Political Biography.* Reviewed by Marvin Zonis. 1310
Keech, William R. Review of Hanes Walton, Jr., *Black Political Parties: An Historical and Political Analysis.* 1341
Kee, Robert. *The Green Flag: The Turbulent History of the Irish National Movement.* Reviewed by Brian Farrell. 824
Keeton, George W. and Georg Schwarzenberger, eds. *The Year Book of World Affairs, 1972.* Reviewed by Patrick J. McGowan. 352
Kelleher, Catherine McArdle. Review of Alfred Grosser, *Germany in Our Time: A Political History of the Postwar Years.* 1808
Kelley, Donald R. *Toward a Model of Soviet Decision Making: A Research Note.* 701
Kelley, Stanley, Jr. and Thad W. Mirer. *The Simple Act of Voting.* 572
Kendall, Nellie D., ed. *Willmoore Kendall Contra Mundum.* With an introduction by Jeffrey Hart. Reviewed by Joseph C. Mouledoux. .. 1738
Kennington, Richard. Review of Patrick Riley, ed. and trans., *The Political Writings of Leibniz* (with an introduction and notes). 263
Kenski, Henry C. and James W. Clarke. *Communication* on Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, "The Structuring Principle: Political Socialization and Belief Systems" (June, 1973). 722
Kenworthy, Eldon. Review of Robert E. Scott, ed., *Latin American Modernization Problems.* 1373
Keohane, Nannerl O. Review of J. M. Roberts, *The Mythology of the Secret Societies.* 265
Keohane, Robert O. Review of Joyce Kolko and Gabriel Kolko, *The Limits of Power: The World and United States Foreign Policy, 1945–1954.* 869
Kern, Robert, ed. *The Caciques: Oligarchical Politics and the System of Caciquismo in the Luso-Hispanic World.* Reviewed by Douglas A. Chalmers. 1362
Khadduri, Majid. *Arab Contemporaries: The Role of Personalities in Politics;* and *Political Trends in the Arab World: The Role of Ideas and Ideals in Politics.* Reviewed by Iliya Harik. 1818
Khare, R. S. *The Changing Brahmans: Associations and Elites among the Kanya-Kubjas of North India.* Reviewed by Roderick Church. 1819
Kiernan, Bernard P. *The United States, Communism, and the Emergent World.* Reviewed by Roger E. Kanet. 1401
Kim, K. H., Susan Farrell, and Ewan Clague. *The All-Volunteer Army: An Analysis of Demand and Supply.* Review by Ryan C. Amacher and Robert D. Tollison. 787
Kim, Se-Jin and Chang-Hyun Cho, eds. *Government and Politics of Korea.* Reviewed by Gregory Henderson. 825
Kin, Clan, Raja and Rule: State-Hinterland Relations in Pre-Industrial India. Richard G. Fox. Reviewed by Marguerite Ross Barnett. 817
King, Anthony. Review of Kenneth Bradshaw and David Pring, *Parliament and Congress.* .. 807
Klare, Karl and Dick Howard, eds. *The Unknown Dimension: European Marxism Since Lenin.* Reviewed by James R. Ozinga. 762
Klieman, Aaron S. *Soviet Russia and the Middle East.* Reviewed by Amos Perlmutter. 1864
Review of Harvey Sicherman, *Aden and British Strategy, 1839–1968.* 1877
Klinov, Ruth. Review of Howard Pack, *Structural Change and Economic Policy in Israel.* .. 324
Klose, Werner. *Lebensformen Deutscher Jugend, vom Wandervogel zur Popgeneration.* Reviewed by Peter H. Merkl. 1788
Knutson, Jeanne E. *The Human Basis of the Polity: A Psychological Study of Political Men.* Reviewed by Ira Rohter. 260
Kochan, Lionel. Review of Z. Y. Gitelmann, *Jewish Nationality and Soviet Politics.* 1805
Koh, Hesung Chun and Joan Steffens, eds. *Korea: An Analytical Guide to Bibliographies.* Reviewed by Sungjoo Han. 827
Kohn, Richard H. Review of Richard Buel, Jr., *Securing the Revolution: Ideology in American Politics, 1789–1815.* 274
Kolankiewicz, George and David Lane, eds. *Social Groups in Polish Society.* Reviewed by Szymon Chodak. 1364
Kolko, Gabriel and Joyce Kolko. *The Limits of Power: The World and United States Foreign Policy, 1945–1954.* Reviewed by Robert O. Keohane. 869
Korbel, Josef. *Détente in Europe: Real or Imaginary?* Reviewed by Kendall Myers. 871
Korbonski, Andrzej. Review of Henry W. Schaefer, *Comecon and the Politics of Integration.* 880

Review of J. S. Nye, *Peace in Parts: Integration and Conflict in Regional Organization*. ... 358
Korea: An Analytical Guide to Bibliographies. Hesung Chun Koh and Joan Steffens, eds. Reviewed by Sungjoo Han. 827
Korean Development: The Interplay of Politics and Economics. David C. Cole and Princeton N. Lyman. Reviewed by Gregory Henderson. 297
Korpi, Walter. *Conflict, Power and Relative Deprivation.* 1569
Kort, Fred. *Communication* in reply to a comment on his article "Regression Analysis and Discriminant Analysis: an Application of R. A. Fisher's Theorem to Data in Political Science" (June, 1973). 203
Koselleck, Reinhart and Roman Schnur, eds. *Hobbes-Forschungen.* Reviewed by Herbert W. Schneider. 755
Kothari, Rajni. *Politics in India.* Reviewed by Myron Weiner. 1363
Kousoulas, D. George. Review of Christos Theodoulou, *Greece and the Entente: August 1, 1914–September 25, 1916.* 1883
Kramer, Daniel C. *Participatory Democracy: Developing Ideals of the Political Left.* Reviewed by Carole Pateman. 1311
Kramer, Fred A. Review of David Howard Davis, *How the Bureaucracy Makes Foreign Policy: An Exchange Analysis.* 781
Krauss, Wilma Rule. *Political Implications of Gender Roles: A Review of the Literature.* 1706
Kress, Paul F. *Social Science and the Idea of Process: The Ambiguous Legacy of Arthur F. Bentley.* Reviewed by Charles B. Hagan. 764
Kryzanek, Michael J. Review of Jerome Slater, *Intervention and Negotiation: The United States and the Dominican Revolution.* 362
Kucherov, Samuel. *The Organs of Soviet Administration of Justice: Their History and Operation.* Reviewed by W. E. Butler. 827
Kurtz, Stephen G. and James H. Hutson, eds. *Essays on the American Revolution.* Reviewed by Richard Crosby. 1332

Labor and Society in Tsarist Russia: The Factory Workers of St. Petersburg, 1855–1870. Reginald E. Zelnik. Reviewed by Richard Pipes. .. 333
Ladd, Everett Carll Jr. Review of Sidney Hyman, *Youth in Politics: Expectations and Realities.* 281
Lakoff, Sanford A. and Maurice Cranston, eds. *A Glossary of Political Ideas.* Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
Land, James W. Review of Leslie L. Roos and Noralou P. Roos, *Managers of Modernization: Organizations and Elites in Turkey (1950–1969).* 328
Land and Social Change in East Nepal: A Study of Hindu-Tribal Relations. Lionel Caplan. Reviewed by Louis Dupree. 809
Landynski, Jacob W. Review of Richard E. Morgan, *The Supreme Court and Religion.* .. 285
Lane, David and George Kolankiewicz, eds. *Social Groups in Polish Society.* Reviewed by Szymon Chodak. 1364
Langdon, Frank C. Review of Robert E. Osgood, *The Weary and the Wary: U.S. and Japanese Security Policies in Transition.* 874
Langer, Paul F. Review of John K. Emmerson, *Arms, Yen and Power: The Japanese Dilemma.* 863
Langer, Walter C. *The Mind of Adolf Hitler.* Reviewed by A. James Gregor. 317
The Language of Cities: A Glossary of Terms. Charles Abrams. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
Laqueur, Walter, ed. *A Dictionary of Politics.* Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
Larkin, Bruce D. *China and Africa, 1949–1970: The Foreign Policy of the People's Republic of China.* Reviewed by Harold C. Hinton. 353
Latham, Earl. *Communication* in reply to criticism of his review of G. William Domhoff, *The Higher Circles: The Governing Class in America* (March, 1972). 1278
Latin American Modernization Problems. Robert E. Scott, ed. Reviewed by Eldon Kenworthy. 1373
Latour, C. F. Review of Max Gallo, *The Night of Long Knives;* and Harold J. Gordon, Jr., *Hitler and the Beer Hall Putsch.* 300
Laumann, Edward O. Review of Anselm L. Strauss, *The Contexts of Social Mobility: Ideology and Theory.* 1781
Launching Social Security: A Capture-and-Record Account, 1935–1937. Charles McKinley and Robert W. Frase. Reviewed by Louis L. Friedland. 777
Lauterpacht, E., ed. *International Law: Collected Papers of Hersch Lauterpacht. Vol. I: General Works.* Reviewed by Louis B. Sohn. 1865
Law in Culture and Society. Laura Nader, ed. Reviewed by Herbert Jacob. 292
Law and the Indo-China War. John Norton Moore. Reviewed by P. E. Corbett. 357
Law and Politics in Outer Space: A Bibliography. Irvin L. White, Clifton E. Wilson, and John A. Vosburgh. Reviewed by Robert H. Puckett. .. 370
Lebensformen Deutscher Jugend, vom Wandervogel zur Popgeneration. Werner Klose. Reviewed by Peter H. Merkl. 1788
Lederer, Ivo and Peter Sugar, eds. *Nationalism in Eastern Europe.* Reviewed by Rudolf L. Tőkés. 849
Lee, Oliver M. Review of Bruce Douglass and Ross Terrill, eds., *China and Ourselves: Explorations and Revisions by a New Generation.* 814
Leggett, John C. *Race, Class, and Political Consciousness.* Reviewed by Michael Cohen. 1770

Legislative Recruitment and Political Integration: Patterns of Political Linkage in an Indian State. Richard Sisson and Lawrence Shrader. Reviewed by William L. Richter. 329
Leibniz, Gottfried Wilhelm. *The Political Writings of Leibniz* (with an introduction and notes). Patrick Riley, ed. and trans. Reviewed by Richard Kennington. 263
Lemarchand, Rene. Review of Benoit Verhaegen, *Rébellions au Congo.* 1845
Lemieux, Peter H. *Communication* on Fred Kort, "Regression Analysis and Discriminant Analysis: an Application of R. A. Fisher's Theorem to Data in Political Science" (June, 1973). ... 202
Lenkowsky, Leslie. Review of Edna Bonacich and Robert F. Goodman, *Deadlock in School Desegregation: A Case Study of Inglewood, California.* 272
Lensen, George Alexander. *The Strange Neutrality: Soviet-Japanese Relations during the Second World War, 1941–1945.* Reviewed by Max Beloff. 354
Lerner, Ralph and Muhsin Mahdi, eds. *Medieval Political Philosophy.* Reviewed by Charles E. Butterworth. 765
Leslie, S. Clement. *The Rift in Israel: Religious Authority and Secular Democracy.* Reviewed by Leonard J. Fein. 1820
Lester, Anthony and Geoffrey Bindman. *Race and Law in Great Britain.* Reviewed by Richard P. Longaker. 1783
Letters of Louis D. Brandeis: Vol. I: *1870–1907, Urban Reformer;* Vol. II: *1907–1912, People's Attorney;* Vol. III: *1913–1915, Progressive and Zionist.* Melvin I. Urofsky and David W. Levy, eds. Reviewed by Robert G. Seddig. ... 1340
Levantrosser, William F. Review of Leo Bogart, ed., *Social Research and the Desegregation of the U.S. Army;* Richard M. Dalfiume, *Desegregation of the U.S. Armed Forces: Fighting on Two Fronts, 1939–1953;* Richard J. Stillman, II, *Integration of the Negro in the U.S. Armed Forces;* and J. N. Wolfe and John Erickson, eds., *The Armed Services and Society.* 1758
Levels of Sociocultural Development Among Soviet Nationalities: A Partial Test of the Equalization Hypothesis. Brian Silver. 1618
Lever, H. *The South African Voter.* Reviewed by Pierre L. van den Berghe. 318
Levine, Charles H. Review of Gary L. Wamsley and Mayer N. Zald, *The Political Economy of Public Organizations: A Critique and Approach to the Study of Public Administration.* 795
Levy, David W. and Melvin I. Urofsky, eds. *Letters of Louis D. Brandeis:* Vol. I: *1870–1907, Urban Reformer;* Vol. II: *1907–1912, People's Attorney;* Vol. III: *1913–1915, Progressive and Zionist.* Reviewed by Robert G. Seddig. 1340
Levy, Marion J., Jr. *Modernization: Latecomers and Survivors.* Reviewed by Lucian W. Pye. .. 1740
Lewy, Guenter. Review of Frederic Spotts, *The Churches and Politics in Germany.* 1376
Liang, Chin-tung. *General Stilwell in China, 1942–1944: The Full Story.* Reviewed by Allen S. Whiting. 1853
Lichtheim, George. *Europe in the Twentieth Century.* Reviewed by George L. Mosse. 1402
Licklider, Roy E. *Communication* on review of his book *The Private Nuclear Strategists* (December, 1973). 1274
The Life and Death of Adolf Hitler. Robert Payne. Reviewed by A. James Gregor. 317
The Limits of Power: The World and United States Foreign Policy, 1945–1954. Joyce Kolko and Gabriel Kolko. Reviewed by Robert O. Keohane. 869
Lindberg, Leon N. and Stuart A. Scheingold, eds. *Regional Integration: Theory and Research.* Reviewed by David P. Calleo. 1867
Lindell, John O. Review of Peter Baehr, *The Role of a National Delegation in the General Assembly.* 1849
Lindqvist, Sven. *The Shadow: Latin America Faces the Seventies.* Keith Bradfield, trans. Reviewed by W. Raymond Duncan. 1821
Lissitzyn, Oliver J. Review of Richard A. Falk, ed., *The International Law of Civil War.* 345
Review of Sir Humphrey Waldock and R. Y. Jennings, eds., *The British Year Book of International Law, 1970, Vol. 44.* 887
The Literature of Isolationism: A Guide to Non-Interventionist Scholarship, 1930–1972. Justus D. Doenecke. Reviewed by Frank Costigliola. 860
Li Yu-ning. *The Introduction of Socialism into China.* Reviewed by Harold Z. Schiffrin. 854
London, Kurt L. Review of János Radványi, *Hungary and the Superpowers: The 1956 Revolution and Realpolitik.* 360
Longaker, Richard P. Review of Simon Abbott, ed., *The Prevention of Racial Discrimination in Britain;* Benjamin W. Heineman, Jr., *The Politics of the Powerless: A Study of the Campaign Against Discrimination;* and Anthony Lester and Geoffrey Bindman, *Race and Law in Great Britain.* 1783
The Long March to Power: A History of the Chinese Communist Party, 1921–72. James Pinckney Harrison. Reviewed by Ezra F. Vogel. 1810
Lorenz, John, Ray Hiebert, Robert Jones and Ernest Lotito, eds. *The Political Image Merchants.* Reviewed by Max M. Kampelman. .. 784
Lotito, Ernest, Ray Hiebert, Robert Jones, and John Lorenz, eds. *The Political Image Merchants.* Reviewed by Max M. Kampelman. .. 784
Loveland, Anne C. *Emblem of Liberty: The*

Image of Lafayette in the American Mind. Reviewed by Michael McGiffert. 282
Lucas, N. Review of J. F. Short, Jr. and M. E. Wolfgang, eds., *Collective Violence.* 1839
Luckham, Robin. *The Nigerian Military: A Sociological Analysis of Authority and Revolt, 1960–67.* Reviewed by Grady H. Nunn. 1822
Luckmann, Thomas and Alfred Schutz. *The Structures of the Life-World.* Richard M. Zaner and H. Tristram Engelhardt, Jr., trans. Reviewed by Hwa Yol Jung. 1319
Lupo, Alan, Frank Colcord, and Edmund P. Fowler. *Rites of Way: The Politics of Transportation in Boston and the U.S. City.* Reviewed by G. Ross Stephens. 282
Luttwak, Edward. *A Dictionary of Modern War.* Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
Luxenburg, Norman. *Europe Since World War II: The Big Change.* Reviewed by Douglas W. Simon. 872
Lyman, Princeton N. and David C. Cole. *Korean Development: The Interplay of Politics and Economics.* Reviewed by Gregory Henderson. 297
Lyons, Gene M. Review of Marlen Blissett, *Politics in Science.* 1726

Mabileau, Albert and the Institut d'Études Politiques de Bordeaux. *Les Facteurs locaux de la vie politique nationale.* Reviewed by William R. Schonfeld. 310
McCormick, Richard P. Review of William R. Brock, *The Evolution of American Democracy.* 273
Macfarlane, Leslie J. Review of Richard E. Flathman, *Political Obligation.* 1305
MacFarquhar, Roderick, ed. *Sino-American Relations, 1949–1971.* Reviewed by Allen S. Whiting. 1853
McGiffert, Michael. Review of Anne C. Loveland, *Emblem of Liberty: The Image of Lafayette in the American Mind.* 282
McGowan, Patrick J. Review of George W. Keeton and Georg Schwarzenberger, eds., *The Year Book of World Affairs, 1972.* 352
McGregor, Eugene B., Jr. *Politics and the Career Mobility of Bureaucrats.* 18
MacIntyre, Donald. *The Naval War Against Hitler.* Reviewed by Paul L. Savage. 802
McKay, Vernon. Review of Helen D. Cohn, *Soviet Policy toward Black Africa: The Focus on National Integration.* 1388
McKinley, Charles and Robert W. Frase. *Launching Social Security: A Capture-and-Record Account, 1935–1937.* Reviewed by Louis L. Friedland. 777
McKinnon, Ronald I. *Money and Capital in Economic Development.* Reviewed by W. W. Rostow. 1822
Mackler, Ian. *Pattern for Profit in Southern Africa.* Reviewed by Julian R. Friedman. ... 1824
McMullin, Ernan, Robert Jungk, Joseph Needham, Joan Robinson, Stuart Schram, William Sewell, and Bernard Towers. *China and the West: Mankind Evolving.* Reviewed by John H. Boyle. 1399
Macpherson, C. B. Review of Maurice Cranston and Richard S. Peters, eds., *Hobbes and Rousseau: A Collection of Critical Essays.* 1730
Macridis, Roy. Review of Alfred Grosser, *L'Explication Politique: Une Introduction a l'Analyse Comparative.* 1736
McWhinney, Edward. *Aerial Piracy and International Law.* Reviewed by D. H. N. Johnson. 355
Madagascar. Nigel Heseltine. Reviewed by L. Gray Cowan. 822
Magid, Alvin. Review of Ikenna Nzimiro, *Studies in Ibo Political Systems: Chieftaincy and Politics in Four Niger States.* 1831
Maguire, John. *Marx's Paris Writings: An Analysis.* With an introduction by David McLellan. Reviewed by Alfred G. Meyer. ... 1741
Mahdi, Muhsin and Ralph Lerner, eds. *Medieval Political Philosophy.* Reviewed by Charles E. Butterworth. 765
Maier, Pauline. *From Resistance to Revolution: Colonial Radicals and the Development of American Opposition to Britain, 1765–1776.* Reviewed by Lance Banning. 284
Mair, L. B. *Australia in New Guinea,* 2nd ed. Reviewed by C. D. Rowley. 828
Majority Party Leadership and the Committee System in the House of Representatives. Louis P. Westefield. 1593
The Making of the Australian Constitution. J. A. La Nauze. Reviewed by G. C. Bolton. 315
Making Federalism Work: A Study of Program Coordination at the Community Level. James L. Sundquist with the collaboration of David W. Davis. Reviewed by Vincent Ostrom. 792
The Making of a Model Citizen in Communist China. Charles Price Ridley, Paul H. B. Godwin and Dennis J. Doolin. Reviewed by Franklin W. Houn. 1835
Malik, Yogendra K. *East Indians in Trinidad: A Study in Minority Politics.* Reviewed by H. Hoetink. 1825
Malloy, James M. *Bolivia: The Uncompleted Revolution.* Reviewed by Robert J. Alexander. 1825
Managers of Modernization: Organizations and Elites in Turkey (1950–1969). Leslie L. Roos and Noralou P. Roos. Reviewed by James W. Land. 328
Man and Citizen. Thomas Hobbes. With an introduction by Bernard Gert, ed. Reviewed by Brian Barry. 759
Manheim, Jarol B. Review of Jerald G. Bachman et al., *Youth in Transition,* Vols. I, II, and

V; and Jerald G. Bachman and Elizabeth Van Duinen, *Youth Looks At National Problems: A Special Report from the Youth in Transition Project.* 1296
Mann, Dean E. Review of James M. Fallows, *The Water Lords.* 1762
Manning, Maurice. *Irish Political Parties: An Introduction.* Reviewed by John H. Whyte. .. 803
Mann, Thomas E., Arthur G. Stevens, Jr. and Arthur H. Miller. *Mobilization of Liberal Strength in the House, 1955–1970: The Democratic Study Group.* 667
Mansfield, Peter, ed. *The Middle East: A Political and Economic Survey,* 4th ed. Reviewed by Robert Owen Freedman. 1365
Mao Tse-tung's Cultural Revolution. Tai Sung An. Reviewed by C. P. FitzGerald. 799
Marihuana Users and Drug Subcultures. Bruce D. Johnson. Reviewed by Joseph L. Zentner. . 1329
Markowitz, Norman. *The Rise and Fall of the People's Century: Henry Agard Wallace and American Liberalism, 1941–1948.* Reviewed by Edward L. Schapsmeier. 1333
Marmion, Harry A. *The Case Against a Volunteer Army.* Reviewed by Ryan C. Amacher and Robert D. Tollison. 787
Marquette, Jesse F. *Social Change and Political Mobilization in the United States: 1870–1960.* 1058
Marr, David G. *Vietnamese Anticolonialism: 1885–1925.* Reviewed by George Mc.T. Kahin. 829
Marshall, D. Bruce. *The French Colonial Myth and Constitution-Making in the Fourth Republic.* Reviewed by Peter Alexis Gourevitch. 1366
Marshall, Richard H. Jr., ed. *Aspects of Religion in the Soviet Union, 1917–1967.* Thomas E. Bird and Andrew Q. Blane, assoc. eds. Reviewed by John A. Armstrong. 1827
Marx, Karl. *Karl Marx on Revolution.* Saul K. Padover, ed. and arranger. With an introduction and new translations by the editor. Reviewed by Z. A. Jordan. 1743
Marx's Paris Writings: An Analysis. John Maguire. With an introduction by David McLellan. Reviewed by Alfred G. Meyer. ... 1741
Mason, Henry L. Review of Harold Sprout and Margaret Sprout, *Toward a Politics of the Planet Earth.* 883
Mason, John Brown. Review of Laurence F. Schmeckebier and Roy B. Eastin, *Government Publications and Their Use,* 2nd rev. ed.; Alexander C. Body, *Annotated Bibliography of Bibliographies on Selected Government Publications and Supplementary Guides to the Superintendent of Documents Classification System;* and John L. Andriot, *Guide to U.S. Publications.* 1753
Mathews, Thomas. Review of Herbert Corkran, Jr., *Patterns of International Cooperation in the Caribbean, 1942–1969.* 341
The Mature Society. Dennis Gabor. Reviewed by Kenneth W. Thompson. 756
Maung, Mya. *Burma and Pakistan: A Comparative Study of Development.* Reviewed by Frank N. Trager. 1828
Maurice Barrès et le nationalisme français. Zeev Sternhell. Reviewed by Eugen Weber. .. 848
Mayer, J. P., ed. Alexis de Tocqueville, *Journey to America.* George Lawrence, trans. Reviewed by Aldo Tassi. 254
Review of Hugh Brogan, *Tocqueville.* 1299
May, Judith V. Review of David R. Godschalk, *Participation, Planning, and Exchange in Old and New Communities: A Collaborative Paradigm.* 1766
Mazour, Anatole G. *The Writing of History in the Soviet Union.* Reviewed by George Enteen. 319
Mazrui, Ali A. *Cultural Engineering and Nation-Building in East Africa.* Reviewed by John Nellis. 831
Review of Eugene Victor Walter, *Terror and Resistance: A Study of Political Violence, with Case Studies of Some Primitive African Communities.* 271
Meagher, Robert E., ed. *Toothing Stones: Rethinking the Political.* Reviewed by D. M. White. 1313
Means, Gordon P. Review of William A. Douglas, *Developing Democracy.* 813
The Measurement of Party Competition. David J. Elkins. 682
Measures of Political Attitudes. John P. Robinson, Jerrold G. Rusk, and Kendra B. Head, eds. Reviewed by David E. RePass. 771
The Mechanics of Independence: Patterns of Political and Economic Transformation in Trinidad and Tobago. A. N. R. Robinson. Reviewed by Wendell Bell. 326
The Mediating Influence of State Legislatures on the Linkage Between Interparty Competition and Welfare Policies. Edward G. Carmines. .. 1118
Medieval Political Philosophy. Ralph Lerner and Muhsin Mahdi, eds. Reviewed by Charles E. Butterworth. 765
Mednick, Martha T., Jerald G. Bachman, Robert L. Kahn, Terrence N. Davidson, and Lloyd D. Johnston. *Youth in Transition,* Vol. I: *Blueprint for a Longitudinal Study of Adolescent Boys.* Reviewed by Jarol B. Manheim. .. 1296
Mehrotra, S. R. *The Emergence of the Indian National Congress.* Reviewed by Paul Wallace. 1367
Meier, Gerald M., ed. *International Economic Reform: Collected Papers of Emile Despres.* Reviewed by Robert E. Asher. 1403
Meiji Japan's Centennial: Aspects of Political Thought and Action. David Wurfel, ed. Reviewed by F. G. Notehelfer. 852
Meisel, John. *Working Papers on Canadian Politics.* Reviewed by Peter Regenstreif. 1369

Melson, Robert. Review of Joseph Okpaku, ed.. *Nigeria: Dilemma of Nationhood, an African Analysis of the Biafran Conflict.* 837
Membership Stability in Three State Legislatures: 1893–1969. David Ray. 106
Menacher, Peter. *Jugendliche und Parteien.* Reviewed by Peter H. Merkl. 1788
Men Versus Systems: Agriculture in the USSR, Poland, and Czechoslovakia. Arthur E. Adams and Jan S. Adams. Reviewed by Sidney Ploss. 798
Merkl, Peter H. Review of Klaus R. Allerbeck and Leopold Rosenmayr, *Aufstand der Jugend? Neue Aspekte der Jugendsoziologie;* Werner Klose, *Lebensformen Deutscher Jugend, vom Wandervogel zur Popgeneration;* and Peter Menacher, *Jugendliche und Parteien.* .. 1788
Merriam, John G. Review of Derek Hopwood, ed., *The Arabian Peninsula: Society and Politics;* and Elizabeth Monroe, *The Changing Balance of Power in the Persian Gulf: An International Seminar Report.* 1359
Metropolitan Political Structure: Intergovernmental Relations and Political Integration in the Quad-Cities. H. Paul Friesema. Reviewed by Francis M. Wilhoit. 1765
Meyer, Alfred G. Review of John Maguire, *Marx's Paris Writings: An Analysis.* With an introduction by David McLellan. 1741
Middle-Class Voters and Progressive Reform: The Conflict of Class and Culture. Roger E. Wyman. 488
The Middle East: A Political and Economic Survey, 4th ed. Peter Mansfield, ed. Reviewed by Robert Owen Freedman. 1365
The Migration of Workers in the United Kingdom and the European Community. W. R. Böhning. Reviewed by Dennis Brooks. 1851
Mikdashi, Zuhayr. *The Community of Oil Exporting Countries: A Study in Governmental Cooperation.* Reviewed by J. S. Nye. 873
Milenky, Edward S. Review of William O. Douglas, *Holocaust or Hemispheric Co-Op: Cross Currents in Latin America.* 861
The Military Indoctrination of Soviet Youth. Leon Goure, Reviewed by Jaan Pennar. 1808
Millar, James R. Review of Karl-Eugen Wädekin, *The Private Sector in Soviet Agriculture,* 2nd rev. ed. of *Privatproduzenten in der sowjetischen Landwirtschaft.* G. F. Karcz, ed. K. Bush, trans. 1379
Miller, Abraham H. and Stephen E. Bennett. *Communication* on James Q. Wilson and Edward C. Banfield, "Political Ethos Revisited" (December, 1971). 1265
Miller, Arthur H., Arthur G. Stevens, Jr., and Thomas E. Mann. *Mobilization of Liberal Strength in the House, 1955–1970: The Democratic Study Group.* 667
Miller, Arthur H. *Political Issues and Trust in Government: 1964–1970.* 951
Rejoinder to "Comment" by Jack Citrin: Political Discontent or Ritualism? 989
Miller, Brown, Neil J. Pinney, and William S. Saslow. *Innovation In New Communities.* Reviewed by Thomas P. Murphy. 788
Miller, Lynn H. *Organizing Mankind: An Analysis of Contemporary International Organization.* Reviewed by Garth Stevenson. 356
Miller, Thomas G. Jr., Robert C. Fraser, and Alan D. Donheiser. *Civil Aviation Development: A Policy and Operating Analysis.* Prepared for Arthur D. Little, Inc. Reviewed by Robert L. Thornton. 857
Miller, William L., D. Woods Thomas, Harry R. Potter, and Adrian F. Aveni, eds. *Institution Building: A Model for Applied Social Change.* Reviewed by Seymour B. Sarason. 268
Milsten, Donald E. Review of Edward Wenk, Jr., *The Politics of the Ocean.* 1343
Minar, David W., Michael D. Usdan, and Emanuel Hurwitz, Jr. *Education and State Politics.* Reviewed by Carl Grafton. 794
The Mind of Adolf Hitler. Walter C. Langer. Reviewed by A. James Gregor. 317
Mind and Politics. Ellen Meiksins Wood. Reviewed by Vincent E. Starzinger. 1321
Minimum Winning Coalitions Reconsidered: A Rejoinder to Butterworth's "Comment." Kenneth A. Shepsle. 522
Minogue, K. R. *Nationalism.* Reviewed by Walker Connor. 320
Mirer, Thad W. and Stanley Kelley, Jr. *The Simple Act of Voting.* 572
Mitchell, Allan. *Bismarck and the French Nation, 1848–1890.* Reviewed by Otto Pflanze. 874
Mitchell, Robert C., Donald G. Morrison, John N. Paden, Hugh M. Stevenson, and others. *Black Africa: A Comparative Handbook.* Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* 243
Mitchell, William C. *Why Vote?* Reviewed by Max M. Kampelman. 784
Review of William A. Niskanen, Jr., *Bureaucracy and Representative Government.* 1775
Mittelman, James H. Review of Peter M. Gukiina, *Uganda: A Case Study in African Political Development.* 303
Mittelstand, Demokratie und Nationalsozialismus: Die politische Entwicklung von Handwerk und Kleinhandel in der Weimarer Republik. August Heinrich Winkler. Reviewed by Walter Struve. 851
Mobilization of Liberal Strength in the House, 1955–1970: The Democratic Study Group. Arthur G. Stevens, Jr., Arthur H. Miller, and Thomas E. Mann. 667

Models of Statistical Relationship. Herbert F. Weisberg. 1638
Modernization: Latecomers and Survivors. Marion J. Levy, Jr. Reviewed by Lucian W. Pye. 1740
Modern Revolution: An Introduction to the Analysis of a Political Phenomenon. John Dunn. Reviewed by Rex Mortimer. 1351
Moeller, Beverly Bowen. *Phil Swing and Boulder Dam.* Reviewed by Daniel M. Ogden, Jr. 1771
Mohr, Lawrence B. Review of James L. Price, *Handbook of Organizational Measurement.* .. 1746
Money and Capital in Economic Development. Ronald I. McKinnon. Reviewed by W. W. Rostow. 1822
Monroe, Elizabeth. *The Changing Balance of Power in the Persian Gulf: An International Seminar Report.* Reviewed by John G. Merriam. 1359
Monypenny, Phillip. Review of Daniel J. Elazar, *The Politics of Belleville: A Profile of the Civil Community.* 1761
Moore, David W. *Foreign Policy and Empirical Democratic Theory.* 1192
Moore, John E. and Stanley V. Anderson, eds. *Establishing Ombudsman Offices: Recent Experience in the United States.* Transcript of the Ombudsman WORKSHOP, Honolulu, Hawaii, May 5–7, 1971. Reviewed by William B. Gwyn. 1752
Moore, John Norton. *Law and the Indo-China War.* Reviewed by P. E. Corbett. 357
Morgan, David and Harvey Cox. *City Politics and the Press: Journalists and the Governing of Merseyside.* Reviewed by Morris Davis. 1350
Morgan, Richard E. *The Supreme Court and Religion.* Reviewed by Jacob W. Landynski. 285
Morgan, Robert J. Review of William M. Wiecek, *The Guarantee Clause of the U.S. Constitution.* 1344
Morgan, Roger, ed. *The Study of International Affairs: Essays in Honour of Kenneth Younger.* Reviewed by John H. Herz. 1404
Morris, Bernard S. Review of Harish Kapur, *The Soviet Union and the Emerging Nations: A Case Study of Soviet Policy Towards India.* 868
Review of Jack Woddis, *New Theories of Revolution.* 1751
Morrison, Donald G. Review of Victor A. Olorunsola, ed., *The Politics of Cultural Sub-nationalism in Africa: Africa and the Problem of "One state—many nationalisms."*..... 838
Morrison, Donald G., Robert C. Mitchell, John N. Paden, Hugh M. Stevenson, and others. *Black Africa: A Comparative Handbook.* Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* 243
Morse, Stanley J. and Stanton Peele. *Ethnic Voting and Political Change in South Africa.* .. 1520
Mortimer, Rex. Review of John Dunn, *Modern Revolution: An Introduction to the Analysis of a Political Phenomenon.* 1351
Mosca, Gaetano. *The Ruling Class: Elementi di scienza politica.* Reviewed by Dante Germino. 261
Moskos, Charles C., Jr., ed. *Public Opinion and the Military Establishment.* Reviewed by James M. Gerhardt. 1768
Mosse, George L. Review of George Lichtheim, *Europe in the Twentieth Century.* 1402
Mouledoux, Joseph C. Review of Nellie D. Kendall, ed., *Willmoore Kendall Contra Mundum.* With an introduction by Jeffrey Hart. .. 1738
Mowry, George E. *Another Look at the Twentieth-Century South.* Reviewed by Charles S. Bullock, III. 1334
Multivariate Model Building: The Validation of a Search Strategy. John A. Sohnquist. Reviewed by Bernard Grofman. 1749
Munger, Edwin S. Review of Heribert Adam, ed., *South Africa: Sociological Perspectives.* .. 1785
Murphy, James T. *Political Parties and the Pork-barrel: Party Conflict and Cooperation in House Public Works Committee Decision Making.* 169
Murphy, Jeffrie G. Review of Ernest van den Haag, *Political Violence and Civil Disobedience.* 269
Murphy, Russell D. *Political Entrepreneurs and Urban Poverty.* Reviewed by Louis F. Weschler. 1771
Murphy, Thomas P., ed. *Government Management Internships and Executive Development.* Reviewed by Raymond Pomerleau. 1772
Murphy, Thomas P. Review of Brown Miller, Neil J. Pinney, and William S. Saslow, *Innovation In New Communities.* 788
Mushkat, Jerome. *Tammany: Evolution of a Political Machine, 1789–1865.* Reviewed by Joel A. Tarr. 1773
Mutchler, David E. *The Church as a Political Factor in Latin America: With Special Reference to Colombia and Chile.* Reviewed by Ivan Vallier. 832
Myers, Kendall. Review of Josef Korbel, *Détente in Europe: Real or Imaginary?* 871
Myint, Hla. *Southeast Asia's Economy: Development Policies in the 1970s.* Reviewed by H. W. Singer. 1830
The Mythology of the Secret Societies. J. M. Roberts. Reviewed by Nannerl O. Keohane. 265

Nader, Laura, ed. *Law in Culture and Society.* Reviewed by Herbert Jacob. 292
Nagel, Stuart S., ed. *The Rights of the Accused.* Reviewed by David Fellman. 1774
Nagi, Saad Z. and Ronald G. Corwin, eds. *The Social Contexts of Research.* Reviewed by Arnold Thackray. 1314
Napolitan, Joseph. *The Election Game and How*

To Win It. Reviewed by Max M. Kampelman. 784
National Commission on Marihuana and Drug Abuse. *Drug Use in America: Problem in Perspective.* Reviewed by Joseph L. Zentner. 1329
National Interests and the Multinational Enterprise: Tensions Among the North Atlantic Countries. Jack N. Behrman. Reviewed by Vincent J. Fuccillo. 855
Nationalism in Eastern Europe. Peter Sugar and Ivo Lederer, eds. Reviewed by Rudolf L. Tökés. 849
Nationalism. K. R. Minogue. Reviewed by Walker Connor. 320
NATO in the Seventies. Edwin Fedder, ed. Reviewed by Robert S. Jordan. 864
Natural Law in Political Thought. Paul E. Sigmund. Reviewed by Judith N. Shklar. 266
La Nauze, J. A. *The Making of the Australian Constitution.* Reviewed by G. C. Bolton. 315
The Naval War Against Hitler. Donald MacIntyre. Reviewed by Paul L. Savage. 802
The Necessary Majority. Robert C. Wood. Reviewed by Max M. Kampelman. 784
Need for Achievement and Competitiveness as Determinants of Political Party Success in Elections and Coalitions. Bruce Bueno de Mesquita. 1207
Needham, Joseph, Robert Jungk, Ernan McMullin, Joan Robinson, Stuart Schram, William Sewell, and Bernard Towers. *China and the West: Mankind Evolving.* Reviewed by John H. Boyle. 1399
Needler, Martin. *Politics and Society in Mexico.* Reviewed by David Ronfeldt. 322
Review of Shirley Brice Heath, *Telling Tongues: Language Policy in Mexico, Colony to Nation.* 821
Neenan, William B. *Political Economy of Urban Areas.* Reviewed by Lloyd A. Rowe. 789
Neilands, J. B., G. H. Orians, E. W. Pfeiffer, Alje Vennema, and Arthur H. Westing. *Harvest of Death.* Reviewed by George H. Quester in *Chemical and Biological Warfare.* 1285
Nellis, John. Review of Ali A. Mazrui, *Cultural Engineering and Nation-Building in East Africa.* 831
The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science. Ted Robert Gurr. Review of Arthur S. Banks, assembler, *Cross-Polity Time-Series Data;* Donald G. Morrison, Robert C. Mitchell, John N. Paden, Hugh M. Stevenson, and others, *Black Africa: A Comparative Handbook;* J. David Singer and Melvin Small, *The Wages of War, 1816–1965: A Statistical Handbook;* Charles Lewis Taylor, Michael C. Hudson, and others, *World Handbook of Political and Social Indicators,* 2nd ed. 243
Nepal: Strategy for Survival. Leo Rose. Reviewed by Merrill R. Goodall. 878
The New Deal and the Last Hurrah: Pittsburgh Machine Politics. Bruce M. Stave. Reviewed by William C. Seyler. 1779
The New Deal in the Suburbs: A History of the Greenbelt Towns. Joseph L. Arnold. Reviewed by Hugh A. Bone. 776
Newell, Richard S. *The Politics of Afghanistan.* Reviewed by Ludwig Adamec. 834
The New How To Win An Election. Stephen C. Shadegg. Reviewed by Max M. Kampelman. 784
The New Language of Politics: A Dictionary of Catchwords, Slogans & Political Usage. William Safire. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696
New Left Diplomatic Histories and Historians: The American Revisionists. Joseph M. Siracusa. Reviewed by Ole R. Holsti. 1878
The New Politics of European Integration. Ghita Ionescu, ed. Reviewed by Ernest B. Haas. 349
New Theories of Revolution. Jack Woddis. Reviewed by Bernard S. Morris. 1751
Nexon, David. Review of John S. Saloma, III and Frederich H. Sontag, *Parties: The Real Opportunity For Effective Citizen Politics.* 792
Nichols, James H., Jr. Review of Bernard Crick, *In Defence of Politics,* 2nd ed. 1731
Nielsen, Donald A. Review of Werner J. Cahnmann and Rudolf Heberle, eds., *Ferdinand Toennies on Sociology: Pure, Applied and Empirical. Selected Writings.* 1728
Nigeria: Dilemma of Nationhood, an African Analysis of the Biafran Conflict. Joseph Okpaku, ed. Reviewed by Robert Melson. 837
The Nigerian Military: A Sociological Analysis of Authority and Revolt, 1960–67. Robin Luckham. Reviewed by Grady H. Nunn. 1822
The Night of Long Knives. Max Gallo. Lily Emmet, trans. Reviewed by C. F. Latour. ... 300
The Nihilistic Egoist: Max Stirner. R. W. K. Paterson. Reviewed by Z. A. Jordan. 766
Niskanen, William A., Jr. *Bureaucracy and Representative Government.* Reviewed by William C. Mitchell. 1775
Review of Arnold R. Weber, *In Pursuit of Price Stability: The Wage-Price Freeze of 1971.* 1342
Nonattitudes and American Public Opinion: The Examination of a Thesis. John C. Pierce and Douglas D. Rose. 626
Nordahl, Richard. Review of Howard Sherman, *Radical Political Economy: Capitalism and Socialism from a Marxist-Humanist Perspective.* 771
Northedge, F. S. Review of Correlli Barnett, *The Collapse of British Power.* 1349
Review of Robert Hunter, *Security in Europe.* 1396
North for the Trade: The Life and Times of a Berber Merchant. John Waterbury. Reviewed by I. William Zartman. 850

Notehelfer, F. G. Review of David Wurfel, ed., *Meiji Japan's Centennial: Aspects of Political Thought and Action*. 852
Nottingham, Judith and John Cookson. *A Survey of Chemical and Biological Warfare*. Reviewed by George H. Quester in Chemical and Biological Warfare. 1285
Nova, Fritz. Review of Ismar Schorsch, *Jewish Reactions to German Anti-Semitism, 1870–1914*. 843
Novak, Michael. *The Rise of the Unmeltable Ethnics: The New Political Force of the Seventies*. Reviewed by Lawrence H. Fuchs. 790
Nowak, Thomas C. and Kay A. Snyder. *Clientelist Politics in the Philippines: Integration or Instability?* 1147
Nuclear Proliferation: Prospects for Control. Bennett Boskey and Mason Willrich, eds. Reviewed by Robert L. Rothstein. 336
Nunn, Grady H. Review of Robin Luckham, *The Nigerian Military: A Sociological Analysis of Authority and Revolt, 1960–67*. 1822
Nuscheler, Franz and Winfried Steffani, eds. *Pluralismus: Konzeptionen und Kontroversen*. Reviewed by Henry W. Ehrmann. 1733
Nuthmann, Reinhard, Dirk Hartung, and Dietrich Winterhager. *Politilogen im Beruf. Zur Aufnahme und Durchsetzung neuer Qualifikationen im Beschäftigungssystem*. Reviewed by John Dreijmanis. 304
Nye, Joseph S., Robert W. Cox, Harold K. Jacobson, Gerard and Victoria Curzon, Lawrence Scheinman, James P. Sewell, and Susan Strange. *The Anatomy of Influence: Decision-Making in International Organization*. Reviewed by H. Field Haviland. 1389
Nye, J. S. *Peace in Parts: Integration and Conflict in Regional Organization*. Reviewed by Andrzej Korbonski. 358
Review of Zuhayr Mikdashi, *The Community of Oil Exporting Countries: A Study in Governmental Cooperation*. 873
Nzimiro, Ikenna. *Studies in Ibo Political Systems: Chieftaincy and Politics in Four Niger States*. Reviewed by Alvin Magid. 1831

Oates, Wallace E. *Fiscal Federalism*. Reviewed by Deil S. Wright. 1777
O'Barr, William M., David H. Spain, and Mark A. Tessler, eds. *Survey Research in Africa: Its Applications and Limits*. Reviewed by Michael Armer. 835
Obligation: A Social Theory. Ralph Ross. Reviewed by Melvin M. Tumin. 1316
Odegard, Holton P. *The Politics of Truth*. Reviewed by Victor A. Thompson. 1742
Offner, Arnold A. Review of Robert A. Divine, *Roosevelt and World War II*. 1858
Ogden, Daniel M. Jr. Review of Beverly Bowen Moeller, *Phil Swing and Boulder Dam*. 1771
Okpaku, Joseph, ed. *Nigeria: Dilemma of Nationhood, an African Analysis of the Biafran Conflict*. Reviewed by Robert Melson. 837
Olorunsola, Victor A., ed. *The Politics of Cultural Subnationalism in Africa: Africa and the Problem of "One-state—many nationalisms."* Reviewed by Donald G. Morrison. 838
One Country or Two? R. M. Burns, ed. Reviewed by Peter Regenstreif. 808
On the City's Rim: Politics and Policy in Suburbia. Frederick M. Wirt, Benjamin Walter, Francine Rabinovitz, and Deborah Hensler. Reviewed by John H. Baker. 291
Ordeshook, Peter C., Peter H. Aranson, and Melvin J. Hinich. *Election Goals and Strategies: Equivalent and Nonequivalent Candidate Objectives*. 135
An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems. Kenneth Jowitt. 1171
Organizing Mankind: An Analysis of Contemporary International Organization. Lynn H. Miller. Reviewed by Garth Stevenson. 356
The Organs of Soviet Administration of Justice: Their History and Operation. Samuel Kucherov. Reviewed by W. E. Butler. 827
Orians, G. H., J. B. Neilands, E. W. Pfeiffer, Alje Vennema, and Arthur H. Westing. *Harvest of Death*. Reviewed by George H. Quester in *Chemical and Biological Warfare*. 1285
The Origin of Forced Labor in the Soviet State, 1917–1921: Documents and Materials. James Bunyan. Reviewed by Leonard B. Schapiro. 296
The Origins of Capitalism in Russia: Industry and Progress in the Sixteenth and Seventeenth Centuries. Joseph T. Fuhrmann. Reviewed by Evsey D. Domar. 818
Origins of Franco's Spain: The Right, the Republic and Revolution, 1931–1936. Richard A. H. Robinson. Reviewed by Glen A. Waggoner. 1836
On the Origins of Interest-Group Theory: A Critique of a Process. G. David Garson. 1505
Ornelas, Charles. Review of Richard B. Craig, *The Bracero Program: Interest Groups and Foreign Policy*. 858
Ørvik, Nils. Review of August Schou and Arne O. Brundtland, eds., *Small States in International Relations, Nobel Symposium 17*. 1875
Review of Helge Hveem, *International Relations and World Images: A Study of Norwegian Foreign Policy Elites*. 1397
Osborne, Melville E. Review of Karel Holbrik and Philip L. Swan, *Trade and Industrialization in the Central American Common Market, the First Decade*. 824
Osgood, Robert E. *The Weary and Wary: U.S. and Japanese Security Policies in Transition*. Reviewed by Frank C. Langdon. 874

Ostrom, Vincent. *The Political Theory of a Compound Republic: A Reconstruction of the Logical Foundations of American Democracy as Presented in the Federalist.* Reviewed by Robert S. Ross. 1315
Review of James L. Sundquist, *Making Federalism Work: A Study of Program Coordination at the Community Level.* 792
The Other Israel: The Radical Case Against Zionism. Arie Bober, ed. Reviewed by Allan E. Shapiro. 804
Overmyer, Daniel L. Review of Jean Chesneaux, ed., *Popular Movements and Secret Societies in China, 1840–1950.* 1795
Ozinga, James R. Review of Dick Howard and Karl Klare, eds., *The Unknown Dimension: European Marxism Since Lenin.* 762

Pacific Estrangement: Japanese and American Expansion, 1897–1911. Akira Iriye. Reviewed by Warren I. Cohen. 350
Pack, Howard. *Structural Change and Economic Policy in Israel.* Reviewed by Ruth Klinov. .. 324
Paden, John N., Hugh M. Stevenson, Donald G. Morrison, Robert C. Mitchell, and others. *Black Africa: A Comparative Handbook.* Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* 243
Padover, Sauk K., ed. and arranger. *Karl Marx on Revolution.* With an introduction and new translations by the editor. Reviewed by Z. A. Jordan. 1743
Page, Stanley W. Review of Anna Bourguina, *Russian Social Democracy; The Menshevik Movement: A Bibliography.* 806
Painchaud, Paul, director. *Francophonie, Bibliographie 1960–1969.* Reviewed by Edward M. Corbett. 359
Pakistan: Failure in National Integration. Rounaq Jahan. Reviewed by Lawrence Ziring. 311
Pangle, Thomas L. Review of R. Hackforth, trans., *Plato's Phaedo, Plato's Phaedrus, Plato's Philebus* (3 vols. with commentary). .. 258
Papanek, Gustav F. and Walter P. Falcon, eds. *Development Policy II: The Pakistan Experience.* Reviewed by Rowland Egger. 1800
Paradoxes of Voting. Peter C. Fishburn. 537
The Paradox of Not Voting: A Decision Theoretic Analysis. John A. Ferejohn and Morris P. Fiorina. 525
Park, Richard L. Review of Milton Singer, *When a Great Tradition Modernizes: An Anthropological Approach to Indian Civilization.* Foreword by M. N. Srinivas. 1840
Park, Rolla Edward. *The Role of Analysis in Regulatory Decision-making: The Case of Cable Television.* Reviewed by Nathan Shoehalter. 1335
Parliament and Congress. Kenneth Bradshaw and David Pring. Reviewed by Anthony King. 807
Parrish, Michael. *The Soviet Armed Forces, Books in English, 1950–1967.* Hoover Institution Bibliographical Series: XLVIII. Reviewed by Leon Goure. 1370
Parris, Judith H. *The Convention Problem: Issues in Reform of Presidential Nominating Procedures.* Reviewed by Charles Press. 1756
Participation, Planning, and Exchange in Old and New Communities: A Collaborative Paradigm. David R. Godschalk. Reviewed by Judith V. May. 1766
Participatory Democracy: Developing Ideals of the Political Left. Daniel C. Kramer. Reviewed by Carole Pateman. 1311
Parties: The Real Opportunity For Effective Citizen Politics. John S. Saloma, III and Frederich H. Sontag. Reviewed by David Nexon. 792
Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis. Lawrence C. Dodd. 1093
Patai, Raphael. *Society, Culture, and Change in the Middle East,* 3rd ed. Reviewed by Amal Vinogradov. 840
Pateman, Carole. Review of Daniel C. Kramer, *Participatory Democracy: Developing Ideals of the Political Left.* 1311
Paterson, R. W. K. *The Nihilistic Egoist: Max Stirner.* Reviewed by Z. A. Jordan. 766
Pattern for Profit in Southern Africa. Ian Mackler. Reviewed by Julian R. Friedman. 1824
Patterns of International Cooperation in the Caribbean, 1942–1969. Herbert Corkran, Jr. Reviewed by Thomas Mathews. 341
Payne, Robert. *The Life and Death of Adolf Hitler.* Reviewed by A. James Gregor. 317
The Peace Conspiracy: Wang Ching-wei and the China War, 1937–1941. Gerald E. Bunker. Reviewed by James E. Sheridan. 1384
Peace in Parts: Integration and Conflict in Regional Organization. J. S. Nye. Reviewed by Andrzej Korbonski. 358
Peasants in Revolt: A Chilean Case Study, 1965–1971. James Petras and Hugo Zemelman Merino. Reviewed by Alan Angell. 1832
Peele, Stanton and Stanley J. Morse. *Ethnic Voting and Political Change in South Africa.* .. 1520
Peirce, Neal R. *The People's President: The Electoral College in American History and the Direct-Vote Alternative.* Reviewed by Charles Press. 1756
Pennar, Jaan. Review of Leon Goure, *The Military Indoctrination of Soviet Youth.* 1808
Penniman, Howard. *Elections in South Vietnam.* Reviewed by Allan E. Goodman. 1370
Pennock, J. Roland and John W. Chapman, eds. *Political and Legal Obligation, Nomos XII, Yearbook of the American Society for Political*

and Legal Philosophy. Reviewed by M. R. Kadish. 768
People of Paradox: An Inquiry Concerning the Origins of American Civilization. Michael Kammen. Reviewed by Jack P. Greene. 1331
The People's President: The Electoral College in American History and the Direct-Vote Alternative. Neal R. Peirce. Reviewed by Charles Press. 1756
Perham, Margery. *Colonial Sequence, 1930 to 1949: A Chronological Commentary upon British Colonial Policy, Especially in Africa;* and *Colonial Sequence, 1949 to 1969: A Chronological Commentary upon British Colonial Policy in Africa*, 2 vols. Reviewed by L. H. Gann. 1870
Perlmutter, Amos. Review of Aaron S. Klieman, *Soviet Russia and the Middle East*. 1864
Review of Nachum Finger, *The Impact of Government Subsidies on Industrial Management: The Israeli Experiment;* and Roy Popkin, *Technology of Necessity: Scientific and Engineering Development in Israel*. 815
The Persian Gulf: Iran's Role. Rouhollah K. Ramazani. Reviewed by R. M. Burrell. 1873
Persistence and Change in Political Systems, 1800–1971. Ted Robert Gurr. 1482
Peters, Richard S. and Maurice Cranston, eds. *Hobbes and Rousseau: A Collection of Critical Essays*. Reviewed by C. B. Macpherson. 1730
Petras, James and Hugo Zemelman Merino. *Peasants in Revolt: A Chilean Case Study, 1965–1971*. Reviewed by Alan Angell. 1832
Pfaff, Richard H. Review of George S. Harris, *Troubled Alliance: Turkish-American Problems in Historical Perspective, 1945–1971*. ... 348
Pfaltzgraff, Robert L., Jr. Review of Steven Joshua Warnecke, ed., *The European Community in the 1970s*. 369
Pfeiffer, E. W., J. B. Neilands, G. H. Orians, Alje Vennema, and Arthur H. Westing. *Harvest of Death*. Reviewed by George H. Quester in *Chemical and Biological Warfare*. 1285
Pflanze, Otto. Review of Allan Mitchell, *Bismarck and the French Nation, 1848–1890*. ... 874
Review of Helmut Böhme, ed., *The Foundation of the German Empire: Select Documents*. Agatha Ramm, trans. 295
Pfotenhauer, David. *Communication* on Philip Stewart et al., "Political Mobility and the Soviet Political Process: A Partial Test of Two Models" (December, 1972). 205
Pfretzschner, Paul A. Review of D. G. Boyce, *Englishmen and Irish Troubles: British Public Opinion and the Making of Irish Policy, 1918–1922*. 295
Phil Swing and Boulder Dam. Beverly Bowen Moeller. Reviewed by Daniel M. Ogden, Jr. .. 1771
Pierce, John C. and Douglas D. Rose. *Nonattitudes and American Public Opinion: The Examination of a Thesis*. 626
Rejoinder to "Comment" by Philip E. Converse. 661
Pinkney, David H. *The French Revolution of 1830*. Reviewed by John Dunn. 1833
Pinney, Neil J., Brown Miller, and William S. Saslow. *Innovation in New Communities*. Reviewed by Thomas P. Murphy. 788
Pipes, Richard. Review of Reginald E. Zelnik, *Labor and Society in Tsarist Russia: The Factory Workers of St. Petersburg, 1855–1870*. .. 333
Pitkin, Hanna Fenichel. *Wittgenstein and Justice: On the Significance of Ludwig Wittgenstein for Social and Political Thought*. Reviewed by Stephen E. Toulmin. 769
Planning, Prediction, and Policymaking in Foreign Affairs. Robert L. Rothstein. Reviewed by Bernard C. Cohen. 1407
Plano, Jack C. and Milton Greenberg. *The American Political Dictionary*, 3rd ed. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay*. 1696
Plant, Raymond. *Hegel*. Reviewed by Judith N. Shklar. 1744
Plato's Phaedo, Plato's Phaedrus, Plato's Philebus (3 vols. with commentary). R. Hackforth, trans. Reviewed by Thomas L. Pangle. 258
Plesur, Milton. *America's Outward Thrust: Approaches To Foreign Affairs, 1865–1890*. Reviewed by Donald M. Borock. 875
Review of Daniel B. Schirmer, *Republic or Empire: American Resistance to the Philippine War*. 1409
Review of Sidney Bell, *Righteous Conquest: Woodrow Wilson and the Evolution of the New Diplomacy*. 1755
Plischke, Elmer. Review of Robert B. Harmon, *The Art and Practice of Diplomacy: A Selected and Annotated Guide*. 347
Ploss, Sidney. Review of Arthur E. Adams and Jan S. Adams, *Men Versus Systems: Agriculture in the USSR, Poland, and Czechoslovakia*. 798
Pluralismus: Konzeptionen und Kontroversen. Franz Nuscheler and Winfried Steffani, eds. Reviewed by Henry W. Ehrmann. 1733
Pluralismus zwischen Liberalismus und Sozialismus. Rainer Eisfeld. Reviewed by Henry W. Ehrmann. 1733
Poats, Rutherford M. *Technology for Developing Nations: New Directions for U.S. Technical Assistance*. Reviewed by Brewster C. Denny. 1371
Poggi, Gianfranco. Review of Herman Turk and Richard L. Simpson, eds., *Institutions and Social Exchange: The Sociologies of Talcott Parsons and C. Homans*. 773
Political Apocalypse: A Study of Dostoevsky's Grand Inquisitor. Ellis Sandoz. Reviewed by Joseph Frank. 1747
Political Dictionaries: A Bibliographical Essay.

Clement E. Vose. Review of Charles Abrams, *The Language of Cities: A Glossary of Terms;* Maurice Cranston and Sanford A. Lakoff, eds., *A Glossary of Political Ideas;* Joseph Dunner, ed., *Dictionary of Political Science;* Florence Elliott, *A Dictionary of Politics*, 6th ed.; Walter Laqueur, ed., *A Dictionary of Politics;* Edward Luttwak, *A Dictionary of Modern War;* Jack C. Plano and Milton Greenberg, *The American Political Dictionary*, 3rd ed.; Geoffrey K. Roberts, *A Dictionary of Political Analysis;* William Safire, *The New Language of Politics: A Dictionary of Catchwords, Slogans & Political Usage;* and Edward C. Smith and Arnold J. Zurcher, *Dictionary of American Politics*, 2nd ed. 1696

The Political Economy of Public Organizations: A Critique and Approach to the Study of Public Administration. Gary L. Wamsley and Mayer N. Zald. Reviewed by Charles H. Levine. ... 795

Political Economy of Urban Areas. William B. Neenan. Reviewed by Lloyd A. Rowe. 789

Political Elite in Bihar. Shashishekhar Jha. Reviewed by Douglas C. Smyth. 1361

The Political Elite of Iran. Marvin Zonis. Reviewed by Marvin G. Weinbaum. 334

Political Entrepreneurs and Urban Poverty. Russell D. Murphy. Reviewed by Louis F. Weschler. 1771

The Political Image Merchants. Ray Hiebert, Robert Jones, John Lorenz, and Ernest Lotito, eds. Reviewed by Max M. Kampelman. 784

Political Implications of Gender Roles: A Review of the Literature. Wilma Rule Krauss. 1706

Political Issues and Trust in Government: 1964–1970. Arthur H. Miller. 951

Political and Legal Obligation, Nomos XII, Yearbook of the American Society for Political and Legal Philosophy. J. Roland Pennock and John W. Chapman, eds. Reviewed by M. R. Kadish. 768

Political Obligation. Richard E. Flathman. Reviewed by Leslie J. Macfarlane. 1305

Political Parties and the Porkbarrel: Party Conflict and Cooperation in House Public Works Committee Decision Making. James T. Murphy. 169

The Political System of Brazil: Emergence of a "Modernizing" Authoritarian Regime, 1964–1970. Ronald M. Schneider. Reviewed by Riodan Roett. 1837

The Political Theory of a Compound Republic: A Reconstruction of the Logical Foundations of American Democracy as Presented in the Federalist. Vincent Ostrom. Reviewed by Robert S. Ross. 1315

Political Trends in the Arab World: The Role of Ideas and Ideals in Politics. Majid Khadduri. Reviewed by Iliya Harik. 1818

Political Violence and Civil Disobedience. Ernest van den Haag. Reviewed by Jeffrie G. Murphy. 269

The Political Writings of Leibniz. (with an introduction and notes). Patrick Riley, ed. and trans. Reviewed by Richard Kennington. ... 263

The Politics of Afghanistan. Richard S. Newell. Reviewed by Ludwig Adamec. 834

The Politics of Assimilation: Hegemony and Its Aftermath. Charles Doran. Reviewed by Richard Rosecrance. 860

The Politics of Authenticity: Radical Individualism and the Emergence of Modern Society. Marshall Berman. Reviewed by Allan Bloom. 1297

The Politics of Belleville: A Profile of the Civil Community. Daniel J. Elazar. Reviewed by Phillip Monypenny. 1761

Politics and the Career Mobility of Bureaucrats. Eugene B. McGregor, Jr. 18

The Politics of Cultural Subnationalism in Africa: Africa and the Problem of "One state—many nationalisms." Victor A. Olorunsola, ed. Reviewed by Donald G. Morrison. 838

The Politics of Economic Reform in the Soviet Union. Abraham Katz. Reviewed by Frederic L. Pryor. 1817

The Politics of Environmental Concern. Walter A. Rosenbaum. Reviewed by Harris S. Cohen. . 1337

Politics in India. Rajni Kothari. Reviewed by Myron Weiner. 1363

The Politics of Iran: Groups, Classes and Modernization. James Alban Bill. Reviewed by Sepehr Zabih. 1793

Politics and the Labour Movement in Chile. Alan Angell. Reviewed by Federico G. Gil. 800

The Politics of Land Reform in Chile. Robert R. Kaufman. Reviewed by Robert L. Ayres. ... 313

The Politics of Mexican Development. Roger D. Hansen. Reviewed by Lawrence S. Graham. . 820

The Politics of the Ocean. Edward Wenk, Jr. Reviewed by Donald E. Milsten. 1343

Politics in Plural Societies: A Theory of Democratic Instability. Alvin Rabushka and Kenneth A. Shepsle. Reviewed by Fred M. Hayward. 841

The Politics of the Powerless: A Study of the Campaign Against Discrimination. Benjamin W. Heineman, Jr. Reviewed by Richard P. Longaker. 1783

Politics and the Power Structure: A Rural Community in the Dominican Republic. Malcolm T. Walker. Reviewed by G. Pope Atkins. ... 1846

Politics in Science. Marlen Blissett. Reviewed by Gene M. Lyons. 1726

Politics and Society in Mexico. Martin Needler. Reviewed by David Ronfeldt. 322

Politics as Symbolic Action: Mass Arousal and Quiescence. Murray Edelman. Reviewed by Marc Howard Ross. 257

The Politics of Truth. Holton P. Odegard. Re-

viewed by Victor A. Thompson. 1742
The Politics of Urban Development in Singapore. Robert E. Gamer. Reviewed by Lynn T. White, III. 1355
La Politique dans L'Espagne Franquiste. Guy Hermet. Reviewed by Raphael Zariski. 822
Politologen im Beruf. Zur Aufnahme und Durchsetzung neuer Qualifikationen im Beschäftigungssystem. Dirk Hartung, Reinhard Nuthmann, and Dietrich Winterhager. Reviewed by John Dreijmanis. 304
Pollinger, Annette C. and Kenneth J. Pollinger. *Community Action and the Poor: Influence vs. Social Control in a New York City Community.* Reviewed by Kenneth R. Greene. 286
Pollinger, Kenneth J. and Annette C. Pollinger. *Community Action and the Poor: Influence vs. Social Control in a New York City Community.* Reviewed by Kenneth R. Greene. 286
Polsby, Nelson W. Review of Perry M. Goldman and James S. Young, eds., *U.S. Congressional Directories, 1798–1840.* 1328
Pomerleau, Raymond. Review of Thomas P. Murphy, ed., *Government Management Internships and Executive Development.* 1772
Pool, Jonathan. Review of Michael Banton, *Racial Minorities;* and Ernest Q. Campbell, ed., *Racial Tensions and National Identity. Proceedings of the Second Annual Vanderbilt Sociology Conference, November 4–6, 1970.* . 1791
Popkin, Roy. *Technology of Necessity: Scientific and Engineering Development in Israel.* Reviewed by Amos Perlmutter. 815
Popkin, Samuel L. Review of William R. Shaffer, *Computer Simulations of Voting Behavior.* ... 1747
Popular Government in an African Town: Kita, Mali. Nicholas S. Hopkins. Reviewed by William J. Foltz. 1358
Popular Movements and Secret Societies in China, 1840–1950. Jean Chesneaux, ed. Reviewed by Daniel L. Overmyer. 1795
Portis, Edward B. Review of James Gouinlock, *John Dewey's Philosophy of Value.* 1735
Possony, Stefan T. and J. E. Pournelle. *The Strategy of Technology: Winning the Decisive War.* Reviewed by George H. Quester. 1872
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Pournelle, J. E. and Stefan T. Possony. *The Strategy of Technology: Winning the Decisive War.* Reviewed by George H. Quester. 1872
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Press, Charles. Review of Alexander M. Bickel, *Reform and Continuity: The Electoral College, the Convention, and the Party System;* Judith H. Parris, *The Convention Problem: Issues in Reform of Presidential Nominating Procedures;* and Neal R. Peirce, *The People's President: The Electoral College in American History and the Direct-Vote Alternative.* 1756
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Price, David E. *Community and Control: Critical Democratic Theory in the Progressive Period.* . 1663
Price, James L. *Handbook of Organizational Measurement.* Reviewed by Lawrence B. Mohr. 1746
Pring, David and Kenneth Bradshaw. *Parliament and Congress.* Reviewed by Anthony King... 807
Pringle, Robert. *Rajahs and Rebels: The Ibans of Sarawak under Brooke Rule, 1841–1941.* Reviewed by M. C. Ricklefs. 325
The Private Sector in Soviet Agriculture, 2nd rev. ed. of *Privatproduzenten in der sowjetischen Landwirtschaft.* Karl-Eugen Wädekin. G. F. Karcz, ed. K. Bush, trans. Reviewed by James R. Millar. 1379
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The Protracted Game: A Wei-ch'i Interpretation of Maoist Revolutionary Strategy. Scott A. Boorman. Reviewed by Gordon A. Bennett. . 805
Proudhon, P.-J. *General Idea of the Revolution in the 19th Century.* John Beverly Robinson, trans. Reviewed by Charles E. Butterworth. . 1737
Provincial Magistrates and Revolutionary Politics in France, 1789–1795. Philip Dawson. Reviewed by Robert Forster. 811
Provision of Collective Goods As a Function of Group Size. John Chamberlin. 707
Pryor, Frederic L. Review of Abraham Katz, *The Politics of Economic Reform in the Soviet Union.* 1817
Psychoanalytic Jurisprudence: On Ethics, Aesthetics, and Law— On Crime, Tort, and Procedure. Albert A. Ehrenzweig. Reviewed by G. Edward White. 754
Psychology of Political Science With Special Consideration for the Political Acumen of Destutt de Tracy. John M. Dorsey. Reviewed by G. David Garson. 750
Public Diplomacy and the Behavioral Sciences. Glen H. Fisher. Reviewed by Richard A. Rettig. 1859
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Puckett, Robert H. Review of Irvin L. White, Clifton E. Wilson, and John A. Vosburgh, *Law and Politics in Outer Space: A Bibliography.* 370
Purcal, J. T. *Rice Economy, Employment and Income in Malaysia.* Reviewed by Martin Rudner. 326
In Pursuit of Price Stability: The Wage-Price Freeze of 1971. Arnold R. Weber. Reviewed by William A. Niskanen. 1342
Pye, Lucian W. Introduction to APSA Presidential Address, 1973. 188
Review of Marion J. Levy, Jr., *Modernization: Latecomers and Survivors.* 1740
Review of Milton Esman, *Administration and Development in Malaysia: Institution Building and Reform in a Plural Society.* 1352

Quanbeck, Alton H. and Barry M. Blechman. *Strategic Forces: Issues for the Mid-Seventies.* Reviewed by Herbert Scoville, Jr. 1405
Quebec—Only the Beginning: The Manifestoes of the Common Front. Daniel Drache, ed. Reviewed by Peter Regenstreif. 808
Quester, George H. *Chemical and Biological Warfare.* Review of Frederic J. Brown, *Chemical Warfare: A Study in Restraints;* Carnegie Endowment for International Peace, *The Control of Chemical and Biological Weapons;* John Cookson and Judith Nottingham, *A Survey of Chemical and Biological Warfare;* J. B. Neilands, G. H. Orians, E. W. Pfeiffer, Alje Vennema, and Arthur H. Westing, *Harvest of Death;* Steven Rose, ed. *CBW;* Stockholm International Peace Research Institute, *The Problem of Chemical and Biological Warfare,* 6 vols. 1285
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Rabushka, Alvin and Kenneth A. Shepsle. *Politics in Plural Societies: A Theory of Democratic Instability.* Reviewed by Fred M. Hayward. 841
Race, Class, and Political Consciousness. John C. Leggett. Reviewed by Michael Cohen. 1770
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Racial Differences in Protest Participation. Peter K. Eisinger. 592
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Radványi, János. *Hungary and the Superpowers: The 1956 Revolution and Realpolitik.* Reviewed by Kurt L. London. 360
Raeff, Marc. Review of S. Frederick Starr, *Decentralization and Self-Government in Russia, 1830–1870.* 332
Rajahs and Rebels: The Ibans of Sarawak under Brooke Rule, 1841–1941. Robert Pringle. Reviewed by M. C. Ricklefs. 325
Rakowska-Harmstone, Teresa. Review of Roger E. Kanet and Ivan Volgyes, eds., *On the Road to Communism: Essays on Soviet Domestic and Foreign Politics.* 1816
Ramazani, Rouhollah K. *The Persian Gulf: Iran's Role.* Reviewed by R. M. Burrell. 1873
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Ranney, Austin. *Comment on "Changing the Rules Changes the Game."* 43
Rawson, D. W. Review of Don Aitkin, *The*

Country Party in New South Wales: A Study of Organisation and Survival. 1786
Ray, David. *Membership Stability in Three State Legislatures: 1893–1969.* 106
Ray, James Lee. Review of Harvey Starr, *War Coalitions: The Distributions of Payoffs and Losses.* 1880
Readings in the Chinese Communist Cultural Revolution. Wen-shun Chi, comp. and ed. Reviewed by Arthur Huck. 810
Reban, Milan J. Review of Vladimir Dedijer, *The Battle Stalin Lost: Memoirs of Yugoslavia, 1948–1953.* 298
Rébellions au Congo. Benoit Verhaegen. Reviewed by Rene Lemarchand. 1845
Reform and Continuity: The Electoral College, the Convention, and the Party System. Alexander M. Bickel. Reviewed by Charles Press. 1756
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Regenstreif, Peter. Review of John Meisel, *Working Papers on Canadian Politics.* 1369
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Regional Integration: Theory and Research. Leon N. Lindberg and Stuart A. Scheingold, eds. Reviewed by David P. Calleo. 1867
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Reitman, Allen and Robert D. Davidson. *The Election Process, Voting Laws and Procedures.* Reviewed by Max M. Kampelman. 784
Rejoinder to "Comment" by Jack Citrin: Political Discontent or Ritualism? Arthur H. Miller. . 989
Rejoinder to "Comments" by Philip Converse and Jerrold Rusk. Walter Dean Burnham. 1050
Remington, Robin Alison. *The Warsaw Pact: Case Studies in Communist Conflict Resolution.* Reviewed by Adam Bromke. 876
Repass, David E. Review of John P. Robinson, Jerrold G. Rusk, and Kendra B. Head, eds., *Measures of Political Attitudes.* 771
Republic or Empire: American Resistance to the Philippine War. Daniel B. Schirmer. Reviewed by Milton Plesur. 1409
Republic to Reich: The Making of the Nazi Revolution. Hajo Holborn. Reviewed by Gerhard L. Weinberg. 306
Resources Devoted to Military Research and Development: An International Comparison. Stockholm International Peace Research Institute. Reviewed by Barry M. Blechman. 1841
The Retreat of American Power. Henry Brandon. Reviewed by Frank Burd. 1383
Rettig, Richard A. Review of Glen H. Fisher, *Public Diplomacy and the Behavioral Sciences.* 1859
Reveley, W. Taylor, III. Review of Lawrence R. Velvel, *Undeclared War and Civil Disobedience: The American System in Crisis.* 287
The Revival of American Socialism: Selected Papers of the Socialist Scholars Conference. George Fischer, ed. Reviewed by Gary D. Glenn. 1764
Revolt in Athens: The Greek Communist "Second Round," 1944–1945. John O. Iatrides. Reviewed by Van Coufoudakis. 309
Revolutionary Education in China: Documents and Commentary. Peter J. Seybolt. Reviewed by Anthony W. Ferguson. 1375
Revolutionary Justice: The Social and Political Theory of P.-J. Proudhon. Robert L. Hoffman. Reviewed by Charles E. Butterworth. 1737
Revolutionary Leaders of Modern China. Chün-tu Hsüeh, ed. Reviewed by Robert E. Bedeski. . 307
The Revolutionary Potential of Peasants in Latin America. Gerrit Huizer. Reviewed by Enrique Baloyra. 1812
Revolution in Peru: Mariátegui and the Myth. John M. Baines. Reviewed by Harry Kantor. 1348
Revolution and Tradition in People's Poland: Education and Socialization. Joseph R. Fiszman. Reviewed by V. C. Chrypinski. 1801
Rice Economy, Employment and Income in Malaysia. J. T. Purcal. Reviewed by Martin Rudner. 326
Richardson, J. L. Review of David V. Edwards, *Creating a New World Politics: From Conflict to Co-operation.* 1392
Rich, Norman. *Hitler's War Aims: Ideology, the Nazi State, and the Course of Expansion.* Reviewed by A. J. P. Taylor. 1406
Richter, William L. Review of Richard Sisson and Lawrence Shrader, *Legislative Recruitment and Political Integration: Patterns of Political Linkage in an Indian State.* 329
Ricklefs, M. C. Review of Robert Pringle, *Rajahs and Rebels: The Ibans of Sarawak under Brooke Rule, 1841–1941.* 325
Ridley, Charles Price, Paul H. B. Godwin, and Dennis J. Doolin. *The Making of A Model Citizen in Communist China.* Reviewed by Franklin W. Houn. 1835
Rifkin, Susan Beth. Review of Surveys and Research Corporation, *Directory of Selected Scientific Institutions in Mainland China.* 333
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Righteous Conquest: Woodrow Wilson and the Evolution of the New Diplomacy. Sidney Bell. Reviewed by Milton Plesur. 1755
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Riker, William H. and Steven J. Brams. *Communications* in reply to comments on their article "The Paradox of Vote Trading" (December, 1973). 1688, 1692
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Riley, Matilda White et al. *Aging and Society:* Matilda White Riley and Anne Foner. Vol. I: *An Inventory of Research Findings;* Matilda White Riley, John W. Riley, Jr., and Marilyn Johnson. Vol. II: *Aging and the Professions;* Matilda White Riley, Marilyn Johnson, and Anne Foner. Vol. III: *A Sociology of Age Stratification*. Reviewed by Frederick R. Eisele. 262
Riley, Patrick, ed. and trans. *The Political Writings of Leibniz* (with an introduction and notes). Reviewed by Richard Kinnington. .. 263
Ripley, Randall B. Review of Barbara Hinckley, *Stability and Change in Congress*. 280
The Rise and Fall of the People's Century: Henry Agard Wallace and American Liberalism, 1941–1948. Norman Markowitz. Reviewed by Edward L. Schapsmeier. 1333
The Rise of the Unmeltable Ethnics: The New Political Force of the Seventies. Michael Novak. Reviewed by Lawrence H. Fuchs. .. 790
The Rise of Warren Gamaliel Harding, 1865–1920. Randolph C. Downes. Reviewed by Frank A. Burd. 277
Rites of Way: *The Politics of Transportation in Boston and the U.S. City*. Alan Lupo, Frank Colcord, and Edmund P. Fowler. Reviewed by G. Ross Stephens. 282
On the Road to Communism: Essays on Soviet Domestic and Foreign Politics. Roger E. Kanet and Ivan Volgyes, eds. Reviewed by Teresa Rakowska-Harmstone. 1816
Roberts, Geoffrey K. *A Dictionary of Political Analysis*. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay*. 1696
Roberts, J. M. *The Mythology of the Secret Societies*. Reviewed by Nannerl O. Keohane. .. 265
Roberts, Walter R. *Tito, Mihailović and the Allies, 1941–1945*. Reviewed by Alex N. Dragnich. 1372
Robinson, A. N. R. *The Mechanics of Independence: Patterns of Political and Economic Transformation in Trinidad and Tobago*. Reviewed by Wendell Bell. 326
Robinson, Joan, Robert Jungk, Ernan McMullin, Joseph Needham, Stuart Schram, William Sewell, and Bernard Towers. *China and the West: Mankind Evolving*. Reviewed by John Boyle. 1399
Robinson, John P. Jerrold G. Rusk, and Kendra B. Head, eds. *Measures of Political Attitudes*. Reviewed by David E. RePass. 771
Robinson, Richard A. H. *Origins of Franco's Spain: The Right, the Republic and Revolution, 1931–1936*. Reviewed by Glen A. Waggoner. 1836
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Rohter, Ira. Review of Jeanne E. Knutson, *The Human Basis of the Polity: A Psychological Study of Political Men*. 260
The Role of Analysis in Regulatory Decisionmaking: The Case of Cable Television. Rolla Edward Park. Reviewed by Nathan Shoehalter. 1335
The Role of a National Delegation in the General Assembly. Peter Baehr. Reviewed by John O. Lindell. 1849
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Rotfeldt, David. Review of Martin Needler, *Politics and Society in Mexico;* and Daniel Cosío Villegas, *El Sistema Politico Mexicano: Las Posibilidades de Cambio*. 322
Roosevelt and World War II. Robert A. Divine. Reviewed by Arnold A. Offner. 1858
Roos, Leslie L., Jr. and Noralou P. Roos. *Managers of Modernization: Organizations and Elites in Turkey (1950–1969)*. Reviewed by James W. Land. 328
The Rope of Moka: Big-Men and Ceremonial Exchange in Mount Hagen, New Guinea. Andrew Strathern. Reviewed by Ralph R. Premdas. 1842
Rosecrance, Richard. Review of Charles Doran, *The Politics of Assimilation: Hegemony and Its Aftermath*. 860
Rose, Douglas D. and John C. Pierce. *Nonattitudes and American Public Opinion: The Examination of a Thesis*. 626
Rejoinder to "Comment" by Philip E. Converse. 661
Rose, Douglas D. *Communication* in reply to criticism of his article "National and Local Forces in State Politics" (December, 1973). . 1265
Rose, Leo. *Nepal: Strategy for Survival*. Reviewed by Merrill R. Goodall. 878
Rosenbaum, Walter A. *The Politics of Environmental Concern*. Reviewed by Harris S. Cohen. 1337
Rosenbloom, David Lee. *The Election Men*. Reviewed by Max M. Kampelman. 784
Rosenmayr, Leopold and Klaus R. Allerbeck. *Aufstand der Jugend? Neue Aspekte der Jugendsoziologie*. Reviewed by Peter H. Merkl. . 1788
Rose, Steven, ed. *CBW*. Reviewed by George H. Quester in *Chemical and Biological Warfare*. . 1285
Ross, C. D., S. B. Chrimes, and R. A. Griffiths, eds. *Fifteenth Century England, 1399–1509:*

Studies in Politics and Society. Reviewed by Arthur Joseph Slavin. 1797
Ross, Marc Howard. Review of Murray Edelman, *Politics as Symbolic Action: Mass Arousal and Quiescence*. 257
Ross, Ralph. *Obligation: A Social Theory*. Reviewed by Melvin M. Tumin. 1316
Ross, Robert S. Review of Donald Young, *American Roulette: The History and Dilemma of the Vice Presidency*, rev. ed. 796
Review of Vincent Ostrom, *The Political Theory of a Compound Republic: A Reconstruction of the Logical Foundations of American Democracy as Presented in the Federalist*. 1315
Rostow, W. W. *The Diffusion of Power, 1957–1972: An Essay in Recent History*. Reviewed by Lincoln P. Bloomfield. 1874
Review of Ronald I. McKinnon, *Money and Capital in Economic Development*. 1822
Roth, Guenther. Review of Norman Birnbaum, *Toward a Critical Sociology*. 1724
Rothman, Stanley. Review of David E. Apter and James Joll, eds., *Anarchism Today*. 1346
Rothstein, Robert L. *Planning, Prediction, and Policymaking in Foreign Affairs*. Reviewed by Bernard C. Cohen. 1407
Review of Alastair Buchan, *Power and Equilibrium in the 1970s*. 1852
Review of Bennett Boskey and Mason Willrich, eds., *Nuclear Proliferation: Prospects for Control*. 336
Rowe, Lloyd A. Review of William B. Neenan, *Political Economy of Urban Areas*. 789
Rowland, Benjamin M. and David P. Calleo. *America and the World Political Economy*. Reviewed by Ole R. Holsti. 1385
Rowley, C. D. Review of L. B. Mair, *Australia in New Guinea*, 2nd ed. 828
Rowse, A. L., Goldwin Smith, and J. H. Hexter. *The Professor and the Public: The Role of the Scholar in the Modern World*. Goldwyn Smith, compiler. Reviewed by Jurgen Herbst. 267
Rubin, Alfred P. Review of U. O. Umozurike, *Self-Determination in International Law*. 885
Rubinstein, Alvin Z. and George Ginsburg, eds. *Soviet and American Policies in the United Nations*. Reviewed by Cheryl Christensen. .. 878
Rudner, Martin. Review of J. T. Purcal, *Rice Economy, Employment and Income in Malaysia*. 326
Rudoy, Dean William. *Armed and Alone: The American Security Dilemma*. Reviewed by Thomas J. Volgy. 360
The Ruling Class: Elementi di scienza politica. Gaetano Mosca. Reviewed by Dante Germino. 261
Rummel, R. J. *The Dimensions of Nations*. Reviewed by Gordon Hilton. 1408
Runnalls, J. D., Barbara Ward, and Lenore D'Anjou, eds. *The Widening Gap: Development in the 1970s*. Reviewed by Walter Buhr. 367
Rural Politics and Social Change in the Middle East. Richard Antoun and Iliya Harik, eds. Reviewed by Douglas E. Ashford. 1345
Rusk, Jerrold G. *Comment: The American Electoral Universe: Speculation and Evidence*. ... 1028
Review of James Alden Barber, Jr., *Social Mobility and Voting Behavior*. 1754
Rusk, Jerrold G., John P. Robinson, and Kendra B. Head, eds. *Measures of Political Attitudes*. Reviewed by David E. RePass. 771
Russian Social Democracy; The Menshevik Movement: A Bibliography. Anna Bourguina. Reviewed by Stanley W. Page. 806

Safire, William. *The New Language of Politics: A Dictionary of Catchwords, Slogans & Political Usage*. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay*. 1696
Sale, Kirkpatrick. *S.D.S.* Reviewed by Edward J. Bacciocco, Jr. 1338
Salkever, Stephen G. *Virtue, Obligation and Politics*. 78
Saloma, John S., III and Frederich H. Sontag. *Parties: The Real Opportunity For Effective Citizen Politics*. Reviewed by David Nexon. . 792
Sandbrook, Richard. Review of Ben Whitaker, ed., *The Fourth World: Victims of Group Oppression*. 1381
Sandoz, Ellis. *Political Apocalypse: A Study of Dostoevsky's Grand Inquisitor*. Reviewed by Joseph Frank. 1747
Sarason, Seymour B. Review of D. Woods Thomas, Harry R. Potter, William L. Miller, and Adrian F. Aveni, eds., *Institution Building: A Model for Applied Social Change*. 268
Saslow, William S., Brown Miller, and Neil J. Pinney. *Innovation in New Communities*. Reviewed by Thomas P. Murphy. 788
Savage, Paul L. Review of Walter Ansel, *Hitler and The Middle Sea;* and Donald MacIntyre, *The Naval War Against Hitler*. 802
Sayeed, Khalid B. Review of John Badgley, *Asian Development: Problems and Prognosis*. . 1347
Sayyid Jamal ad-Din "al-Afghani": A Political Biography. Nikki R. Keddie. Reviewed by Marvin Zonis. 1310
Schaefer, Henry W. *Comecon and the Politics of Integration*. Reviewed by Andrzej Korbonski. 880
Schapiro, Leonard B. Review of James Bunyan, *The Origin of Forced Labor in the Soviet State, 1917–1921: Documents and Materials*. 296
Schapsmeier, Edward L. Review of Norman Markowitz, *The Rise and Fall of the People's Century: Henry Agard Wallace and American Liberalism, 1941–1948*. 1333
Scheingold, Stuart A. and Leon N. Lindberg,

eds. *Regional Integration: Theory and Research.* Reviewed by David P. Calleo. 1867
Scheinman, Lawrence, Robert W. Cox, Harold K. Jacobson, Gerard and Victoria Curzon, Joseph S. Nye, James P. Sewell, and Susan Strange. *The Anatomy of Influence: Decision-Making in International Organization.* Reviewed by H. Field Haviland. 1389
Schiffrin, Harold Z. Review of Li Yu-ning, *The Introduction of Socialism into China.* 854
Schirmer, Daniel B. *Republic Or Empire: American Resistance to the Philippine War.* Reviewed by Milton Plesur. 1409
Schlesinger, Thomas O. *Austrian Neutrality in Postwar Europe: The Domestic Roots of a Foreign Policy.* Reviewed by Konrad Ginther. 881
Schmeckebier, Laurence F. and Roy B. Eastin. *Government Publications and Their Use,* 2nd rev. ed. Reviewed by John Brown Mason. .. 1753
Schmidt, Steffen. Review of Arnold Strickon and Sidney M. Greenfield, eds., *Structure and Process in Latin America: Patronage, Clientage, and Power Systems.* 1377
Schneider, Herbert W. Review of Winfried Förster, *Thomas Hobbes und der Puritanismus;* and Reinhart Koselleck and Roman Schnur, eds., *Hobbes-Forschungen.* 755
Schneider, Louis and Charles Bonjean, eds. *The Idea of Culture in the Social Sciences.* Reviewed by Harry C. Triandis. 1318
Schneider, Ronald M. *The Political System of Brazil: Emergence of a "Modernizing" Authoritarian Regime, 1964–1970.* Reviewed by Riordan Roett. 1837
Schnitzer, Martin. *East and West Germany: A Comparative Economic Analysis.* Reviewed by Gerald G. Watson. 1373
Schnur, Roman and Reinhart Koselleck, eds. *Hobbes-Forschungen.* Reviewed by Herbert W. Schneider. 755
Schonfeld, William R. Review of the Institut d'Études Politiques de Bordeaux, under the direction of Albert Mabileau, *Les Facteurs locaux de la vie politique nationale.* 310
Schorsch, Ismar. *Jewish Reactions to German Anti-Semitism, 1870–1914.* Reviewed by Fritz Nova. 843
Schou, August and Arne O. Brundtland, eds. *Small States in International Relations, Nobel Symposium 17.* Reviewed by Nils Ørvik. 1875
Schramm, Percy Ernst. *Hitler: The Man and the Military Leader.* Reviewed by Gerhard L. Weinberg. 306
Schram, Stuart, Robert Jungk, Ernan McMullin, Joseph Needham, Joan Robinson, William Sewell, and Bernard Towers. *China and the West: Mankind Evolving.* Reviewed by John H. Boyle. 1399
Schutz, Alfred and Thomas Luckmann. *The Structures of the Life-World.* Richard M. Zaner and H. Tristram Engelhardt, Jr., trans. Reviewed by Hwa Yol Jung. 1319
Schwartz, Joel J. and Donald D. Searing. *Communication* in reply to criticism of their and Alden E. Lind's article "The Structuring Principle: Political Socialization and Belief Systems" (June, 1973). 725
Schwartzman, Edward. *Campaign Craftsmanship.* Reviewed by Max M. Kampelman. ... 784
Schwarzenberger, Georg and George W. Keeton, eds. *The Year Book of World Affairs, 1972.* Reviewed by Patrick J. McGowan. 352
Science, Values and Politics in Max Weber's Methodology. H. H. Bruun. Reviewed by Jerome Gittleman. 1727
The Scientific Management of Society. V. G. Afanasyev. Reviewed by Arthur Trueger. ... 253
Scott, Robert E., ed. *Latin American Modernization Problems.* Reviewed by Eldon Kenworthy. 1373
Scoville, Herbert, Jr. Review of Alton H. Quanbeck and Barry M. Blechman, *Strategic Forces: Issues for the Mid-Seventies.* 1405
S.D.S. Kirkpatrick Sale. Reviewed by Edward J. Bacciocco, Jr. 1338
Searing, Donald D. and Joel J. Schwartz. *Communication* in reply to criticism of their and Alden E. Lind's article "The Structuring Principle: Political Socialization and Belief Systems" (June, 1973). 725
Le Secrétaire Général des Nations Unies: Travaux et recherches de Science politique, #16. Marie-Claude Smouts. Reviewed by Oscar Svarlien. 882
A Secret War: Americans in China, 1944–1945. Oliver J. Caldwell. Reviewed by Allen S. Whiting. 1853
Securing the Revolution: Ideology in American Politics, 1789–1815. Richard Buel, Jr. Reviewed by Richard H. Kohn. 274
Security in Europe. Robert Hunter. Reviewed by F. S. Northedge. 1396
Seddig, Robert G. Review of Melvin I. Urofsky and David W. Levy, eds., *Letters of Louis D. Brandeis:* Vol. I: *1870–1907, Urban Reformer;* Vol. II: *1907–1912, People's Attorney;* Vol. III: *1913–1915, Progressive and Zionist.* 1340
Seeley, J. R. *The Expansion of England.* With an introduction by John Gross, ed. Reviewed by A. L. Burns. 844
Self-Determination in International Law. U. O. Umozurike. Reviewed by Alfred P. Rubin. . 885
Semidei, Manuela. Review of Luis E. Aguilar, *Cuba 1933, Prologue to Revolution.* 335
Separate and Unequal: Governmental Inequality in the Metropolis. Richard Child Hill. 1557
Sewell, James P., Robert W. Cox, Harold K. Jacobson, Gerard and Victoria Curzon, Joseph S. Nye, Lawrence Scheinman, and Susan Strange. *The Anatomy of Influence: De-*

cision-Making in International Organization. Reviewed by H. Field Haviland. 1389
Sewell, William, Robert Jungk, Ernan McMullin, Joseph Needham, Joan Robinson, Stuart Schram, and Bernard Towers. *China and the West: Mankind Evolving.* Reviewed by John H. Boyle. 1399
Seybolt, Peter J. *Revolutionary Education in China: Documents and Commentary.* Reviewed by Anthony W. Ferguson. 1375
Seyler, William C. Review of Bruce M. Stave, *The New Deal and the Last Hurrah: Pittsburgh Machine Politics.* 1779
Shadegg, Stephen C. *The New How To Win An Election.* Reviewed by Max M. Kampelman. . 784
The Shadow: Latin America Faces the Seventies. Sven Lindqvist. Keith Bradfield, trans. Reviewed by W. Raymond Duncan. 1821
Shaffer, William R. *Computer Simulations of Voting Behavior.* Reviewed by Samuel L. Popkin. 1747
Shapiro, Allan E. Review of Arie Bober, ed., *The Other Israel: The Radical Case Against Zionism.* 804
Shaw, Michael E. and Penelope H. Carson, associate eds. *The American Bibliography of Russian and East European Studies for 1966.* Fritz T. Epstein, ed. Reviewed by John A. Armstrong. 299
Shepsle, Kenneth A. and Alvin Rabushka. *Politics in Plural Societies: A Theory of Democratic Instability.* Reviewed by Fred M. Hayward. 841
Shepsle, Kenneth A. *Minimum Winning Coalitions Reconsidered: A Rejoinder to Butterworth's "Comment."* 522
On the Size of Winning Coalitions. 505
Shewmaker, Kenneth E. *Americans and Chinese Communists, 1927–1945: A Persuading Encounter.* Reviewed by Allen S. Whiting. 1853
Sheridan, James E. Review of Gerald E. Bunker, *The Peace Conspiracy: Wang Ching-wei and the China War, 1937–1941.* 1384
Sherman, Howard. *Radical Political Economy: Capitalism and Socialism from a Marxist-Humanist Perspective.* Reviewed by Richard Nordahl. 771
Shklar, Judith N. Review of Paul E. Sigmund, *Natural Law in Political Thought.* 266
Review of Raymond Plant, *Hegel.* 1744
Shoehalter, Nathan. Review of Rolla Edward Park, *The Role of Analysis in Regulatory Decision-making: The Case of Cable Television.* . 1335
A Short History of the European Working Class. Wolfgang Abendroth. Nicholas Jacobs, Brian Trench, and Joris de Bres, translators. Reviewed by Graham Wootton. 797
Short, J. F., Jr. and M. E. Wolfgang, eds. *Collective Violence.* Reviewed by N. Lucas..... 1839
Shrader, Lawrence and Richard Sisson. *Legislative Recruitment and Political Integration: Patterns of Political Linkage in an Indian State.* Reviewed by William L. Richter. 329
Sicherman, Harvey. *Aden and British Strategy, 1839–1968.* Reviewed by Aaron S. Klieman. . 1877
Sigmund, Paul E. *Natural Law in Political Thought.* Reviewed by Judith N. Shklar. 266
Silver, Brian. *Levels of Sociocultural Development Among Soviet Nationalities: A Partial Test of the Equalization Hypothesis.* 1618
Social Mobilization and the Russification of Soviet Nationalities. 45
Simon, Douglas W. Review of Norman Luxenburg, *Europe Since World War II: The Big Change.* 872
Simonsson, Ragnar. *Frankrikes författningar.* Reviewed by M. Donald Hancock. 845
The Simple Act of Voting. Stanley Kelley, Jr. and Thad W. Mirer. 572
Simpson, Richard L. and Herman Turk, eds. *Institutions and Social Exchange: The Sociologies of Talcott Parsons and C. Homans.* Reviewed by Gianfranco Poggi. 773
Singer, H. W. Review of Hla Myint, *Southeast Asia's Economy: Development Policies in the 1970s.* 1830
Singer, J. David and Melvin Small. *The Wages of War, 1816–1965: A Statistical Handbook.* Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* 243
Singer, Marshall R. *Weak States in a World of Powers.* Reviewed by Annette Baker Fox. .. 1877
Singer, Milton. *When a Great Tradition Modernizes: An Anthropological Approach to Indian Civilization.* Foreword by M. N. Srinivas. Reviewed by Richard L. Park. 1840
Sino-American Relations, 1949–1971. Roderick MacFarquhar, ed. Reviewed by Allen S. Whiting. 1853
Siracusa, Joseph M. *New Left Diplomatic Histories and Historians: The American Revisionists.* Reviewed by Ole R. Holsti. 1878
Sisson, Richard and Lawrence Shrader. *Legislative Recruitment and Political Integration: Patterns of Political Linkage in an Indian State.* Reviewed by William L. Richter. 329
El Sistema Politico Mexicano: Las Posibilidades de Cambio. Daniel Cosío Villegas. Reviewed by David Ronfeldt. 322
Size and Democracy. Robert A. Dahl and Edward R. Tufte. Reviewed by Heinz Eulau. .. 1301
On the Size of Winning Coalitions. Kenneth A. Shepsle. 505
Slater, Jerome. *Intervention and Negotiation: The United States and the Dominican Revolution.* Reviewed by Michael J. Kryzanek. 362
Slavin, Arthur Joseph. Review of S. B. Chrimes,

C. D. Ross, and R. A. Griffiths, eds., *Fifteenth Century England, 1399–1509: Studies in Politics and Society*. 1797
Slusser, Robert M. *The Berlin Crisis of 1961: Soviet-American Relations and the Struggle for Power in the Kremlin, June–November 1961*. Reviewed by Werner J. Feld. 1410
Small, Melvin and J. David Singer. *The Wages of War, 1816–1965: A Statistical Handbook*. Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science*. 243
Small States in International Relations, Nobel Symposium 17. August Schou and Arne O. Brundtland, eds. Reviewed by Nils Ørvik.... 1875
Smith, Denis Mack. *Victor Emanuel, Cavour, and the Risorgimento*. Reviewed by Giovanni Bognetti. 330
Smith, Edward C. and Arnold J. Zurcher. *Dictionary of American Politics*, 2nd ed. Reviewed by Clement E. Vose in *Political Dictionaires: A Bibliographical Essay*..... 1696
Smith, Goldwin, A. L. Rowse, and J. H. Hexter. *The Professor and the Public: The Role of the Scholar in the Modern World*. Goldwyn Smith, compiler. Reviewed by Jurgen Herbst. 267
Smith, Ralph. *Vietnam and the West*. Reviewed by Robert L. Youngblood. 1375
Smolansky, O. M. Review of Tarun Chandra Bose, *The Superpowers and the Middle East*... 1382
Smouts, Marie-Claude. *Le Secrétaire Général des Nations Unies: Travaux et recherches de Science politique, #16*. Reviewed by Oscar Svarlien. 882
Smucker, Joseph. Review of Jules Cohn, *The Conscience of the Corporations: Business and Urban Affairs, 1967–1970*. 1760
Smyth, Douglas C. Review of Shashishekhar Jha, *Political Elite in Bihar*. 1361
Snyder, Kay A. and Thomas C. Nowak. *Clientelist Politics in the Philippines: Integration or Instability?* 1147
Social Change and Political Mobilization in the United States: 1870–1960. Jesse F. Marquette. 1058
The Social Contexts of Research. Saad Z. Nagi and Ronald G. Corwin, eds. Reviewed by Arnold Thackray. 1314
Social Groups in Polish Society. David Lane and George Kolankiewicz, eds. Reviewed by Szymon Chodak. 1364
Socialism in America: From the Shakers to the Third International. Albert Fried, ed. Reviewed by Gary D. Glenn. 1764
Social Mobility and Voting Behavior. James Alden Barber, Jr. Reviewed by Jerrold G. Rusk. 1754
Social Mobilization and the Russification of Soviet Nationalities. Brian Silver. 45
The Social Origins of Liberal Democracy: The Swedish Case. Timothy A. Tilton. 561
Social Radicalism and the Arts: Western Europe. Donald Drew Egbert. Reviewed by Louis Wasserman. 753
Social Research and the Desegregation of the U.S. Army. Leo Bogart, ed. Reviewed by William F. Levantrosser. 1758
Social Science and the Idea of Process: The Ambiguous Legacy of Arthur F. Bentley. Paul F. Kress. Reviewed by Charles B. Hagan. 764
Social Science Literature: A Bibliography for International Law. Wesley L. Gould and Michael Barkun. Reviewed by Anthony D'Amato. 1394
Social Security: Universal or Selective? Wilbur J. Cohen and Milton Friedman. Reviewed by Louis L. Friedland. 777
Société et Politique: La vie des Groupes. Tome premier: Fondements de la Société Libérale. Tome second: Dynamique de la Société Libérale. Léon Dion. Reviewed by Henry W. Ehrmann. 1733
Society, Culture, and Change in the Middle East, 3rd ed. Raphael Patai. Reviewed by Amal Vinogradov. 840
A Socioeconomic Profile of South Africa. William R. Duggan. Reviewed by Thomas Karis. ... 1391
Sohn, Louis B. Review of E. Lauterpacht, ed., *International Law: Collected Papers of Hersch Lauterpacht. Vol. I: General Works*. 1865
Sohnquist, John A. *Multivariate Model Building: The Validation of a Search Strategy*. Reviewed by Bernard Grofman. 1749
Soja, Edward W. *The Geography of Modernization in Kenya: A Spatial Analysis of Social, Economic, and Political Change*. Reviewed by Jay E. Hakes. 331
Sontag, Frederich H. and John S. Saloma, III. *Parties: The Real Opportunity For Effective Citizen Politics*. Reviewed by David Nexon. . 792
The South African Voter. H. Lever. Reviewed by Pierre L. van den Berghe. 318
South Africa: Sociological Perspectives. Heribert Adam, ed. Reviewed by Edwin S. Munger. .. 1785
Southeast Asia's Economy: Development Policies in the 1970s. Hla Myint. Reviewed by H. W. Singer. 1830
The Southern Expansion of the Chinese People. C. P. FitzGerald. Reviewed by D. G. E. Hall. 1802
Soviet and American Policies in the United Nations. Alvin Z. Rubinstein and George Ginsburg, eds. Reviewed by Cheryl Christensen. . 878
Soviet and American Society: A Comparison. Paul Hollander. Reviewed by Zygmunt Bauman. 1811
The Soviet Armed Forces, Books in English, 1950–1967. Michael Parrish. Hoover Institution Bibliographical Series: XLVIII. Reviewed by Leon Goure. 1370

Soviet Policy toward Black Africa: The Focus on National Integration. Helen D. Cohn. Reviewed by Vernon McKay. 1388
Soviet Relations with India and Pakistan. Davendra Kaushik. Reviewed by Paul Gerhardt. 1400
Soviet Russia and the Middle East. Aaron S. Klieman. Reviewed by Amos Perlmutter. ... 1864
The Soviet Union and Emerging Nations: A Case Study of Soviet Policy Towards India. Harish Kapur. Reviewed by Bernard S. Morris. 868
Spain, David H., William M. O'Barr, and Mark A. Tessler, eds. *Survey Research in Africa: Its Applications and Limits.* Reviewed by Michael Armer. 835
Spending to Save: The Complete Story of Relief. Harry L. Hopkins. Reviewed by Frank T. Colon. 786
Sperlich, Peter W. *Conflict and Harmony in Human Affairs: A Study of Cross-Pressures and Political Behavior.* Reviewed by Ada W. Finifter. 1749
Spinoza's Philosophy of Law. Gail Belaief. Reviewed by Douglas Sturm. 1724
Spotts, Frederic. *The Churches and Politics in Germany.* Reviewed by Guenter Lewy. 1376
Sprout, Margaret and Harold Sprout. *Toward a Politics of the Planet Earth.* Reviewed by Henry L. Mason. 883
Stability and Change in Congress. Barbara Hinckley. Reviewed by Randall B. Ripley. .. 280
Starr, Harvey. *War Coalitions: The Distributions of Payoffs and Losses.* Reviewed by James Lee Ray. 1880
Starr, S. Frederick. *Decentralization and Self-Government in Russia, 1830–1870.* Reviewed by Marc Raeff. 332
Starzinger, Vincent E. Review of Ellen Meiksins Wood, *Mind and Politics.* 1321
Stave, Bruce M. *The New Deal and the Last Hurrah: Pittsburgh Machine Politics.* Reviewed by William C. Seyler. 1779
Staveley, E. S. *Greek and Roman Voting and Elections.* Reviewed by Charles D. Hamilton. 847
Steffani, Winfried and Franz Nuscheler, eds. *Pluralismus: Konzeptionen und Kontroversen.* Reviewed by Henry W. Ehrman. 1733
Steffens, Joan and Hesung Chun Koh, eds. *Korea: An Analytical Guide to Bibliographies.* Reviewed by Sungjoo Han. 827
Stepanov, L., Y. Zhukov, L. Delyusin, and A. Iskenderov. *The Third World: Problems and Prospects.* Reviewed by Edward Taborsky. .. 889
Stephens, G. Ross. Review of Alan Lupo, Frank Colcord, and Edmund P. Fowler, *Rites of Way: The Politics of Transportation in Boston and the U.S. City.* 282
Stephens, Otis H., Jr. *The Supreme Court and Confessions of Guilt.* Reviewed by David Chadwick-Brown. 1780
Stern, David. Review of Robert D. Reischauer and Robert W. Hartman, *Reforming School Finance.* 1778
Sternhell, Zeev. *Maurice Barrès et le nationalisme français.* Reviewed by Eugen Weber. 848
Stevens, Arthur G., Jr., Arthur H. Miller, and Thomas E. Mann. *Mobilization of Liberal Strength in the House, 1955–1970: The Democratic Study Group.* 667
Stevenson, Garth. Review of Lynn H. Miller, *Organizing Mankind: An Analysis of Contemporary International Organization.* 356
Stevenson, Hugh M., Donald G. Morrison, Robert C. Mitchell, John N. Paden, and others. *Black Africa: A Comparative Handbook.* Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* 243
Stillman, Peter G. *Hegel's Critique of Liberal Theories of Rights.* 1086
Stillman, Richard J., II. *Integration of the Negro in the U.S. Armed Forces.* Reviewed by William F. Levantrosser. 1758
Stinchcombe, Arthur L. *Communication* on systems analyses. 1262
Stinchcombe, William C. *The American Revolution and the French Alliance.* Reviewed by Alexander DeConde. 1881
Stockholm International Peace Research Institute. *The Problem of Chemical and Biological Warfare,* 6 vols. Reviewed by George H. Quester in *Chemical and Biological Warfare.* . 1285
Resources Devoted to Military Research and Development: An International Comparison. Reviewed by Barry M. Blechman. 1841
Stoessinger, John G. Review of E. Berkeley Tompkins, ed., *The United Nations in Perspective.* 366
The Strange Neutrality: Soviet-Japanese Relations during the Second World War, 1941–1945. George Alexander Lensen. Reviewed by Max Beloff. 354
Strange, Susan, Robert W. Cox, Harold K. Jacobson, Gerard and Victoria Curzon, Joseph S. Nye, Lawrence Scheinman, and James P. Sewell. *The Anatomy of Influence: Decision-Making in International Organization.* Reviewed by H. Field Haviland..... 1389
Strategic Forces: Issues for the Mid-Seventies. Alton H. Quanbeck and Barry M. Blechman. Reviewed by Herbert Scoville, Jr. 1405
Strategic Power and National Security. Joseph I. Coffey. Reviewed by Jerome H. Kahan. 340
The Strategy of Technology: Winning the Decisive War. Stefan T. Possony and J. E. Pournelle. Reviewed by George H. Quester. . 1872
Strathern, Andrew. *The Rope of Moka: Big-Men and Ceremonial Exchange in Mount Hagen,*

New Guinea. Reviewed by Ralph R. Premdas. 1842
Strauss, Anselm L. *The Contexts of Social Mobility: Ideology and Theory.* Reviewed by Edward O. Laumann. 1781
Strickon, Arnold and Sidney M. Greenfield, eds. *Structure and Process in Latin America: Patronage, Clientage, and Power Systems.* Reviewed by Steffen Schmidt. 1377
Structural Change and Economic Policy in Israel. Howard Pack. Reviewed by Ruth Klinov. .. 324
Structure and Process in Latin America: Patronage, Clientage, and Power Systems. Arnold Strickon and Sidney M. Greenfield, eds. Reviewed by Steffen Schmidt. 1377
The Structures of the Life-World. Alfred Schutz and Thomas Luckmann. Richard M. Zaner and H. Tristram Engelhardt, Jr., trans. Reviewed by Hwa Yol Jung. 1319
Struve, Walter. Review of August Heinrich Winkler, *Mittelstand, Demokratie und Nationalsozialismus: Die politische Entwicklung von Handwerk und Kleinhandel in der Weimarer Republik.* 851
Review of Ger van Roon, *German Resistance to Hitler: Count von Moltke and the Kreisau Circle.* 1844
Studies in Ibo Political Systems: Chieftaincy and Politics in Four Niger States. Ikenna Nzimiro. Reviewed by Alvin Magid. 1831
The Study of International Affairs: Essays in Honour of Kenneth Younger. Roger Morgan, ed. Reviewed by John H. Herz. 1404
The Study of International Politics Makes Strange Bedfellows: Theories of the Radical Right and the Radical Left. Ole R. Holsti. 217
Sturm, Douglas. Review of Gail Belaief, *Spinoza's Philosophy of Law.* 1724
Sugar, Peter and Ivo Lederer, eds. *Nationalism in Eastern Europe.* Reviewed by Rudolf L. Tökés. 849
Sundquist, James L. with the collaboration of David W. Davis. *Making Federalism Work: A Study of Program Coordination at the Community Level.* Reviewed by Vincent Ostrom. . 792
The Superpowers and the Middle East. Tarun Chandra Bose. Reviewed by O. M. Smolansky. 1382
The Supreme Court and Confessions of Guild. Otis H. Stephens, Jr. Reviewed by David Chadwick-Brown. 1780
The Supreme Court and Religion. Richard E. Morgan. Reviewed by Jacob W. Landynski. . 285
A Survey of Chemical and Biological Warfare. John Cookson and Judith Nottingham. Reviewed by George H. Quester in *Chemical and Biological Warfare.* 1285
Survey Research in Africa: Its Applications and Limits. William M. O'Barr, David H. Spain, and Mark A. Tessler, eds. Reviewed by Michael Armer. 835
Surveys and Research Corporation. *Directory of Selected Scientific Institutions in Mainland China.* Reviewed by Susan Beth Rifkin. 333
Svarlien, Oscar. Review of Marie-Claude Smouts, *Le Secrétaire Général des Nations Unies: Travaux et recherches de Science politique, #16.* 882
Swan, Philip L. and Karel Holbrik. *Trade and Industrialization in the Central American Common Market, the First Decade.* Reviewed by Melville E. Osborne. 824
Swaziland: The Dynamics of Political Modernization. Christian P. Potholm. Reviewed by Sheridan W. Johns, III. 1833
Szalai, Alexander. *The United Nations and the News Media.* With Margaret Croke and Associates. Reviewed by Leon Gordenker. 1881

Taborsky, Edward. Review of Y. Zhukov, L. Delyusin, A. Iskenderov, and L. Stepanov, *The Third World: Problems and Prospects.* .. 889
Tallon, Denis and Mauro Cappelletti, eds. *Fundamental Guarantees of the Parties in Civil Litigation: Studies in National, International and Comparative Law.* Reviewed by Richard Pierre Claude. 1386
Tammany: Evolution of a Political Machine, 1789–1865. Jerome Mushkat. Reviewed by Joel A. Tarr. 1773
Tanter, Raymond and Richard H. Ullman, eds. *Theory and Policy in International Relations.* Reviewed by Lloyd Jensen. 363
Tarr, Joel A. Review of Jerome Mushkat, *Tammany: Evolution of a Political Machine, 1789–1865.* 1773
Tassi, Aldo. Review of Alexis de Tocqueville, *Journey to America.* George Lawrence, trans. J. P. Mayer, ed. 254
Tate, C. Neal. *Individual and Contextual Variables in British Voting Behavior: An Exploratory Note.* 1656
Taylor, A. J. P. Review of Norman Rich, *Hitler's War Aims: Ideology, the Nazi State, and the Course of Expansion.* 1406
Taylor, Charles Lewis, Michael C. Hudson, and others. *World Handbook of Political and Social Indicators,* 2nd ed. Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* 243
Taylor, Peter J. *Communication* in reply to comments on his article "A New Shape Measure for Evaluating Electoral District Patterns" (September, 1973). 1276
Teachers' Unions and Interest Group Politics: A Study in the Behaviour of Organised Teachers in England and Wales. R. D. Coates. Reviewed by James B. Christoph. 1798
Technology Assessment: Understanding the Social

Consequences of Technological Applications. Raphael G. Kaspar, ed. Reviewed by Victor Ferkiss. 1310
Technology for Developing Nations: New Directions for U.S. Technical Assistance. Rutherford M. Poats. Reviewed by Brewster C. Denny. 1371
Technology of Necessity: Scientific and Engineering Development in Israel. Roy Popkin. Reviewed by Amos Perlmutter. 815
Tedin, Kent L. *The Influence of Parents on the Political Attitudes of Adolescents.* 1579
Television and the Public. Robert T. Bower. Reviewed by Paul Conway. 1322
Telling Tongues: Language Policy in Mexico, Colony to Nation. Shirley Brice Heath. Reviewed by Martin C. Needler. 821
Terrill, Ross and Bruce Douglass, eds. *China and Ourselves: Explorations and Revisions by a New Generation.* Reviewed by Oliver M. Lee. 814
Terror and Resistance: A Study of Political Violence, with Case Studies of Some Primitive African Communities. Eugene Victor Walter. Reviewed by Ali A. Mazrui. 271
Tessler, Mark A., William M. O'Barr, and David H. Spain, eds. *Survey Research in Africa: Its Applications and Limits.* Reviewed by Michael Armer. 835
Thackray, Arnold. Review of Saad Z. Nagi and Ronald G. Corwin, eds., *The Social Contexts of Research.* 1314
Theis, Paul A. and Edmund L. Henshaw, Jr., eds. *Who's Who in American Politics: 1973–1974,* 4th ed. Reviewed by Charles O. Jones. . 1339
Theodoulou, Christos. *Greece and the Entente: August 1, 1914–September 25, 1916.* Reviewed by D. George Kousoulas. 1883
Theory and Policy in International Relations. Raymond Tanter and Richard H. Ullman, eds. Reviewed by Lloyd Jensen. 363
Theory and Voting Research: Some Reflections on Converse's "Change in the American Electorate." Walter Dean Burnham. 1002
The Third World: Problems and Prospects. Y. Zhukov, L. Delyusin, A. Iskenderov, and L. Stepanov. Reviewed by Edward Taborsky. .. 889
Thomas, A. J. Jr. and Ann Van Wynen Thomas. *The Concept of Agression in International Law.* Reviewed by Norman V. Walbek. 365
Thomas, Ann Van Wynen and A. J. Thomas, Jr. *The Concept of Aggression in International Law.* Reviewed by Norman V. Walbek. 365
Thomas, D. Woods, Harry R. Potter, William L. Miller, and Adrian F. Aveni, eds. *Institution Building: A Model for Applied Social Change.* Reviewed by Seymour B. Sarason. . 268
Thomas Hobbes und der Puritanismus. Winfried Förster. Reviewed by Herbert W. Schneider. 755
Thompson, Kenneth W. Review of Alfred O. Hero, Jr., *American Religious Groups View Foreign Policy: Trends in Rank-and-File Opinion, 1937–1969.* 186
Review of Dennis Gabor, *The Mature Society.* 75
Thompson, Victor A. Review of Holton P. Odegard, *The Politics of Truth.* 174
Thompson, Virginia and Richard Adloff. *The French Pacific Islands: French Polynesia and New Caledonia.* Reviewed by Francis West. . 184
Thornton, Robert L. Review of Robert C. Fraser, Alan D. Donheiser, and Thomas G. Miller, *Civil Aviation Development: A Policy and Operating Analysis;* and Richard Y. Chuang, *The International Air Transport Association.* 85
Review of Robert L. Fruhan, *The Fight for Competitive Advantage: A Study of the United States Domestic Air Trunk Carriers.* 132
The 3/2's Rule in Presidential Campaigning. Steven J. Brams and Morton D. Davis. 11
Thucydides' View of Athenian Imperialism. Christopher Bruell. 1
Tidmarch, Charles M. Review of David W. Brady, *Congressional Voting in a Partisan Era: A Study of the McKinley Houses and A Comparison to the Modern House of Representatives.* 132
Tilman, Robert O. Review of James W. Gould, *The United States and Malaysia.* 186
Review of Stephen Fitzgerald, *China and the Overseas Chinese: A Study of Peking's Changing Policy, 1949–1970.* 86
Tilton, Timothy A. *The Social Origins of Liberal Democracy: The Swedish Case.* 56
Tinder, Glenn. *Transcending Tragedy: The Idea of Civility.* 54
Tito, Mihailović and the Allies, 1941–1945. Walter R. Roberts. Reviewed by Alex N. Dragnich. 137
Tocqueville, Alexis de. *Journey to America.* George Lawrence, trans. J. P. Mayer, ed. Reviewed by Aldo Tassi. 25
Tocqueville. Hugh Brogan. Reviewed by J. P. Mayer. 129
Tökés, Rudolf L. Review of Peter Sugar and Ivo Lederer, eds., *Nationalism in Eastern Europe.* . 84
Tollison, Robert D. and Ryan C. Amacher. Review of K. H. Kim, Susan Farrell, and Ewan Clague, *The All-Volunteer Army: An Analysis of Demand and Supply;* and Harry A. Marmion, *The Case Against a Volunteer Army.* .. 78
Tompkins, E. Berkeley, ed. *The United Nations in Perspective.* Reviewed by John G. Stoessinger. 36
Toothing Stones: Rethinking the Political. Robert E. Meagher, ed. Reviewed by D. M. White. .. 131
Toulmin, Stephen E. Review of Hanna Fenichel Pitkin, *Wittgenstein and Justice: On the Sig-*

nificance of Ludwig Wittgenstein for Social and Political Thought. 769
Toward a Critical Sociology. Norman Birnbaum. Reviewed by Guenther Roth. 1724
Toward a Model of Soviet Decision Making: A Research Note. Donald R. Kelley. 701
Toward a Politics of the Planet Earth. Harold Sprout and Margaret Sprout. Reviewed by Henry L. Mason. 883
Towards a New Theory of Distributive Justice. Norman E. Bowie. Reviewed by Edward J. Erler. 749
Towers, Bernard, Robert Jungk, Ernan McMullin, Joseph Needham, Joan Robinson, Stuart Schram, and William Sewell. *China and the West: Mankind Evolving.* Reviewed by John H. Boyle. 1399
Trade and Industrialization in the Central American Common Market, the First Decade. Karel Holbrik and Philip L. Swan. Reviewed by Melville E. Osborne. 824
Tradition and Nationality: A Classic Revisited. Samuel Beer. Review of Walter Bagehot, *The English Constitution.* With an introduction by R. H. S. Crossman, M.P. 1292
Trager, Frank N. Review of Mya Maung, *Burma and Pakistan: A Comparative Study of Development.* 1828
Transcending Tragedy: The Idea of Civility. Glenn Tinder. 547
A Treatise on Political Economy. Antoine Louis Claude Destutt de Tracy. Reviewed by G. David Garson. 750
Triandis, Harry C. Review of Louis Schneider and Charles Bonjean, eds., *The Idea of Culture in the Social Sciences.* 1318
Troubled Alliance: Turkish-American Problems in Historical Perspective, 1945–1971. George S. Harris. Reviewed by Richard H. Pfaff. ... 348
Trueger, Arthur. Review of V. G. Afanasyev, *The Scientific Management of Society.* 253
Tufte, Edward R. *Communication* in reply to comments on his article "The Relationship Between Seats and Votes in Two Party Systems" (June, 1973). 207
Tufte, Edward R. and Robert A. Dahl. *Size and Democracy.* Reviewed by Heinz Eulau. 1301
Tugwell, Rexford G. *In Search of Roosevelt.* Reviewed by Thomas H. Geer. 793
Tullock, Gordon. *Communication* on William H. Riker and Steven J. Brams, "The Paradox of Vote Trading" (December, 1973). 1687
Tumin, Melvin M. Review of Ralph Ross, *Obligation: A Social Theory.* 1316
Turk, Herman and Richard L. Simpson, eds. *Institutions and Social Exchange: The Sociologies of Talcott Parsons and C. Homans.* Reviewed by Gianfranco Poggi. 773
Twitchett, Kenneth J., ed. *The Evolving United Nations: A Prospect for Peace?* Reviewed by Harold K. Jacobson. 884
Two Concepts of the Rule of Law. Gottfried Dietze. Reviewed by Stephen L. Wasby. 1303

Uganda: A Case Study in African Political Development. Peter M. Gukiina. Reviewed by James H. Mittelman. 303
Ullman, Richard H. and Raymond Tanter, eds. *Theory and Policy in International Relations.* Reviewed by Lloyd Jensen. 363
Umozurike, U. O. *Self-Determination in International Law.* Reviewed by Alfred P. Rubin. . 885
Undeclared War and Civil Disobedience: The American System in Crisis. Lawrence R. Velvel. Reviewed by W. Taylor Reveley, III. . 287
The United Nations and the News Media. Alexander Szalai. With Margaret Croke and Associates. Reviewed by Leon Gordenker. 1881
The United Nations in Perspective. E. Berkeley Tompkins., ed. Reviewed by John G. Stoessinger. 366
The United States and China, 3rd ed. John King Fairbank. Reviewed by C. P. FitzGerald. ... 344
The United States, Communism, and the Emergent World. Bernard P. Kiernan. Reviewed by Roger E. Kanet. 1401
The United States and Malaysia. James W. Gould. Reviewed by Robert O. Tilman. ... 1860
The United States and World War II: Military and Diplomatic Documents. A. Russell Buchanan, ed. Reviewed by Manfred Jonas. ... 780
The Unknown Dimension: European Marxism Since Lenin. Dick Howard and Karl Klare, eds. Reviewed by James R. Ozinga. 762
Urbanization, Planning and National Development. John Friedmann. Reviewed by Jorge I. Dominguez. 1354
Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities. Wayne A. Cornelius. 1125
Urofsky, Melvin I. and David W. Levy, eds. *Letters of Louis D. Brandeis:* Vol. I: *1870–1907, Urban Reformer;* Vol. II: *1907–1912, People's Attorney;* Vol. III: *1913–1915, Progressive and Zionist.* Reviewed by Robert G. Seddig. 1340
U.S. Congressional Directories, 1798–1840. Perry M. Goldman and James S. Young, eds. Reviewed by Nelson W. Polsby. 1328
Usdan, Michael D., David W. Minar, and Emanuel Hurwitz, Jr. *Education and State Politics.* Reviewed by Carl Grafton. 794
U.S. Department of State. *Diplomacy for the 70s: A Program of Management Reform for the Department of State.* Reviewed by William I. Bacchus in *Diplomacy for the 70s: An Afterview and Appraisal.* 736

U.S. International Business and Governments. Jack N. Behrman. Reviewed by Vincent J. Fuccillo. 855

Vallières, Pierre. *Choose!* Reviewed by Peter Regenstreif. 808
Vallier, Ivan. Review of David E. Mutchler, *The Church as a Political Factor in Latin America: With Special Reference to Colombia and Chile.* 832
van den Berghe, Pierre L. Review of H. Lever, *The South African Voter.* 318
van den Haag, Ernest. *Political Violence and Civil Disobedience.* Reviewed by Jeffrie G. Murphy. 269
Van Duinen, Elizabeth and Jerald G. Bachman. *Youth Looks at National Problems: A Special Report from the Youth in Transition Project.* Reviewed by Jarol B. Manheim. 1296
van Roon, Ger. *German Resistance to Hitler: Count von Moltke and the Kreisau Circle.* Peter Ludlow, trans. Reviewed by Walter Struve. 1844
Velvel, Lawrence R. *Undeclared War and Civil Disobedience: The American System in Crisis.* Reviewed by W. Taylor Reveley, III. 287
Vennema, Alje, J. B. Neilands, G. H. Orians, E. W. Pfeiffer, and Arthur H. Westing. *Harvest of Death.* Reviewed by George H. Quester in *Chemical and Biological Warfare.* 1285
Verhaegen, Benoit. *Rébellions au Congo.* Reviewed by Rene Lemarchand. 1845
Victor Emanuel, Cavour, and the Risorgimento. Denis Mack Smith. Reviewed by Giovanni Bognetti. 330
Vietnam and the West. Ralph Smith. Reviewed by Robert L. Youngblood. 1375
Vietnamese Anticolonialism: 1885–1925. David G. Marr. Reviewed by George Mc.T. Kahin. 829
Villegas, Daniel Cosío. *El Sistema Politico Mexicano: Las Posibilidades de Cambio.* Reviewed by David Ronfeldt. 322
Vinogradov, Amal. Review of Raphael Patai, *Society, Culture, and Change in the Middle East,* 3rd ed. 840
Virtue, Obligation and Politics. Stephen G. Salkever. 78
Vogel, Ezra F. Review of James Pinckney Harrison, *The Long March to Power: A History of the Chinese Communist Party, 1921–72.* 1810
Volgyes, Ivan and Roger E. Kanet, eds. *On the Road to Communism: Essays on Soviet Domestic and Foreign Politics.* Reviewed by Teresa Rakowska-Harmstone. 1816
Volgy, Thomas J. Review of Dean William Rudoy, *Armed and Alone: The American Security Dilemma.* 360
Vosburgh, John A., Irvin L. White, and Clifton E. Wilson. *Law and Politics in Outer Space: A Bibliography.* Reviewed by Robert H. Puckett. 370
Vose, Clement E. *Political Dictionaries: A Bibliographical Essay.* Review of Charles Abrams, *The Language of Cities: A Glossary of Terms;* Maurice Cranston and Sanford A. Lakoff, eds., *A Glossary of Political Ideas;* Joseph Dunner, ed., *Dictionary of Political Science;* Florence Elliott, *A Dictionary of Politics,* 6th ed.; Walter Laqueur, ed., *A Dictionary of Politics;* Edward Luttwak, *A Dictionary of Modern War;* Jack C. Plano and Milton Greenberg, *The American Political Dictionary,* 3rd ed. Geoffrey K. Roberts, *A Dictionary of Political Analysis;* William Safire, *The New Language of Politics: A Dictionary of Catchwords, Slogans & Political Usage;* and Edward C. Smith and Arnold J. Zurcher, *Dictionary of American Politics,* 2nd ed. 1696

Wädekin, Karl-Eugen. *The Private Sector in Soviet Agriculture,* 2nd rev. ed. of *Privatproduzenten in der sowjetischen Landwirtschaft.* G. F. Karcz, ed. K. Bush, trans. Reviewed by James R. Millar. 1379
The Wages of War, 1816–1965: A Statistical Handbook. J. David Singer and Melvin Small. Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* 243
Waggoner, Glen A. Review of Richard A. H. Robinson, *Origins of Franco's Spain: The Right, The Republic and Revolution, 1931–1936.* 1836
Walbek, Norman V. Review of Ann Van Wynen Thomas and A. J. Thomas, Jr., *The Concept of Aggression in International Law.* . 365
Waldock, Sir Humphrey and R. Y. Jennings, eds. *The British Year Book of International Law, 1970, Vol. 44.* Reviewed by Oliver J. Lissitzyn. 887
Walker, Malcolm T. *Politics and the Power Structure: A Rural Community in the Dominican Republic.* Reviewed by G. Pope Atkins. . 1846
Walker, Richard L., ed. *Prospects in the Pacific.* With an introduction by the editor. Reviewed by Willard H. Elsbree. 1883
Wallace, Paul. Review of S. R. Mehrotra, *The Emergence of the Indian National Congress.* . 1367
Walter, Benjamin, Frederick M. Wirt, Francine Rabinovitz, and Deborah Hensler. *On the City's Rim: Politics and Policy in Suburbia.* Reviewed by John H. Baker. 291
Walter, Eugene Victor. *Terror and Resistance: A Study of Political Violence, with Case Studies of Some Primitive African Communities.* Reviewed by Ali A. Mazrui. 271
Walton, Hanes, Jr. *Black Political Parties: An Historical and Political Analysis.* Reviewed by William R. Keech. 1341
Wamsley, Gary L. and Mayer N. Zald. *The Po-*

litical Economy of Public Organizations: A Critique and Approach to the Study of Public Administration. Reviewed by Charles H. Levine. 795
Wanat, John. *Bases of Budgetary Incrementalism.* 1221
War Coalitions: The Distributions of Payoffs and Losses. Harvey Starr. Reviewed by James Lee Ray. 1880
Ward, Barbara, J. D. Runnalls, and Lenore D'Anjou, eds. *The Widening Gap: Development in the 1970s.* Reviewed by Walter Buhr. 367
Ward, Robert E. APSA Presidential Address, 1973: *Culture and the Comparative Study of Politics, or The Constipated Dialectic.* 190
The War Industries Board: Business-Government Relations During World War I. Robert D. Cuff. Reviewed by Robert A. Kagan. 1325
Warnecke, Steven Joshua, ed. *The European Community in the 1970s.* Reviewed by Robert L. Pfaltzgraff, Jr. 369
The Warsaw Pact: Case Studies in Communist Conflict Resolution. Robin Alison Remington. Reviewed by Adam Bromke. 876
Wasby, Stephen L. Review of Gottfried Dietze, *Two Concepts of the Rule of Law.* 1303
Wasserman, Louis. Review of Donald Drew Egbert, *Social Radicalism and the Arts: Western Europe.* 753
Waterbury, John. *North for the Trade: The Life and Times of a Berber Merchant.* Reviewed by I. William Zartman. 850
The Water Lords. James M. Fallows, Reviewed by Dean E. Mann. 1762
Watson, Gerald G. Review of Martin Schnitzer, *East and West Germany: A Comparative Economic Analysis.* 1373
Weak States in a World of Powers. Marshall R. Singer. Reviewed by Annette Baker Fox. ... 1877
Weapons Technology and Arms Control. W. F. Biddle. Reviewed by Bernard Brodie. 1382
The Weary and the Wary: U.S. and Japanese Security Policies in Transition. Robert E. Osgood. Reviewed by Frank C. Langdon. 874
Weber, Arnold R. *In Pursuit of Price Stability: The Wage-Price Freeze of 1971.* Reviewed by William A. Niskanen. 1342
Weber, Eugen. Review of Theodore Zeldin, *France, 1848–1945. Volume I: Ambition, Love, and Politics.* 1847
Review of Zeev Sternhell, *Maurice Barrès et le nationalisme français.* 848
Weinbaum, Marvin G. Review of Marvin Zonis, *The Political Elite of Iran.* 334
Weinberg, Gerhard L. Review of Hajo Holborn, *Republic to Reich: The Making of the Nazi Revolution;* Eberhard Jäckel, *Hitler's Weltanschauung: A Blueprint for Power;* and Percy Ernest Schramm, *Hitler: The Man and the Military Leader.* 306
Weiner, Myron. Review of Rajni Kothari, *Politics in India.* 1363
Weintraub, Ruth G. Review of Jonathan D. Casper, *American Criminal Justice: The Defendant's Perspective;* Jack D. Douglas, ed., *Crime and Justice in American Society;* and John T. Elliff, *Crime, Dissent, and the Attorney General: The Justice Department in the 1960s.* 1324
Weisberg, Herbert F. *Models of Statistical Relationship.* 1638
Weiss, Richard. Review of Otis L. Graham, Jr., *The Great Campaigns: Reform and War in America, 1900–1928.* 783
Weistart, John C., ed. *Community Economic Development.* Reviewed by Emmett H. Buell, Jr. 288
Wenk, Edward, Jr. *The Politics of the Ocean.* Reviewed by Donald E. Milsten. 1343
Weschler, Louis F. Review of Russell D. Murphy, *Political Entrepreneurs and Urban Poverty.* 1771
Westefield, Louis P. *Majority Party Leadership and the Committee System in the House of Representatives.* 1593
West, Francis. Review of Virginia Thompson and Richard Adloff, *The French Pacific Islands: French Polynesia and New Caledonia.* 1843
Review of W. J. Hudson, ed., *Australia and Papua New Guinea.* 308
Westing, Arthur H., J. B. Neilands, G. H. Orians, E. W. Pfeiffer, and Alje Vennema. *Harvest of Death.* Reviewed by George H. Quester in *Chemical and Biological Warfare.* . 1285
Whaley, Barton. Review of Janet Ziegler, comp., *World War II: Books in English, 1945–65.* .. 1884
When a Great Tradition Modernizes: An Anthropological Approach to Indian Civilization. Milton Singer. Foreword by M. N. Srinivas. Reviewed by Richard L. Park. 1840
Whitaker, Ben, ed. *The Fourth World: Victims of Group Oppression.* Reviewed by Richard Sandbrook. 1381
White, D. M. Review of Robert E. Meagher, ed., *Toothing Stones: Rethinking the Political.* ... 1313
White, G. Edward. Review of Albert A. Ehrenzweig, *Psychoanalytic Jurisprudence: On Ethics, Aesthetics, and Law—On Crime, Tort, and Procedure.* 754
White, Irvin L., Clifton E. Wilson, and John A. Vosburgh. *Law and Politics in Outer Space: A Bibliography.* Reviewed by Robert H. Puckett. 370
White, Lynn T., III. Review of Robert E. Gamer, *The Politics of Urban Development in Singapore.* 1355
Whiting, Allen S. Review of Oliver J. Caldwell, *A Secret War: Americans in China, 1944–1945;* John Paton Davies, Jr., *Dragon by the Tail;* Chin-tung Liang, *General Stilwell in China, 1942–1944: The Full Story;* Roderick Mac-

Farquhar, ed., *Sino-American Relations, 1949–1971;* and Kenneth E. Shewmaker, *Americans and Chinese Communists, 1927–1945: A Persuading Encounter.* 1853
Who's Who in American Politics: 1973–1974, 4th ed. Paul A. Theis and Edmund L. Henshaw, Jr., eds. Reviewed by Charles O. Jones. 1339
Who's Who in Government, 1972–1973, 1st ed. Reviewed by Thomas E. Cronin. 290
Whyte, John H. Review of Paul C. Bartholomew, *The Irish Judiciary;* Al Cohan, *The Irish Political Elite;* Brian Farrell, *Chairman or Chief? The Role of Taoiseach in Irish Government,* and *The Founding of Dáil Éireann: Parliament and Nation-Building;* and Maurice Manning, *Irish Political Parties: An Introduction.* 803
Why Vote? William C. Mitchell. Reviewed by Max M. Kampelman. 784
Wiarda, Howard J. Review of Jesús de Galíndez, *The Era of Trujillo: Dominican Dictator.* 818
Review of Carlos María Gutiérrez, *The Dominican Republic: Rebellion and Repression.* . 1809
The Widening Gap: Development in the 1970s. Barbara Ward, J. D. Runnalls, and Lenore D'Anjou, eds. Reviewed by Walter Buhr. ... 367
Wiecek, William M. *The Guarantee Clause of the U.S. Constitution.* Reviewed by Robert J. Morgan. 1344
Wildavsky, Aaron and Jeffrey L. Pressman. *Implementation: How Great Expectations in Washington are Dashed in Oakland; Or, Why It's Amazing that Federal Programs Work At All, This Being a Saga of the Economic Development Administration as Told by Two Sympathetic Observers Who Seek to Build Morals on a Foundation of Ruined Hopes.* Reviewed by Martha Derthick. 1336
Wilhoit, Francis M. Review of H. Paul Friesema, *Metropolitan Political Structure: Intergovernmental Relations and Political Integration in the Quad-Cities.* 1765
Willmoore Kendall Contra Mundum. Nellie D. Kendall, ed. With an introduction by Jeffrey Hart. Reviewed by Joseph C. Mouledoux. .. 1738
Willrich, Mason and Bennett Boskey, eds. *Nuclear Proliferation: Prospects for Control.* Reviewed by Robert L. Rothstein. 336
Wilson, Clifton E., Irvin L. White, and John A. Vosburgh. *Law and Politics in Outer Space: A Bibliography.* Reviewed by Robert H. Puckett. 370
Wilson, James Q. and Edward C. Banfield. *Communication* in reply to criticism of their article "Political Ethos Revisited" (December, 1971). 1271
Winkler, Heinrich August. *Mittelstand, Demokratie und Nationalsozialismus: Die politische Entwicklung von Handwerk und Kleinhandel in der Weimarer Republik.* Reviewed by Walter Struve. 851
Winterhager, Dietrich, Dirk Hartung, and Reinhard Nuthmann. *Politologen im Beruf. Zur Aufnahme und Durchsetzung neuer Qualifikationen im Beschäftigungssystem.* Reviewed by John Dreijmanis. 304
Wirt, Frederick M., Benjamin Walter, Francine Rabinovitz, and Deborah Hensler. *On the City's Rim: Politics and Policy in Suburbia.* Reviewed by John H. Baker. 291
Wittgenstein and Justice: On the Significance of Ludwig Wittgenstein for Social and Political Thought. Hanna Fenichel Pitkin. Reviewed by Stephen E. Toulmin. 769
Woddis, Jack. *New Theories of Revolution.* Reviewed by Bernard S. Morris. 1751
Wolanin, Thomas R. Review of Hadley Arkes, *Bureaucracy, the Marshall Plan, and the National Interest.* 1848
Wolfe, J. N. and John Erickson, eds. *The Armed Services and Society.* Reviewed by William F. Levantrosser. 1758
Wolfgang, M. E. and J. F. Short, Jr., eds. *Collective Violence.* Reviewed by N. Lucas. 1839
Wolinetz, Steven B. *Communication* on his review of S. J. R. Noel, *Politics in Newfoundland* (September, 1973). 731
Wood, Ellen Meiksins. *Mind and Politics.* Reviewed by Vincent E. Starzinger. 1321
Wood, Robert C. *The Necessary Majority.* Reviewed by Max M. Kampelman. 784
Wootton, Graham. Review of Stephen Blank, *Industry and Government in Britain: The Federation of British Industries in Politics, 1945–65.* 1794
Review of Wolfgang Abendroth, *A Short History of the European Working Class.* Nicholas Jacobs, Brian Trench, and Joris de Bres, translators. 797
Working Papers on Canadian Politics. John Meisel. Reviewed by Peter Regenstreif. 1369
A World Elsewhere: The New American Foreign Policy. James Chace. Reviewed by Hilliard A. Gardiner. 1387
World Handbook of Political and Social Indicators, 2nd ed. Charles Lewis Taylor, Michael C. Hudson, and others. Reviewed by Ted Robert Gurr in *The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.* 243
World Society. John Burton. Reviewed by Hedley Bull 857
World War II: Books in English, 1945–65. Janet Ziegler, comp. Reviewed by Barton Whaley. . 1884
Wright, Deil S. Review of Wallace E. Oates, *Fiscal Federalism.* 1777
Wright, Edward Reynolds. *Barriers to Progress in South Vietnam: The United States Experience.* Reviewed by William W. Boyer. .. 1411

The Writing of History in the Soviet Union. Anatole G. Mazour. Reviewed by George Enteen. 319
Wurfel, David, ed. *Meiji Japan's Centennial: Aspects of Political Thought and Action.* Reviewed by F. G. Notehelfer. 852
Wyman, Roger E. *Middle-Class Voters and Progressive Reform: The Conflict of Class and Culture.* 488
Review of Ronald P. Formisano, *The Birth of Mass Political Parties: Michigan, 1827–1861.* 782

The Year Book of World Affairs, 1972. George W. Keeton and Georg Schwarzenberger, eds. Reviewed by Patrick J. McGowan. 352
Youngblood, Robert L. Review of Ralph Smith, *Vietnam and the West.* 1375
Young, Donald. *American Roulette: The History and Dilemma of the Vice Presidency,* rev. ed. Reviewed by Robert S. Ross. 796
Young, James S. and Perry M. Goldman, eds. *U.S. Congressional Directories, 1798–1840.* Reviewed by Nelson W. Polsby. 1328
Young Men Look at Military Service: A Preliminary Report. Jerome Johnston and Jerald G. Bachman. Reviewed by James M. Gerhardt. . 1768
Youth Looks at National Problems: A Special Report from the Youth in Transition Project. Jerald G. Bachman and Elizabeth Van Duinen. Reviewed by Jarol B. Manheim. ... 1296
Youth in Politics: Expectations and Realities. Sidney Hyman. Reviewed by Everett Carll Ladd, Jr. 281
Youth in Transition, Vols. I, II, and V. Jerald G. Bachman et al. Reviewed by Jarol B. Manheim. 1296

Zabih, Sepehr. Review of James Alban Bill, *The Politics of Iran: Groups, Classes and Modernization.* 1793
Zald, Mayer N. and Gary L. Wamsley. *The Political Economy of Public Organizations: A Critique and Approach to the Study of Public Administration.* Reviewed by Charles H. Levine. 795
Zariski, Raphael. Review of Guy Hermet, *La Politique dans L'Espagne Franquiste.* 822
Zartman, I. William. Review of John Waterbury, *North for the Trade: The Life and Times of a Berber Merchant.* 850
Zeldin, Theodore. *France, 1848–1945. Volume I: Ambition, Love and Politics.* Reviewed by Eugen Weber. 1847
Zelnik, Reginald E. *Labor and Society in Tsarist Russia: The Factory Workers of St. Petersburg, 1855–1870.* Reviewed by Richard Pipes. 333
Zemelman Merino, Hugo and James Petras. *Peasants in Revolt: A Chilean Case Study, 1965–1971.* Reviewed by Alan Angell. 1832
Zentner, Joseph L. Review of Bruce D. Johnson, *Marihuana Users and Drug Subcultures;* and National Commission on Marihuana and Drug Abuse, *Drug Use in America: Problem in Perspective.* 1329
Zhukov, Y., L. Delyusin, A. Isenderov, and L. Stepanov. *The Third World: Problems and Prospects.* Reviewed by Edward Taborsky. .. 889
Ziegler, Janet, comp. *World War II: Books in English, 1945–65.* Reviewed by Barton Whaley. 1884
Zinnes, Dina A. Review of Thomas F. Carney, *Content Analysis: A Technique for Systematic Inference from Communications.* 750
Ziring, Lawrence, Review of Rounaq Jahan, *Pakistan: Failure in National Integration.* ... 311
Zonis, Marvin. *The Political Elite of Iran.* Reviewed by Marvin G. Weinbaum. 334
Zonis, Marvin. Review of Nikki R. Keddie, *Sayyid Jamal ad-Din "al-Afghani": A Political Biography.* 1310
Zurcher, Arnold J. and Edward C. Smith. *Dictionary of American Politics,* 2nd ed. Reviewed by Clement E. Vose in *Political Dictionaries: A Bibliographical Essay.* 1696

The American Political Science Review

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VOL. LXVIII 1974

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CONTENTS

NUMBER 1–MARCH, 1974

Thucydides' View of Athenian Imperialism. *Christopher Bruell* 11
Politics and the Career Mobility of Bureaucrats. *Eugene B. McGregor, Jr.* 18
Changing the Rules Changes the Game: Party Reform and the 1972 California Delegation to the Democratic National Convention. *William Cavala* 27
Comment. *Austin Ranney* 43
Social Mobilization and the Russification of Soviet Nationalities. *Brian Silver* 45
The Divisive Primary Revisited: Party Activists in Iowa. *Donald Bruce Johnson and James R. Gibson* ... 67
Virtue, Obligation and Politics. *Stephen G. Salkever* 78
Generational Change in American Electoral Behavior. *Paul R. Abramson* 93
Membership Stability in Three State Legislatures: 1893–1969. *David Ray* 106
The 3/2's Rule in Presidential Campaigning. *Steven J. Brams and Morton D. Davis* 113
Election Goals and Strategies: Equivalent and Nonequivalent Candidate Objectives. *Peter H. Aranson, Melvin J. Hinich and Peter C. Ordeshook* 135
Helvétius and the Roots of the "Closed" Society. *Blair Campbell* 153
Political Parties and the Porkbarrel: Party Conflict and Cooperation in House Public Works Committee Decision Making. *James T. Murphy* 169
APSA Presidential Address 187
Introduction. *Lucian W. Pye* 188
Culture and the Comparative Study of Politics, or The Constipated Dialectic. *Robert E. Ward* 190
Communications 202
Editorial Comment 214
Book Reviews 217
The Study of International Politics Makes Strange Bedfellows: Theories of the Radical Right and the Radical Left. *Ole R. Holsti* 217
The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science. *Ted Robert Gurr* .. 243

NUMBER 2–JUNE, 1974

Deferential-Participant Politics: The Early Republic's Political Culture, 1789–1840. *Ronald P. Formisano* . 473
Middle-Class Voters and Progressive Reform: The Conflict of Class and Culture. *Roger E. Wyman* 488
On the Size of Winning Coalitions. *Kenneth A. Shepsle* 505
Comment. *Robert Lyle Butterworth* 519
Rejoinder. *Kenneth A. Shepsle* 522
The Paradox of Not Voting: A Decision Theoretic Analysis, *John A. Ferejohn and Morris P. Fiorina* .. 525
Paradoxes of Voting. *Peter C. Fishburn* 537
Transcending Tragedy: The Idea of Civility. *Glenn Tinder* 547
The Social Origins of Liberal Democracy: The Swedish Case. *Timothy A. Tilton* 561
The Simple Act of Voting. *Stanley Kelley, Jr. and Thad W. Mirer* 572
Racial Differences in Protest Participation. *Peter K. Eisinger* 592
The Friendship Group as a Protective Environment for Political Deviants. *Ada W. Finifter* 607
Nonattitudes and American Public Opinion: The Examination of a Thesis. *John C. Pierce and Douglas D. Rose* 626
Comment. *Philip E. Converse* 650
Rejoinder. *John C. Pierce and Douglas D. Rose* 661
Mobilization of Liberal Strength in the House, 1955–1970: The Democratic Study Group. *Arthur G. Stevens, Jr., Arthur H. Miller and Thomas E. Mann* 667
The Measurement of Party Competition. *David J. Elkins* 682
Toward a Model of Soviet Decision Making: A Research Note. *Donald R. Kelley* 701
Provision of Collective Goods as a Function of Group Size. *John Chamberlin* 707
Communications 717
Editorial Comment 733
Book Reviews and Essays 736
Diplomacy for the 70's: An Afterview and Appraisal. *William I. Bacchus* 736

NUMBER 3–SEPTEMBER, 1974

Political Issues and Trust in Government: 1964–1970. *Arthur H. Miller* 951
Comment. *Jack Citrin* 973
Rejoinder. *Arthur H. Miller* 989
Theory and Voting Research: Some Reflections on Converse's "Change in the American Electorate" *Walter Dean Burnham* 1002
Comment. *Philip E. Converse* 1024
Comment. *Jerrold G. Rusk* 1028
Rejoinder. *Walter Dean Burnham* 1050
Social Change and Political Mobilization in the United States: 1870–1960. *Jesse F. Marquette* 1058
Institutionalization, the Ombudsman, and Bureaucracy. *Larry B. Hill* 1075
Hegel's Critique of Liberal Theories of Rights. *Peter G. Stillman* 1086
Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis. *Lawrence C. Dodd* 1093
The Mediating Influence of State Legislatures on the Linkage Between Interparty Competition and Welfare Policies. *Edward G. Carmines* 1118
Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities. *Wayne A. Cornelius* 1125
Clientelist Politics in the Philippines. Integration or Instability? *Thomas C. Nowak and Kay A. Snyder* 1147
An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems. *Kenneth Jowitt* 1171
Foreign Policy and Empirical Democratic Theory. *David W. Moore* 1192
The ABM Issue in the Senate, 1968–1970: The Importance of Ideology. *Robert A. Bernstein and William W. Anthony* 1198
Need for Achievement and Competitiveness as Determinants of Political Party Success in Elections and Coalitions. *Bruce Bueno de Mesquita* 1207
Bases of Budgetary Incrementalism. *John Wanat* 1221
Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior. *Paul Allen Beck* 1229
Conflict in the Community: A Theory of the Effects of Community Size. *Gordon S. Black* 1245
Communications 1262
Editorial Comment 1281
Book Reviews and Essays 1285
Chemical and Biological Warfare. *George H. Quester* 1285
Tradition and Nationality: A Classic Revisited. *Samuel Beer* 1292

NUMBER 4–DECEMBER, 1974

Editorial Comment 1478
Persistence and Change in Political Systems, 1800–1971. *Ted Robert Gurr* 1482
On the Origins of Interest-Group Theory: A Critique of a Process. *G. David Garson* 1505
Ethnic Voting and Political Change in South Africa. *Stanton Peele and Stanley J. Morse* 1520
Hobbes and His Audience: The Dynamics of Theorizing. *Mark Gavre* 1542
Separate and Unequal: Governmental Inequality in the Metropolis. *Richard Child Hill* 1557
Conflict, Power and Relative Deprivation. *Walter Korpi* 1569
The Influence of Parents on the Political Attitudes of Adolescents. *Kent L. Tedin* 1579
Majority Party Leadership and the Committee System in the House of Representatives. *Louis P. Westefield* 1593
Ethnic Politics, Representative Bureaucracy and Development Administration: The Zambian Case. *Dennis L. Dresang* 1605
Levels of Sociocultural Development Among Soviet Nationalities: A Partial Test of the Equalization Hypothesis. *Brian Silver* 1618
Models of Statistical Relationship. *Herbert F. Weisberg* 1638
Individual and Contextual Variables in British Voting Behavior: An Exploratory Note. *C. Neal Tate* 1656
Community and Control: Critical Democratic Theory in the Progressive Period. *David E. Price* 1663
Communications 1679
Book Reviews and Essays
Political Dictionaries: A Bibliographical Essay. *Clement E. Vose* 1696
Political Implications of Gender Roles: A Review of the Literature. *Wilma Rule Krauss* 1706
Index to Volume LXVIII 1885

The American Political Science Association

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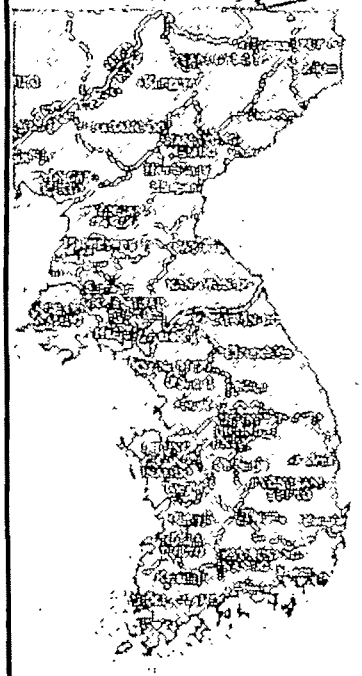
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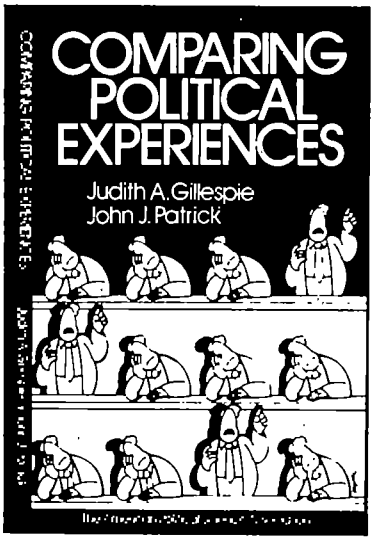
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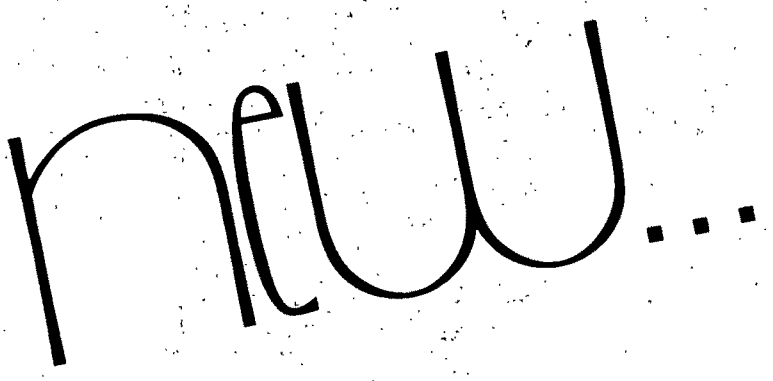
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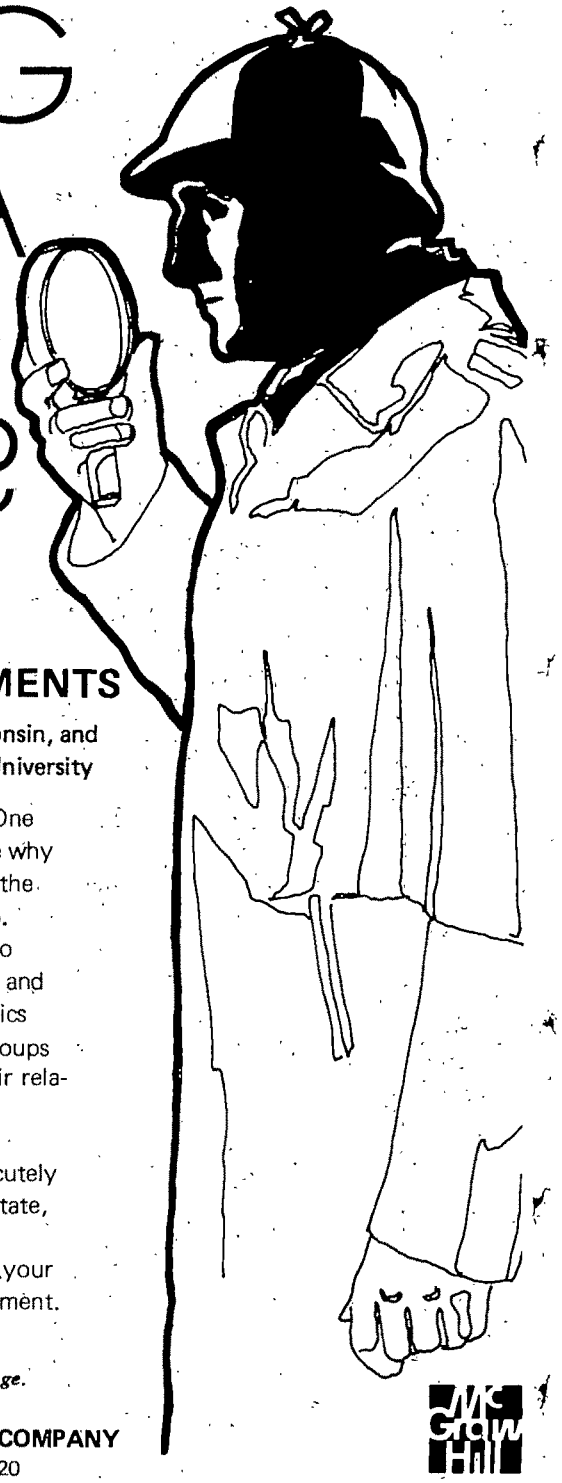
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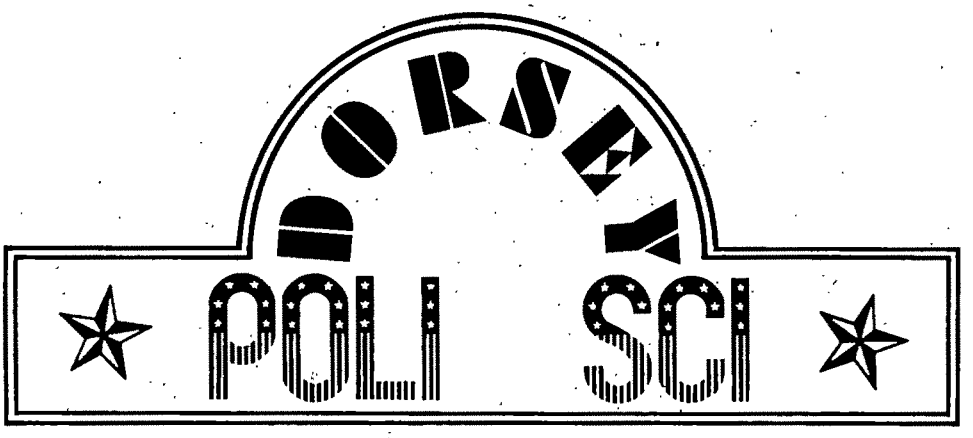
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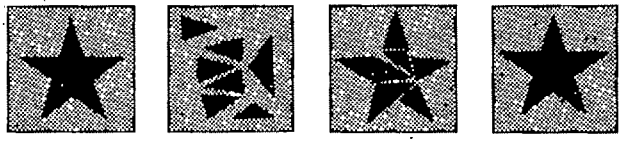
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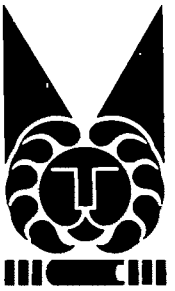
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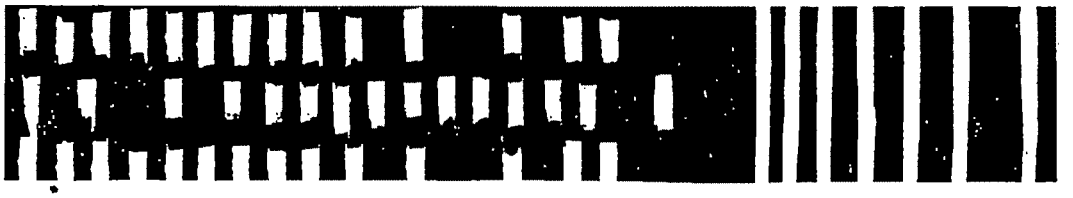
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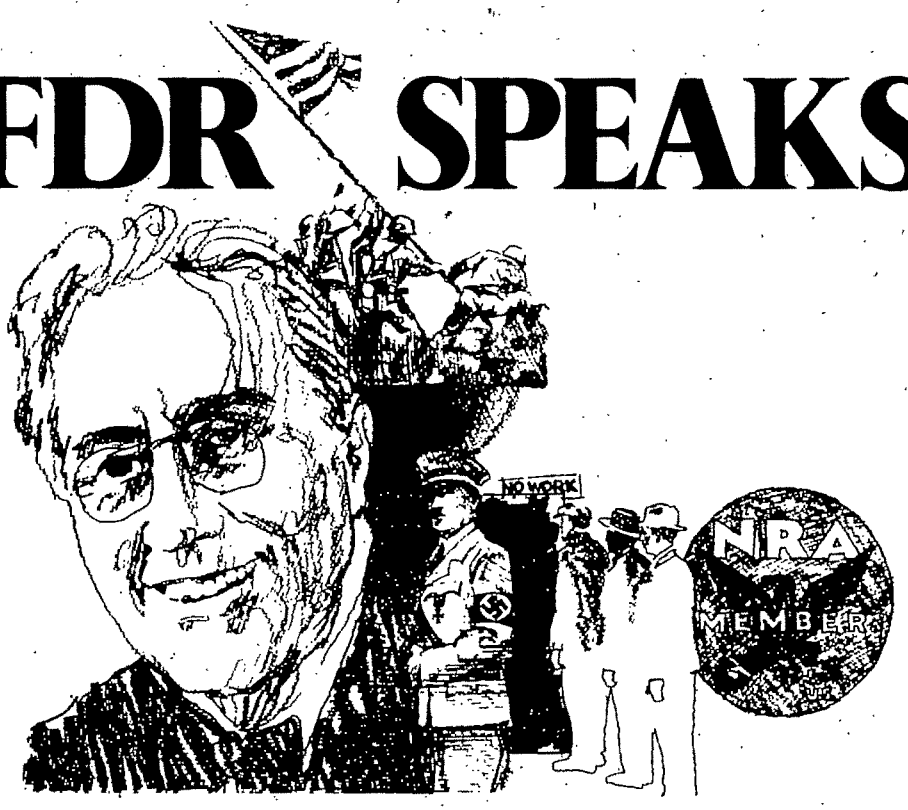
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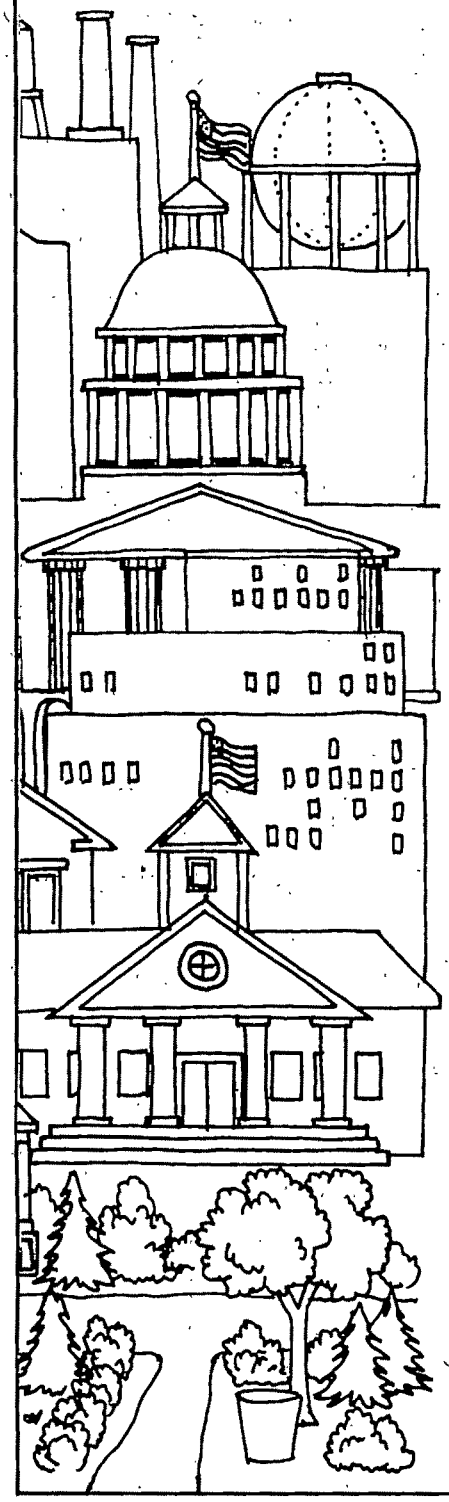
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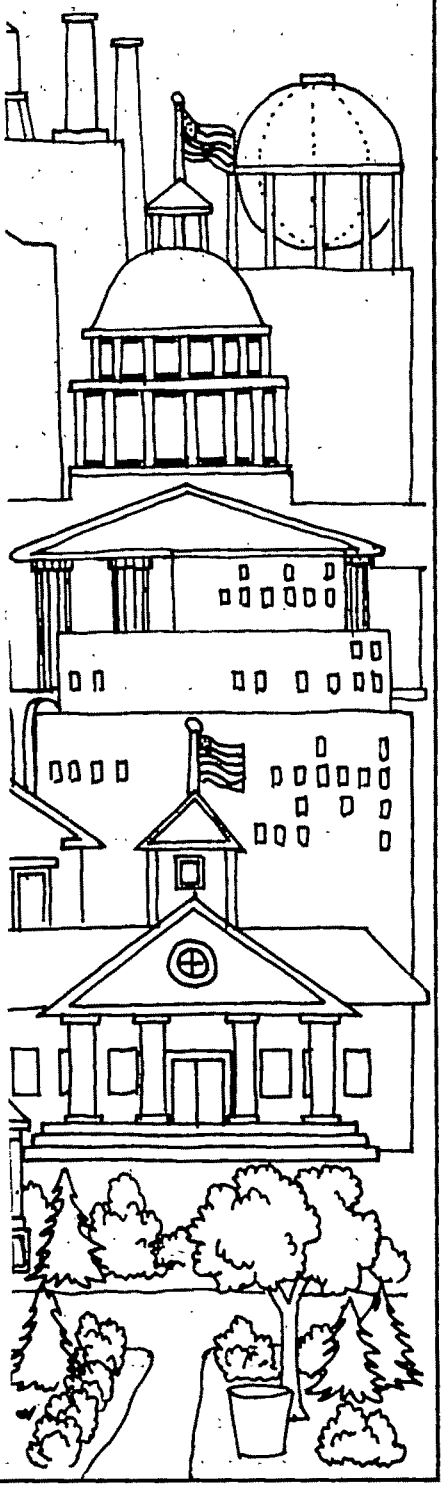
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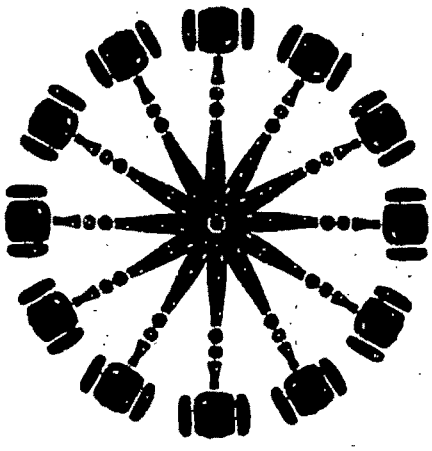
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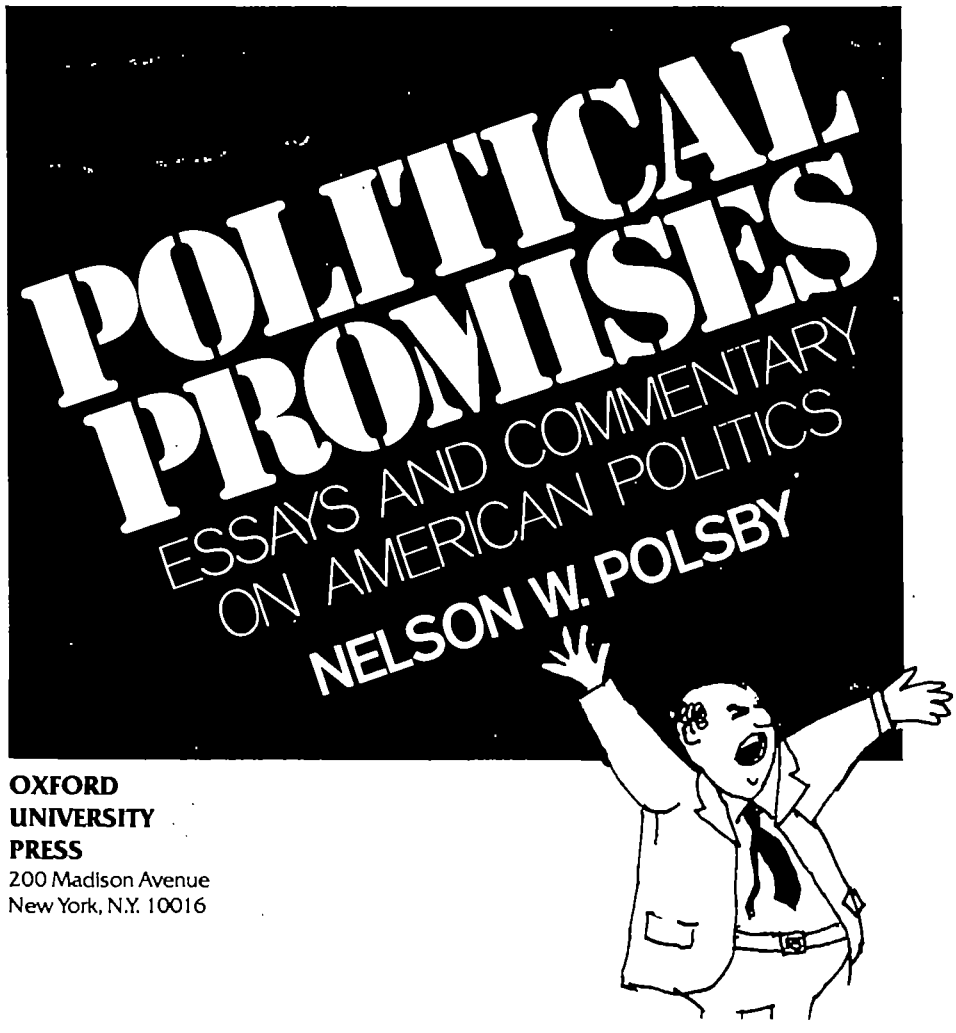
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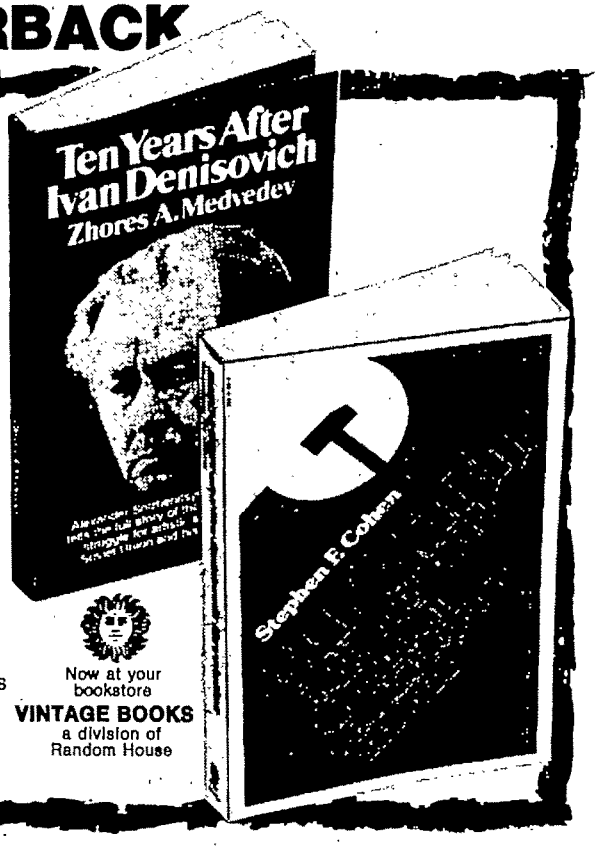
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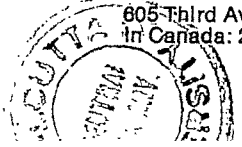
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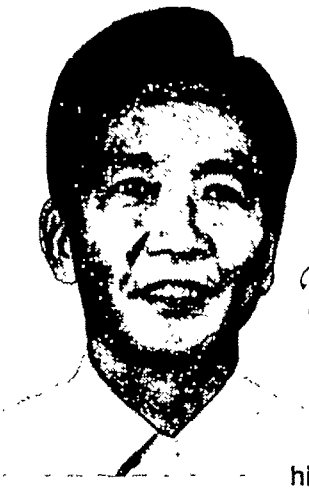
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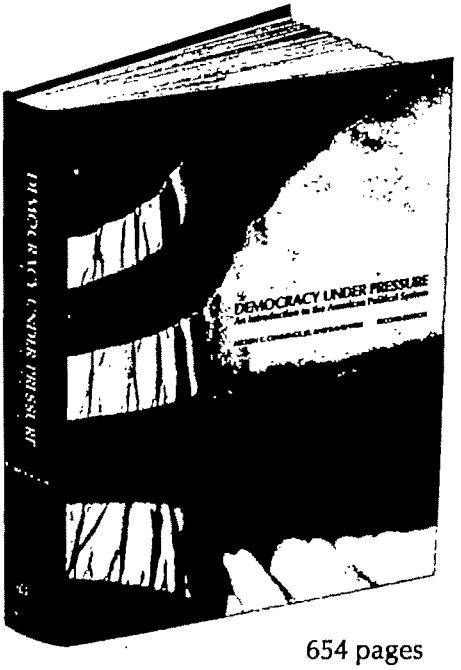
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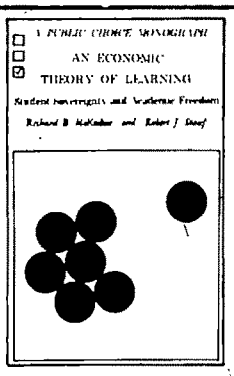
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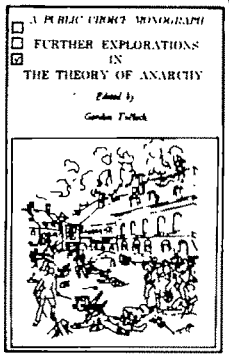
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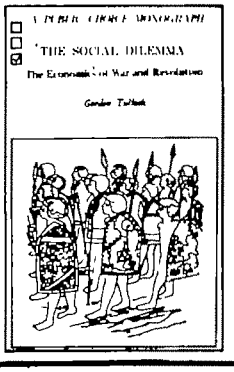


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